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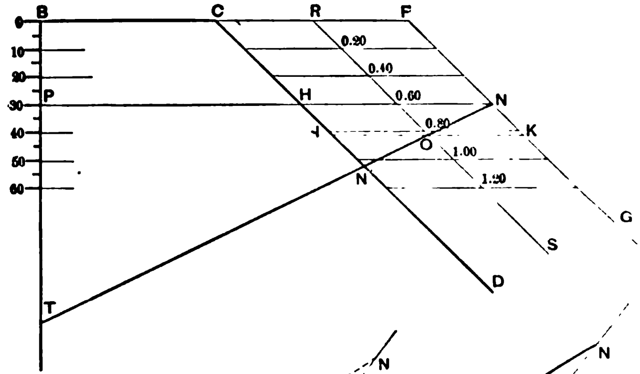


Fig.5

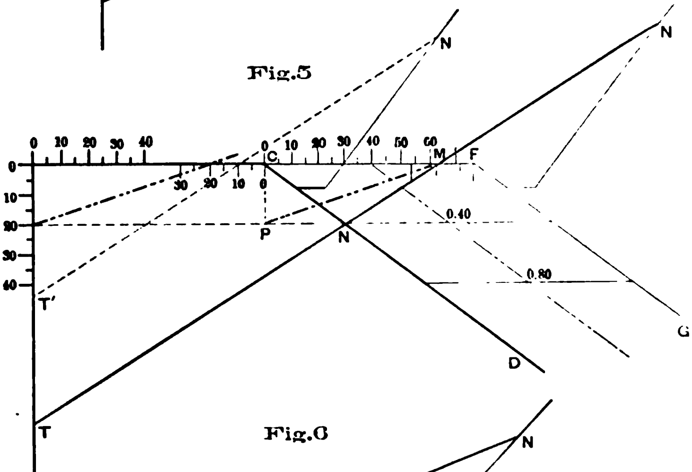


Fig.6

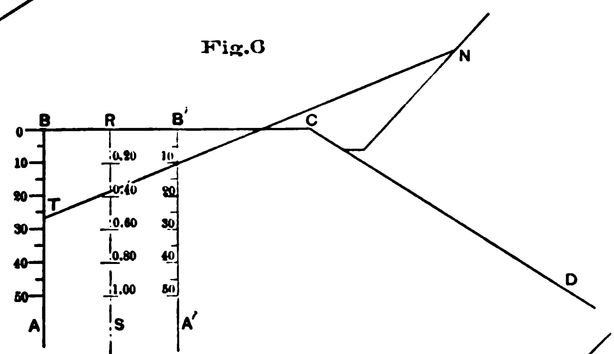


Fig.7

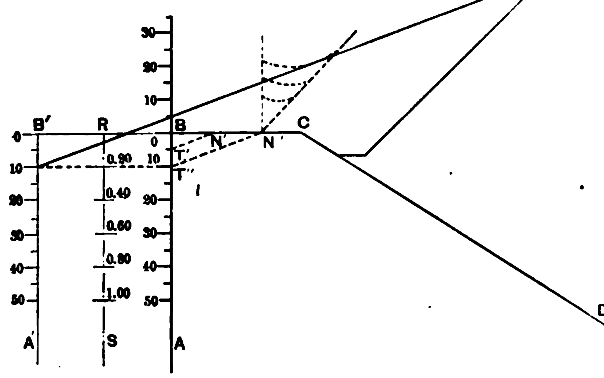
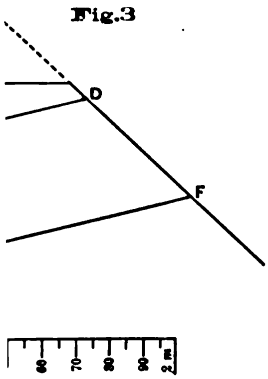


Fig.3



VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXIX.—JANUARY, 1883.—VOL. XXVIII.

GRAPHIC METHOD ON DETERMINING AREAS OF RAILWAY CROSS-SECTIONS.

By M. DUBRET.

Translated from Annales des Ponts et Chaussées for VAN NOSTRAND'S MAGAZINE.

THE method of M. Villotte, of which the present article is an extension, was originally published in *Annales des Ponts et Chaussées*, in October, 1880. [See VAN NOSTRAND'S MAGAZINE for February, 1881. As M. Dubret's article seems to require the former one for complete elucidation, a portion of the original translation is herewith given.] He proposed to solve the problem of constructing the profile of a cross-section and estimating its area by the following simple method:

"It is clear that nothing more is necessary than to draw the profile of the original surface. This may be done in a ready manner by the aid of a prepared scale, graduated to the scale of trigonometrical tangents, as shown in Fig. 2.

"This little instrument is then applied to the axis of the cross-section diagram at the cut or fill height, and inclined at the angle of the natural surface.

"It only remains to follow with a pencil the edge of the protractor, in order to complete the half profile TNMP.

"The length of the slope and the breadth of base are read at once from the inclined scale drawn on the prepared card.

"It is required finally to determine the area TNMP. This is done in the following manner, which is only an application of a more general method for the measurement of areas.

"If we consider some point A arbitrarily chosen on the movable scale, but having a position mathematically defined, it follows that for each position of A, a corresponding point on the engraved sheet may be determined.

"We can, therefore, trace beforehand upon the prepared sheet a series of curves, giving the area of the half cross-sections for each position of the point A, and the problem is then solved by a simple reading.

"For reasons which will be immediately apparent, the point A is taken at A, the point of tangency of TN (original surface line), and the curve to which all the lines are tangent, which cut off areas equal to the area TNMP.

"To determine the nature of this curve we seek for the position of the point of tangency A, under the above conditions. This point will be at the intersection of TN and another line T′N′, drawn infinitely near it in such way that the areas PMNT and PMN′T′ are equivalent.

"The two infinitely small triangles TA_1T and NA_1N are equivalent. Hence the point A_1 is at the center of the line TN.*

* TN is throughout the original surface

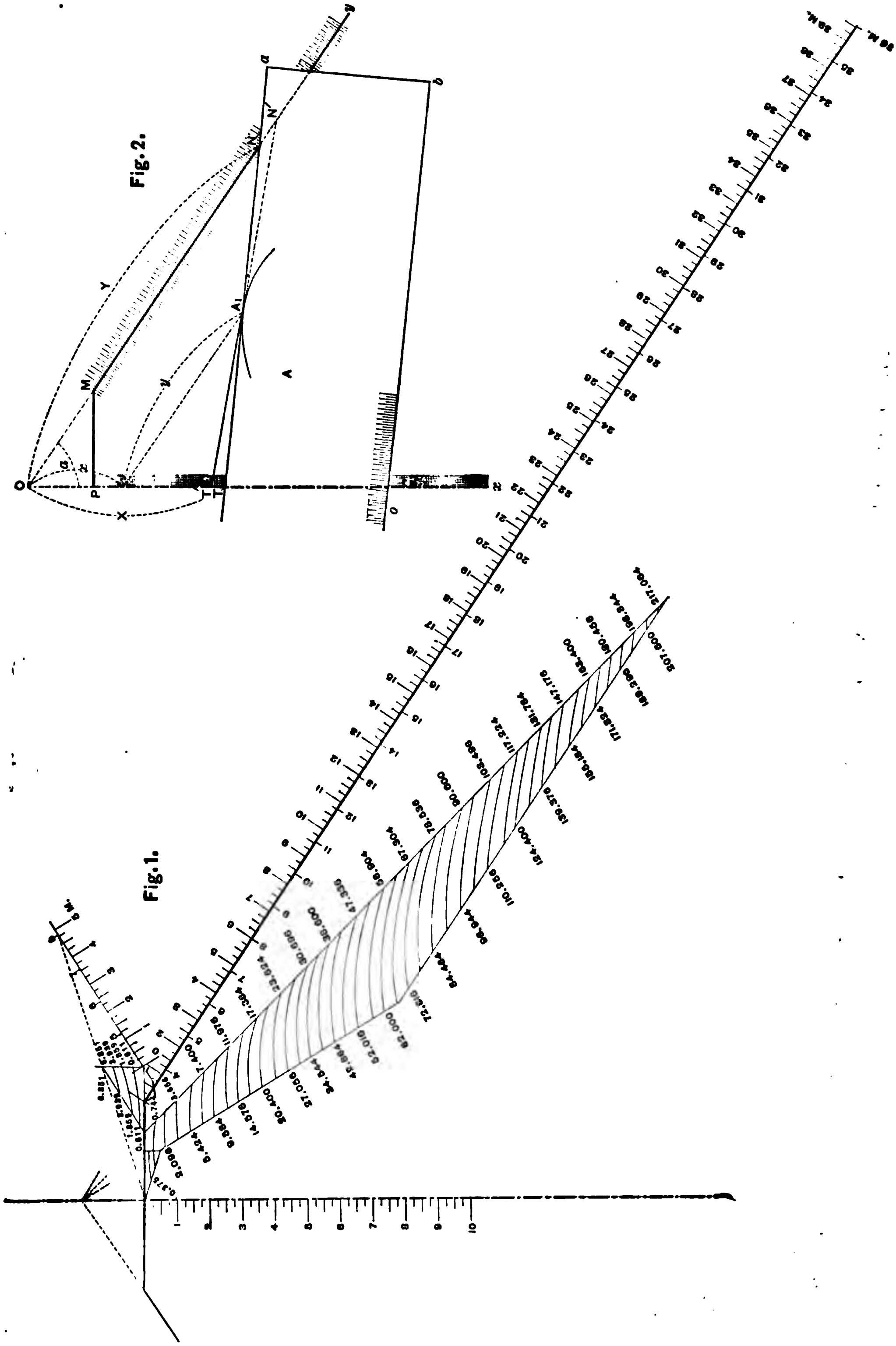


Fig. 2.

Fig. 1.

"To determine the equation of the curve of the locus of A_1 under the prescribed conditions. Let Ox and Oy be co-ordinate axes, and let $OT=X$ and $ON=Y$. The area PMNT being constant, the area ONT is also constant (so long as the line NT does not intersect the line PM within the angle xoy, a condition which is taken into account further on).

"Then we have

$$\text{area} \quad OTN = \frac{ON \,.\, OT \,.\, \sin\alpha}{2} = \frac{XY \sin \alpha}{2}$$

$$\text{or} \quad XY = 4K^2 \text{ a constant.}$$

"As x and y are the co-ordinates of A_1 we have

$$x = \frac{X}{2} \text{ and } y = \frac{Y}{2}$$

$$\text{and} \quad xy = \frac{XY}{4} = K^2.$$

"The locus of A_1 is, therefore, a hyperbola having for asymptotes the axes Ox and Oy.

"For different values of K^2 we may obtain a series of hyperbolas corresponding to different values of the half section PMNT. These curves may be termed *curves of equal areas.*

"The advantages arising from the selection of the point A_1 under the assigned conditions, are obvious.

"1st. The curves of equal surfaces being hyperbolas drawn relative to the same point O, and to the same asymptotes, are easily traced. Any one is easily constructed by means of some one of its tangents, and others of the series are readily derived from this.

"The preparation of the table of areas is not at all complicated. An area is calculated corresponding to one of the curves. Other areas are determined by the law of the dimensions of similar surfaces, and the numbers may be written upon the curves.

"If the curves are traced in such a mrnner that they cut equal segments on one of the radii vectores from the point O, the *second differences* of these numbers are equal.

"2nd. For each of the different methods of choosing the point, there is a graphic table of determinate form; and for each table there exists a system of equal surface curves generated by the different positions of A corresponding to equal areas. The curves are generally of a degree above the second, but as we have already seen they become hyperbolas of easy construction when we take for auxiliaries points analogous to A_1.

"But the simplicity of construction is not the only advantage arising from this selection; for it is clear that it is an advantage to diminish as much as possible the length of the curves of equal surface; in taking A_1 the minimum of length is secured.

"Fig. 1 represents cross sections, comprised between the following limits:

"1st. Inclinations of surface varying from -0.33 to $+0.33$.

"2d. Depths or *fillings* at the center, of 10 meters.

"The prepared sheet would not be inconveniently large if both these limits were extended. A single sheet might serve for cross sections of different slopes.

"The mode of proceeding to employ the system is as follows:

"1st. Make a book of the cross sections, also a sufficient number of the proposed sheets to serve the purposes of the work in hand.

"2d. Write within the profile of each cross section, the center cut or fill and the slope of the original surface (expressed as a ratio or tangent of the angle made with the horison).

"3d. Proceed to trace the lines TN by aid of the movable scale (the inclination being measured by aid of graduations on the lower edge as at O in Fig. 2).

"4th. Read the numbers upon the curves to which in each case the line falls tangent, and write these areas within the respective cross sections.

"It may be remarked here that the tangency of the line TN is easy to determine, and moreover that an error of the first order, in determining the point of tangency, results in an error of the second order only in getting the areas.

"5th. Complete the calculations indicated for estimating the volumes.

"As the accuracy of the work depends so largely upon the accuracy of the prepared sheets, it will suggest itself that a shrinkage of the sheet would result in errors in the work. This does not necessarily follow except in cases obviously rare of unequal shrinkage in different directions.

"In place of the movable scale for the measurement of inclinations, a common straight-edge ruler may be employed and the inclinations be obtained, by using the method of drawing parallels from a set of prepared lines like Fig. 3, drawn to the different inclinations on each of the working sheets."

II.

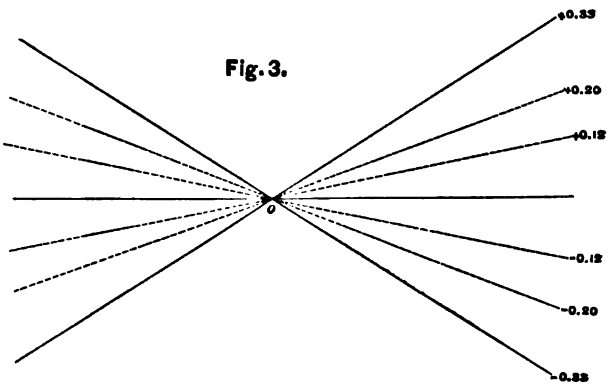
A simplification of the foregoing method is here suggested.

M. Villotte concludes the exposition of his method with some considerations relative to the shrinkage of the prepared sheets, which would tend to result in inaccuracies in the diagrams.

The instrument which is here proposed is designed to render the line of natural surface (the line T N of Villottes diagram) unnecessary. It is nothing more than a transparent protractor, in the form of a trapezoidal frame of wood or card-board, over the open part of which is stretched cloth or vellum, on which has been previously traced the different inclinations of the natural surface. It is necessary to fasten this vellum securely to the inner edge of the frame so that the horizontal line of the figure shall be accurately perpendicular to the basis of the trapezoid. Finally the graduations are made on a vertical line along the longer base.

This frame is made to slide against a

Fig. 3.



It is admitted that, notwithstanding such shrinkage, if the straight lines continue straight and the parallel lines parallel, the process would still be correct if the use of the protractor be abandoned and a sort of mariner's card be substituted on the corner of every sheet, having radial lines drawn in the direction of the different surface slopes. (See Fig. 3.)

It would be still better, however, to rely upon a single sheet, or two at most, one for excavations and the other for embankments. If drawn upon stout card-board they would not be liable to become faulty through shrinkage. They might be made upon a large scale so that the number of hyperbolas might be increased, and a greater accuracy of results obtained.

It is true no trace of successive problems would be preserved, but it would always be easy to verify a doubtful result, and thus the principal object of the work would be obtained.

ruler fixed to the left hand side of the table or board to which the diagram is firmly fixed, and is so adjusted that the graduated vertical shall constantly coincide with the axis of the diagram. When a line representing the natural surface of a cross section falls into the proper relation to the curves of the prepared diagram, it serves the purpose of the line TN of M. Villotte's system and the reading is at once made.

Without making the instrument of inconvenient size, all the inclinations from +0.30 to −0.30 from millimeter to millimeter, can be drawn. But experience shows that it is not necessary to draw them all. Alternate lines may be omitted without impairing the usefulness of the instrument.

Fig. 2 (plate) is a partial reproduction of a diagram, representing a road bed of 4.^{m}55 width. The divisions along the axis are in centimeters. The curves correspond-

ing to the heights are multiples of 5 centimeters; and the inclinations are between +0.50 and −0.50. The spacing is sufficient to permit the eye to follow the line to the extremity, but to avoid confusion the alternate lines are made heavier. On one side are numbers showing the areas, on the other are numbers written in red ink forming a kind of interpolation table, as each of them is $\frac{1}{5}$ of the difference of the two numbers against which it is placed. This facilitates the estimate of the value of a space when the surface line falls between two curves without being quite tangent to either. In such a case, if the position of the line be estimated by the eye to the nearest fifth of the space, then the *difference* referred to multiplied at most by 2 gives the correction to be applied to the nearest tabular number.

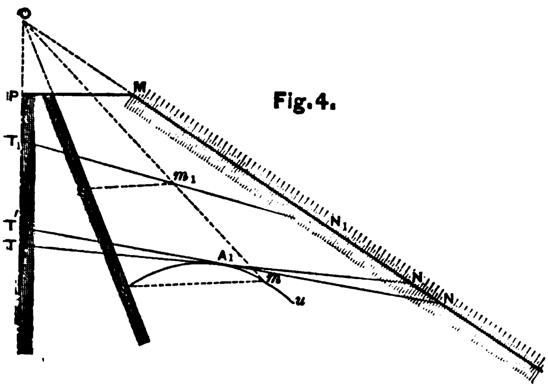


Fig. 4.

The diagram is drawn to working size upon a sheet of bristol-board $0^m.53 \times 0^m.48$. It does not admit of numbers along the axes higher than six meters, for with a road-bed of $4^m.55$ width, it is not indispensable to provide curves corresponding to greater heights, as by means of the following formulas deduced by M. Villotte in his essay, they suffice to supplement the diagrams so that excavations not exceeding 15 meters or embankments of 13.5 meters are provided for.

The following are the formulas:

If we let S_c represent the area of a half section of which the number on the axis is c; by l the half width of the road-bed and by t the slope, we have by taking AB $=\frac{AC}{2}=\frac{lt+c}{2}$ and supposing BD parallel to CF (Fig. 3, plate).

$$OB_{=\frac{c-lt}{2}}$$

and
$$\frac{S_{c+\frac{l^2t}{2}}}{S_{\frac{c-lt}{2}}+\frac{l^2t}{2}}=\frac{(c+lt)^2}{\frac{(c+lt)^2}{4}}=4.$$

From this we get:

$$4S_{\frac{c-lt}{2}}+\frac{3l^2t}{2}$$

which gives, in case of a road-bed $4^m.55$ wide, not including a ditch of $0^m.75$, for the embankment,

$$S_c = 4S_{\frac{c-lt}{2}}+5^m.18,$$

for the excavation,

$$S_c = 4S_{\frac{c-lt}{2}}+13^m.35.$$

We find also for the length of the slope of the embankment,

$$T_c = 2T_{\frac{c-lt}{2}}+2.78.$$

of the cutting,

$$T_c = 2T_{\frac{c-lt}{2}}+3.08.$$

By the aid of the transparent protractor above described, the areas of fifty cross sections, with the length of slopes can be determined per hour. It is necessary to say that previous to this operation, the inclination of the natural surface for each cross section together with the cuttings and fillings have been properly recorded.

Furnished with these data an assistant determines upon the diagram the un-

known elements of each half-section and reads them aloud. Another writes them upon a sheet prepared in advance and adds them up.

The process, as is easily seen, is very expeditious. It is also in a high degree accurate; for the maximum error in estimating a surface is at most one-half of the amount expressed by one of the small difference numbers. That is to say, that for a diagram constructed to a scale of $\frac{1}{20}$, the area of a half profile measuring forty square meters, the error would at most not exceed $0^{m}.05$. and generally would be less than $0^{m}.04$.

III.

These diagrams of M. Villotte are susceptible of an easy generalization, aside from the question of mixed profiles.

It would suffice to prolong the homothetic hyperbolas to their center of similitude, to take this point as the origin of axial graduations, and for each change of width, to determine the value of the constant which should be added to each of the numbers along the axis before proceeding to the operations.

The problem of mixed profiles is evidently not easily solved in the same manner, that is to say, by the construction of hyperbolas to the two angles of the road-bed.

The means to be suggested do not constitute a generalization in the strict sense of the term, but they require only the drawing of one or two right lines and the establishing of a complementary graduation when we wish to pass from one section to the other, and even in ordinary cross sections, the method presents, as a generalization some advantages.

Let ABCD, Fig. 4 (plate) be a pattern diagram drawn for a road-bed whose width is L. And let TN be the line of natural surface of a half section of a width $L+l$, The area of this half section is to be obtained by adding to the surface given by the hyperbola tangent to TN, the area of the trapezoid CFNN′. Now let a horizontal line IK be drawn through the middle O of NN′. The two triangles ION′ and NOK are equal, and consequently the parallelogram CFIK is equivalent to the trapezoid CFNN′.

Now the locus of the point O being the parallel RS of the slopes of the two road-bed linesthrough the middle R of CF; if we divide the space between the slopes into sections of equal area by equidistant horizontal lines, so that the intersections of these horizontals and the line RS will establish a graduation analogous to that of the curves of the original diagram, we shall have constructed upon this line a scale of surfaces which will give the area of the trapezoid to be added to such half section.

The line FG although of no use in the determination of the area should be traced upon the diagram because it is that which determines the length of the slopes and the width the work. The scales constructed upon CD can be dispensed with, for it will be sufficient to follow the horizontal passing by N in order to read upon the scales the length FN=CH of the slope and of the width PH which is equal to the whole width diminished by the constant l.

The scale of surfaces mentioned above corresponds evidently with those of the half-sections which are either all cutting or all filling. They are of no use when the line TN intersects the road-bed between C and F (Fig. 5), and gives rise to a mixed cross section.

In such a case we can obtain the area of the excavation by a simple displacement of the protractor.

For the surface of the embankment; if we observe that the two triangles CNM and CPM, which have the same base and same altitude are equivalent, the method of determining this area is already indicated. It consists in placing the protractor in such a manner that one of the lines of slope intercepts upon the sides of the right angle formed by the axis of the diagram, and the road-bed two distances respectively equal to the height and base of the triangle CNM. It is understood, of course, that the first reading has given the area of the quadrilateral BCNT, the lengths CM, MF and the height CP, which the previous construction rendered necessary; the dimension of the road-bed; the new rules having for their origin the points B, C, F, and disposed as shown in Fig. 5.

Thus a scale of surfaces for an entire cross section, either of excavation or embankment—a scale of lengths for the mixed half cross sections, in which

the point of division falls at a distance from the axis greater than the half width of the primitive road-bed—such are the additions to be made to the tables for each change of diagram, upon the hypothesis that diagrams are to be prepared for the smallest details.

These additions only require a few minutes. The scales BC, CF and CB, and the equidistant horizontal lines, can be traced in advance once for all.

The new profiles are calculated to be narrower than the former by the reduction of the width of the road-bed line on the side of the axis (Fig. 6).

The complementary scale of the surfaces becomes vertical and divides the space between the parallels AB and A′B′ into two bands of equal width. There is no further need of metric scales along the road-bed, but a new scale of red line ought to be made upon the vertical A′B′ which represents the axis of the rectified diagram.

We remark, further, that in the first hypothesis we could much more easily enlarge the diagram on the side of the axis than on that of the slope, which would render equally unnecessary the scales on the road-bed; only it would be necessary then to reproduce above the latter a part of the hyperbolas prepared for the excavations. Figure 7 shows the new arrangement, also the proposed displacement of the protractor when in use on mixed cross sections.

The advantages which this method of generalization presents may be enumerated as follows:

1. Corrections of the figures along the axis are avoided.

2. There is no necessity for so large a number of hyperbolas.

3. Finally the first system affords no practical means of determining the area of cross sections—part excavation and part embankment. The latter system reduces the problem to a simple displacement of the protractor which is in use throughout.

WATER GAS.*

The important results which in the past decade have followed the introduction of generator gas [(a)] (see Appendix) as a heating agent, into many branches of industry, suggests the expectation that gaseous fuel may find more general application, and may be used on a small scale as well as in the larger operations which have heretofore received its benefits. For household uses a gaseous fuel, supplied through pipe, offers many advantages when we consider the difficulty of managing a fire using coal or wood, the labor of starting the fire, and especially when we remember that scarcely more than 10 per cent. of the total heating power of fuel is utilized as it is commonly burned, while more than 80 per cent of the heat of combustion is believed to be available when gaseous fuel is employed. A cheap heating gas would be of especial service to the smaller trades, because gas motors may be made to serve the ends of those trades demanding only small power, while the outlay for a steam engine presupposes the need of heavier work. That the advantages of heating by gas will be more and more appreciated in daily life is to be expected, when we consider the increasing application of illuminating gas to heating purposes, such as the warming of rooms, cooking, and especially for actuating gas motors for small applications of power.

A gas intended only for heating may be prepared at a cost considerably less than that of illuminating gas, since it may be made from the most inferior combustible material, while the choice of material for the manufacturing of illuminating gas is quite limited. The proposal of Siemans, made twenty years ago, to supply cities with heating as well as lighting gas through pipes, remains as yet unfulfilled[(b)] only because such pipes require a heavy outlay, since it is well known that distribution involves the heaviest expense of gas-making plant. Since the manufacturing of lighting gas, as such, is a necessity, while it serves very well also the purpose of a heating agent, and since, moreover, the importance of heating by gas has but recently been made apparent, it is questionable whether the manufa

* A paper by Prof. C v. Marx, read before the Engineering Society of Stuttgart. *From Repertorium der Analytischen Chemie, Nos.* 13, 14, 1882. Translated by Prof. A. A. Breneman, with an appendix by the translator.

ture of such gas would have been profitable heretofore. With the development of electric lighting, however, the *status* of heating gas must sooner or later be changed.

It is now known that more light is obtained when a given volume of illuminating gas is used in a gas engine which in turn acts through an electro-motor to produce the electric light, than when the same volume of gas is burned directly from a gas burner. In view of this fact it cannot be concealed that illuminating gas must soon lose its importance as a lighting agent, and the suggestion follows that illuminating gas works will in time be converted into manufactories of heating gas.

The relation between electric light and gas light is shown in the following table by Alfred Niaudet, in which the light, obtained by different observers using different forms of lamps, is expressed in carcels, the power exerted in each case being 75 kilogrammeters per second=1 horse power, nearly.

		Carcels.
1. Arc Light.	Carbons 10 c. m. apart, a maximum distance which cannot well be exceeded in practice. Common Gramme machine. (Fontaine.)	285
2. " "	Carbons 3 c. m. apart, common distance, Gramme machine. (Fontaine.)	230
3. " "	Number given by the President of the Committee on Lighting by Electricity; 2,400 candles; 9-6 candles = 1 carcel.	250
4. Jablochkoff candle.	Gramme machine with alternating current. One candle uses $\frac{5}{6}$ H. P. (Honoré), and gives a light of 41 carcels (Joubert), *i. e.* per H. P.	49.2
5. Edison's Incandescent lamp.	Number given by Rowland & Barker.	16-21
"	Number given by Bracket & Young.	19
6. Swan's Incandescent lamp.	150 candles, 9=1 carcel.	16.66

The average consumption of illuminating gas in a gas motor is one cubic meter per H.P. per hour.[c] On the other hand, an Argand burner using 150 liters per hour gives a light of 18 candles or 120 candles per cbm. At 9 candles per carcel only, this is 13.33 carcels, which is below most of the figures given above, notably so for arc lights. Incandescent lamps, however, are already in competition with gas. In the combustion of illuminating gas a large part, comparatively, of the chemical energy takes the form of heat, and less appears as light; but in the production of the electric light, the proportion of light to energy expended is much greater.

Since the electric light for the illumination of large areas with light of a given intensity is cheaper than gas light, it may be expected that the attention paid to the problem of electric lighting at present may result in making it also cheaper for lighting on a small scale. Illuminating gas will then have been crowded from the field, while heating gas will have a new claim to consideration.

If it be asked in what way a cheap heating gas is to be prepared, it is evident that it must be by a process different, at least, from that used in making illuminating gas, since by the process of dry distillation as now employed, only a small portion of the combustible matter is converted into gas. Gas-coal yields in this way about 28 cbm. of gas, weighing 14.4 kilos. for each 100 kilos. of coal leaving a residue of 66 kilos. of coke, of which fully 20 kilos. are used again to heat the retorts, and more than 40 remai unused in the process. A method whic could entirely convert the carbon of co into gas would be far preferable. It h been proposed, however, to obtain heatir gas by dry distillation of the brown coal the Fürstenwalde, 35 kilos. from Berli and to carry the same above ground that city in strong sheet-iron pipes, whe it would be stored in twelve large hold and distributed from them in pipes the consumers. A solid fuel may be cc pletely converted into gas by imperf combustion, as occurs in the generat of large furnaces where the carbon of fuel is changed to carbonic oxide oxygen from the air, and this combu ble gas is then burned to carbonic in the heating chamber of the furn Carbon, in burning to carbonic a

yields 8080 heat units in the aggregate, and of these 2473 are developed in the production of carbonic oxide, the first stage of the process. When commercial gas is made, *i. e.*, gas which is to be conveyed to the consumer in pipes, this latter component, 30.6 per cent. of the total heat, is lost by the cooling of the gas. Generator gas contains 70 per cent.[d] of nitrogen also, on an average, which takes no part in the combustion. Assuming the air to contain, in round numbers, 20 per cent. of oxygen and 80 per cent. of nitrogen, and remembering that one volume of oxygen in combustion yields two volumes of carbonic oxide, 100 volumes of air should yield a mixture of 80 of nitrogen and 40 of carbonic oxide, or $66\frac{2}{3}$ per cent. of nitrogen. This gas, of which two-thirds is worthless for heating purposes, cannot profitably be distributed through mains, and generator gas must therefore be impracticable as a commercial gas.

The preparation of water gas, however, presents a much more favorable aspect. This depends upon a well-known reaction, by which glowing coal is made to decompose water led through it in the form of steam. Any form of carbon will answer the purpose, wood-charcoal and coke yielding equally good results. Carbon appropriates the oxygen of water, and hydrogen is set free. The production of a combustible gas by the action of glowing coal on steam was first carefully investigated in 1801, but the fact of its formation was known much earlier. This decomposition of water by coal may take place in either of two different reactions, carbonic oxide resulting from one reaction, carbonic acid from the other, and hydrogen being set free in either case.

$$\begin{array}{lrcl} & C+H_2O & = & H_2+CO \\ (1) & 12+18 & = & 2+28 \\ & 2 \text{ vols.} & = & 2 \text{ vols.}+2 \text{ vols.} \end{array}$$

$$\begin{array}{lrcl} & C+2H_2O & = & CO_2+2H_2 \text{ [e]} \\ (2) & 12+36 & = & 44+4 \\ & 4 \text{ vols.} & = & 2 \text{ vols.}+4 \text{ vols.} \end{array}$$

The first process is effected with excess of steam and at a high temperature; when the temperature falls carbonic acid is produced. The decomposition begins at about 600° C. In practice both processes go on together, and although it is impossible to produce water gas free from carbonic acid yet in the practical working of the water gas process it must be so directed as to approximate as near as may be to the form of Equation I. The following data are useful for calorimetric calculations:

TABLE I.

1 kilogramme	Products of combustion in kilogrammes.	Heat of combustion in kilo. heat-units.
Hydrogen......	9.00 H_2O	34180 (Thomsen)
Carbon........	2.33 CO	2473
"	3.66 CO_2	8080
Carbonic Oxide	1.57 "	2403
Marsh Gas (CH_4)	2.75 "	13346 (Thomsen)
" " "	2.25 H_2O	
Ethylene (C_2H_4)	3.14 CO_2	11960 "
" "	1.30 H_2O	

TABLE II.—(Calculated from Table I.)

1 cubic meter.	Weight in kilogrammes at 0° and 760 m.m.	Heat of combustion of 1 cubic meter in kilo. heat-units.
Hydrogen......	0.0896	3063
Carbonic Oxide	1.2544	3014
Marsh Gas	0.7158	9566
Ethylene......	1.2544	15099

Taking the gases at 0° and 760 m.m., for convenience, it results from the above that equation I yields a water gas composed of equal volumes of carbonic oxide and hydrogen with a combustion heat of $\frac{1}{2}(3063)+\frac{1}{2}(3014)=3038$ units.[f]

Water gas obtained by equation II contains $\frac{2}{3}$ volume of hydrogen and $\frac{1}{3}$ volume of carbonic acid with a combustion heat of $\frac{2}{3}(3063)=2042$ units.

If purified by absorbtion of carbonic acid from the mixture, a process involving some expense, this gas has still a lower calorific power per cubic meter than the first, being simply hydrogen with a combustion heat of 3063 units. Equation I is therefore preferable in practice as a guide to the process. In order to decompose water a definite expenditure of heat is required, the amount bound up in the decomposition being simply that which would be evolved again when water is reproduced by combustion of hydrogen. When we decompose water by hot carbon, oxygen combines with the latter and heat is set free. The difference of these quantities is the quantity of heat taken from external sources in the man-

ufacture of water gas. According to Equation I:

$$2 \times 34180 = 68360$$
$$-(12 \times 2473) = 29676$$
$$38684 \text{ units.}$$

1 kilo. of carbon requires $\frac{38684}{12} = 3224$ units and to develop this quantity of heat 0.40 kilo. of carbon must be completely burned, *i. e.*, $\frac{3224}{8080} = .40$.

Taking now Equation II, we have

$$4 \times 34180 = 136720$$
$$-12 \times 8080 = \frac{96960}{39760}$$

and $\frac{1}{12}(39760) = 3313$.

To develop this heat requires the complete combustion of $\frac{3313}{8080} = 0.41$ kilo. of carbon, rather more than for Equation I, but the gas produced according to Equation II, has a heating power somewhat higher in the aggregate.

Composition of Water Gas.

The theoretical composition of water gas, as already shown, is 50 per cent. nitrogen and 50 per cent. carbonic oxide; in practice, however, the product always contains more or less carbonic acid, since it is impossible to prevent entirely the decomposition of water according to Equation II, and the constituents of the air, oxygen and nitrogen, are also present in small quantity as would be expected from the nature of the apparatus. When uncoked coal is used the gas is also mixed with hydro-carbon gases resulting from dry distillation. The following table shows the composition of water gas from different sources, and, for comparison also, the composition of generator gas and illuminating gas, both of the latter being averages of many different analyses.

TABLE III.

Water Gas.	CO_2.	CO.	H.	CH_4.	C_2H_4.	N.	O.
Strong's Apparatus. (Dr. Moore)	2.1	35.9	52.8	4.1	..	4.4	0.8
Anthracite, American. (Quaglio & Dwight)	2.1	35.4	52.8	4.1	..	4.4	0.8
English Coal. Stockholm	4.0	40.0	49.0	6.0	..	1.0	
Coal from Hägenäs	2.6	34.8	59.6		..	3.0	
Anthracite from Wales	3.6	34.1	61.3		..	1.0	
1 part coke and 3 parts dry peat	7.0	35.5	57.0		..	0.5	
1 " " 3 " wet "	9.0	33.4	57.1		..	0.5	
1 " " 3 " Eng'l coal dust	6.8	35.0	57.2		..	1.0	
Coke in Frankfort. (Dr. Bunte)	7.3	34.5	50.0		..	7.5	0.[illegible]
Illuminating gas	1.00	6.0	47.0	38.0	5.0	2.5	0.[illegible]
Generator gas	6.00	23.0	1.0	..	..	70.0	.

Yield of Gas.

Taking the theoretical composition of water gas and applying the data of Table II, we have for the weight of one cubic meter at 0°,

$$\tfrac{1}{2} \times 1.2544 = 0.6272$$
$$\tfrac{1}{2} \times 0.0896 = .0448$$
$$.6720 \text{ kilo.}$$

and according to Equation I,

12 kilos. C yield 30 kilos. of water gas.
∴ 1 " " " 2.5 " " "

But 2.5 kilos. of gas $= \frac{2.5}{.672} = 3.72$ cubic metres. As already shown 0.4 kilo. carbon must be burned to convert 1 k into gas by supplying the necessary h To obtain 3.72 cbm. of gas, therefore, must use 1.4 kilos. of carbon, or 1 l of carbon yields theoretically $\frac{3.72}{1.4} =$ cbm. of water gas. In practice, howe the yield is much lower. In Stockh for example, 1 kilo. of coal yielded 1 cbm. of water gas, and in Frankfort, 1 cbm., or, admitting with Dr. Bunte coke leaves a residue of 20 per cen of ash and dust, we obtain from 1

of carbon $\frac{1.225 \times 100}{80} = 1.531$ cbm. of gas, *i.e.*, 58 per cant. of the theoretical yield. This small yield is to be ascribed especially to the great waste of heat which the process involves, the consumption of heating coal being much greater than theory requires.

Loss of Heat in the Preperation of Water-Gas.

Taking as a basis the figures of Dr. Bunte's experiments at Frankfort,(f')1 kilo. of coke, containing 80 per cent. of available carbon, yields by direct combustion $0.8 \times 8080 = 6464$ heat units. 1 kilo. coke yields 1.225 cbm. of water-gas, which, using the data already given, develops in burning $0.345 \times 3014 + 50^{(g)} \times 3063 = 2571$ units per cbm. $\therefore$ 1,225 cbm. yields $1.225 \times 2571 = 3149$ units. But 1 kilo. of coke yields 6464 units, the water gas prepared from it therefore yields in combustion only 48.7 per cent. of the original heating power of the coke.

The greatest loss of heat according to this investigation results from the escape of the hot gases into the chimney during the heating period, *i. e.*, while the blast of air is urging the fire to its greatest heat preparatory to the admission of steam. The chimney gases escape at a temperature of 660°C. and the loss from this source is estimated by Bunte at 23 per cent. The finished gas leaves the furnace at 500°C. causing a loss of 3 per cent. and the radiation of heat from the walls of the furnace, which have an area of 40 sq. meters and an average temperature of 140°C., involves a loss of 11 per cent. The remaining difference of 14 per cent. must be ascribed to other imperfections incident to the process. The expenditure of heat in making steam for the furnace and in driving the fan is not taken into account in the calculation.

Comparison of the Heating Power of Water Gas, Coal Gas and Generator Gas.

This comparison may be most simply made by calculating the combustion heats developed by 1 cbm. of each gas under consideration, or more definitely, the number of kilogrammes of water that may be heated 1.°C by the combustion of 1 cbm of the gas. Take, as an example for such calculation, coal gas with the composition given in Table III.; 1 cbm. of such gas contains, of combustible gases,

CO. 0.06 cbm.	CH_4. 0.38 cbm.
H. 0.47 "	C_2H_4. 0.05 "

Using the data of Table II. we have for the combustion heat of the gas, $.06 \times 3014 + 0.47 \times 3063 + 0.38 \times 9566 + 0.05 \times 15099 = 6010$ units.

The combustion heats given in Table V. under the head of available heat in combustion products, "0.°C" are calculated in the same way.

It must be observed, however, that in these operations the products of combustion are supposed to have been cooled to 0.°C and that all vapor of water has become liquid. In practice this assumption is not realized, since the products of our fires escape at a temperature of at least 200°C. The relative combustion heats of the different gases are altered when we calculate the useful effect of a heating gas of which the products escape at 200°C. The heating power of gases containing hydrogen is lowered under these conditions by reason of the high latent heat of steam and its relatively great specific heat. In the calculation also it is simplest to deal with volume relations only and to calculate the specific heat of a standard *volume* of the gas, giving an expression, for example, of the number of heat units required to effect a change of 1°C in a cubic meter. When the specific and latent heat of water have to be introduced into the calculation it is simplest to regard water as a permanent gas which requires 637 units to bring it from 0° to 100°C and above 100° has the specific heat 0.475. Applying these principles we have the following table:

Table IV.

	Latent and specific heat.	1 cbm. gas at 0° and 760 mm. weight kilos.	Latent and specific heat of 1 cbm. gas.
Water..........	637. 0.475	0.8064	514. 0.383
Carbonic acid.....	0 2164	1.9712	0.4266
Nitrogen..........	0 244	1.2544	0.3061

The numbers of the last column are

products of the preceding columns of figures.

The combustible constituents of the kinds of gas already considered are CO, H, CH_4 and C_2H_4, and the combustion reactions of these, using molecular formulæ, are:

(*a*) $2\,CO + O_2 = 2\,CO_2$
4 vols. + 2 vols. = 4 vols.

(*b*) $2\,H_2 + O_2 = 2\,H_2O$
4 vols. + 3 vols. = 4 vols.

(*c*) $CH_4 + 2O_2 = CO_2 + 2\,H_2O$
2 vols. + 4 vols. = 2 vols. + 4 vols.

(*d*) $C_2H_4 + 3\,O_2 = 2\,CO_2 + 2\,H_2O$
2 vols. + 6 vols. = 4 vols. + 4 vols.

In these equations the water is considered as gaseous, which is only the case when the temperature is above its own boiling point.

If we consider that the combustion products escape at 200°C, combustion in air requires not only that these be heated to 200°, but also that the nitrogen, which accompanies oxygen in the air, be raised to the same temperature. Taking the composition of air at 20 per cent. O and 80 per cent. N, instead of 20.9 per cent. and 79.1 per cent., the exact proportions, we can calculate from the data of Tables II. and V. the heat taken off by the whole volume of gases escaping from the furnace at 200°C, and hence the residue of heat available for work.

Since 1 cbm. of carbonic oxide in burning yields an equal volume of carbonic acid, and takes up in the process ½ cbm. of oxygen and $4\times\frac{1}{2}$ cbm. of nitrogen (equation *a*), the available heat for 1 cbm. of CO will be $3014-(200\times0.4266)-(200\times0.3061)\times4\times\frac{1}{2}=2806$ units. For hydrogen (Table II. and equation *b*), $3063-514-(100\times0.383)-(200\times4\times\frac{1}{2}\times0.361)=2388$ units. For marsh gas (Table II. and equation *c*), $9566-(200\times0.4266)-(2\times514)-(2\times100\times0.383)-(200\times0.3061\times4\times2)=7886$ units. For ethylene (Table II. and equation *d*), $15099-(2\times200\times0.4266)-(2\times514)-2\times100\times0.383)-(4\times3\times200\times0.3061)=13089$ units.

With the aid of the numbers thus obtained, the calculation of the available heat, supposing the gases to escape from the furnace at 200°C, is very simple. As an example, we may take illuminating gas with the composition given in Table III.: $.06\times2806+(0.47\times2388)+(0.38\times7886)+(0.05\times13089)-(0.01\times200\times0.4266)-(0.025\times200\times0.3001)=4942$ units. Results obtained in the same way for the gases of Table III., are given in the second column of figures in the following table:

TABLE V.

Water Gas.	Available heat in combustion products.		Theoretical flame temperature.
	0°C.	200°C.	°C.
Strong's Apparatus	3090	2590	2620
Anthracite, American	3080	2570	2610
English coal, Stockholm	3280	2760	2620
Coal from Högenäs	2870	2400	2660
Anthracite from Wales	2910	2420	2660
1 part coke and 3 parts dry peat	2820	2350	2630
1 part coke and 3 parts wet peat	2760	2290	2600
1 part coke and 3 parts Eng. coal-dust	2810	2340	2630
Coke, Frankfort	2570	2150	2560
Illuminating Gas	6010	4940	2460
Generator Gas	724	621	1470

In the third column of the above table are the theoretical flame-temperatures, *i. e.*, the temperatures that would be attained by the flame of burning gas when supplied with exactly the required quantity of air and supposing the entire quantity of heat developed to be applied to raising the temperature of the products of combustion and the accompanying nitrogen. As an example of the method by which this temperature is calculated, take the gas made in Strong's apparatus, Table III, containing in 1 cbm 0.205 CO_2, 0.359 CO, 0.528 H, 0.041 CH and 0.044 N. From Tables IV and V an equations (*a*), (*b*) and (*c*) we have,

$$3090 = 0.4266\,(0.205+0.357+0.041)\,t + 514\,(0.528+2\times.041) + 0.383\,(0.528 + 2\times0.041)\,(t-100) + 0.3061\,(0.044+[\tfrac{1}{2}\times0.359+\tfrac{1}{2}\times0.528+2+0.041])\,t.$$

Whence $t=2620$°C.

Inspection of Table V indicate th water gas has about four times t

heating power of generator gas and that common illuminating gas has more than double the heating power of water gas. The flame temperatures attained by the different gases agree more nearly with one another.

To What Extent is the Water Gas Process Worthy of Practical Application?

Theoretically nothing is gained or lost in the water-gas process; the same quantity of air is necessary to burn the gas as to burn the coal from which it was originally made.

$$C+O_2=CO_2$$
$$C+H_2O=H_2+CO$$
$$H_2+CO+O_2=H_2O+CO_2$$

The process serves only to transform solid fuel into gas, in which latter form it can commonly be burned with better effect. On the whole there is a loss of heat however, since the water which serves the ends of the process must finally be heated to the temperature at which the products of combuotion escape and its relatively high specific and latent heat must be taken into account. It follows from this that for large heating operations a special generator, close to the heating chamber, is better than a water gas furnace, and that even the simultaneous introduction of steam and air into the generator is disadvantageous, although in this case none of the heat produced in the generator is lost, because the immediate transfer of the hot gas into the heating chamber leaves no opportunity for the heat to escape.[h] The case is different when the gas developed in the generator is allowed to cool before reaching the heating chamber; in this case it is worth while to use the heat of the generator in part for the production of water gas the greater part of the heat taken up in the decomposition of water in the generator being set free again in the heating chamber of the furnace, *i. e.*, it is useful in this case to pass a mixture of steam and air through the generator and thus to produce a mixture of water gas and generator gas.

For the purpose of a commercial gas, water gas presents decided advantages over generator gas when we consider the great cost of mains for distribution of the gas. The greater the heating effect of the gas the greater is the practicability of distributing it and the more effective is the system of pipes by which it is carried. As the heating power of water gas is about four times that of generator gas and as the distribution of the latter requires the conveyance of 70 per cent. of worthless nitrogen through a costly system of pipes, it may be dismissed from consideration as a commercial gas. Illuminating gas, however, has a certain advantage over water gas as this has over generator gas, since its heating power is about double and the cost of producing water gas must be considerably lower than that of illuminating gas in order that it may compete with it as a heating material. The possibility of this, flows, however, from the fact that the cheapest combustible matter may be used and that the whole material is converted into gas by a process which is much cheaper in efficiency and labor than the ordinary coal-gas process. It may perhaps even now be of advantage in large gas works to use a water-gas furnace in connection with the retort-furnace during the time of greatest consumption in winter, and to prepare carbureted water gas to mix with the ordinary gas. Such gas is easily made of illuminating power equal to that of the best coal gas.[i].

The superior efficiency of the water-gas furnace is apparent when we consider that an ordinary retort yields only 150 cbm. of coal-gas in 24 hours. In Frankfort 2500 cbm. were made daily, corresponding to the production of 17 retorts. Quaglio asserts that such furnaces may be made to do the work of 60 retorts and will be cheaper in cost of working as the production becomes greater.

If water gas can be made sufficiently cheap, which can hardly be questioned, and if gas lighting should be surpassed in the future by electric lighting, illuminating gas will not even then be entirely abandoned and heating gas will be carbureted by the consumer at his house by a special apparatus, using petroleum naphtha or some such material to give luminosity to the gas.

Water gas, owing to its high per cent. of carbonic oxide is very poisonous, as also is common illuminating gas. The latter has, however, the estimable property of smelling so strongly that $\frac{1}{10000}$ of it can

be detected in the air by its odor. Water gas, on the other hand, has scarcely any smell.(i) This last objection may be met by mixing with the gas some strongly-smelling volatile matter, a device which prevents no great technical difficulty. It appears from the preceding that the water gas process, under certain conditions, has a future before it, and in a limited sense it may be justly designated as the "fuel of the future."

APPENDIX.

(*a*) The term "generator gas" wherever it is used in this paper, refers to the gas obtained when air is passed through a thick bed of incandescent coal, as in the generator of the Siemens furnace.

(*b*) Such a system is now in operation in Yonkers, N. Y. Fuel gas, made by the Strong process is supplied to consumers through street mains.

(*c*) This refers to net H.P. The consumption per I.H.P. is about 22 cu. ft.; 1 cbm.=35 cu. ft., approximately. In regard to the use of water gas in gas engines, see a paper by Prof. W. E. Ayrton, "On the Economical Use of Gas Engines for the Production of Electricity." (London: Wm. Dawson & Son.) This paper relates to experiments made with the "Dowson" gas, which is practically a mixture of water gas and generator gas. Also an article in *Sci. Am.* Suppl. No. 305 on the Dowson process. From some preliminary experim made by the translator using the strong w gas in an Otto engine, he feels warrante stating that the consumption of this gas pe P. (net) in such an engine lies between 60 70 cubic feet per hour.

(*d*) Here, as in all cases in this paper, perc age compostion is indicated in *volumes*.

(*e*) Here, and in similar equations, the vol of the hydrogen *atom* is regarded as unity, the figures of the author, who regards molecule (2 vols.) as the unit of volume, doubled. The *volume ratios* remain, of cou unchanged.

(*f*) Quantities of heat in this paper are pressed in calories or kilogramme units, i the quantity of heat required to raise the t perature of one kilo. of water from 0°C to 1

(*f'*) See "Wagner's Jahresbericht," 1 p. 1049.

(*g*) Assuming for simplicity that this 50 cent. is all hydrogen.

(*h*) In practice with the Siemens furnace generator gas is cooled by passing through iron pipe exposed to the air.

(*i*) The extensive use of the Lowe, Motay other processes for manufacture of carbur water gas in America adds emphasis to statement.

(*j*) A small quantity of sulphur, such water gas commonly contains, and which i for many purposes, unnecessary to rem suffices to give a decided odor to the gas. T sulphur exists largely if not entirely as phureted hydrogen, and is easily removed absorbents when its removal is demanded.

THE MICROSCOPICAL STRUCTURE OF IRON AND STEE

By DR. H. C. SORBY, F. R. S.

From "Iron."

The lecturer said he was first induced to investigate the subject of his lecture as bearing on the structure of meteoric iron. Little or nothing was known of the minute structure of irons and steels, and it was requisite to devise appropriate methods to ascertain their intimate molecular constitution, and also to invent special means of illumination to enable the structure to be examined with a microscope. Much might be learned of the nature of iron and steel by artificial fractures, but these showed more the lines and planes of weakness, and the divisions between the constituent crystals, than the actual structure of the crystals and their relation to one another. It was, therefore, requisite devise some means of ascertaining exact structure of the metals, indepe ent of any lines of weakness revealed fractures. This was accomplished in following manner. Thin flat portions iron or steel were fixed on glass, and upper surface filed flat and afterwa ground to a perfectly even surface fine whetstones, so as to get an even s tion without any tearing or burnish of the surface. Afterwards this up surface was polished in such a man as to leave the most minute portions the metal quite undisturbed, and surface entirely free from polisl grooves or scratches. Great care sho be used to avoid any irregular resu

* Lecture delivered at Firth College, Sheffield.

which might be due to finger-marks or anything else which would disturb the subsequent process. This carefully polished portion of iron or steel was placed in very dilute nitric acid, and its action carefully watched. After remaining in the acid a short time the section was taken out and examined under water in a glass trough; and if the structure had not been sufficiently developed the section was again treated with the dilute acid. As soon as it was thought that the etching was in every respect satisfactory the specimen was quickly washed, dried, and a portion of thin glass mounted over it with Canada balsam. When all these processes had been carried out in a perfect manner such preparations might be studied with a magnifying power of several hundred linear. For this purpose, however, special illuminators were required, since the objects were, of course, opaque, and must be examined either by oblique surface illumination, or by a peculiar direct surface illumination first applied to this subject. The development of the structure by means of weak acid was due to the fact that some of the constituents were not acted upon at all, and others in varying degrees. Portions of slag or cinder remained in their original state, and were seen as black specks or patches of varying size and shape. Some constituents of iron and steel remained perfectly bright and brilliant, whilst others became coated to a varying extent with a more or less brown substance, so as to show the outline of the individual crystals very perfectly. Other constituents were so acted upon as to develop a very close grooved structure, which gave rise to very varying colors of exquisite tint and brilliancy. Thus, by difference of color or other characteristics, the outline of the individual crystals and their own intimate structure were shown to great perfection. Some of the objects prepared in this way might be reckoned amongst the most beautiful microscopical objects that could be seen, and no one who had not examined them would guess their nature, as some looked far more like organic bodies than anything one was accustomed to fancy characteristic of iron or steel. Other specimens were studied in a different way. The surface was polished in a less perfect manner, and the action of acid continued much longer, so as to produce sufficient difference in the relief in the case of the different constituents of the iron and the steel to enable prints to be taken from them as from woodcuts. In the case of some varieties the results thus obtained were exceedingly gratifying. These studies, Dr. Sorby went on to say, were carried out by him many years age, since which time, his attention had been devoted to other subjects, but he was convinced that a very great deal still remained to be learned. In fact, to thoroughly determine the exact nature of all the constituents seen in the specimens would involve many years of careful chemical and microscopical investigation; since, though many of them differed very greatly in microscopical and physical character, their size was so small that it would be difficult or impossible to separate them in such a manner as to determine their chemical constitution, and it would be requisite to ascertain their true nature by careful induction from facts observed under special circumstances. As far as could be learned from the careful use of the microscope, various kinds of iron and steel contained at least seven well-marked constituents. In the first place, there was pure iron, and what were probably three well-marked compounds of iron, with varying amounts of carbon or other substances met with in small quantities in different sorts of iron and steel; portions of included slag, well marked crystals of graphite, and small crystal, which might be silicon. The lecturer then proceeded to exhibit by means of the oxhydrogen lamp a considerable number of illustrations of the structure of various kinds of artificial iron and steel, some being photographed by Mr. Charles Hoole direct from the preparations, others from drawings by the author, and others from nature prints, made by the process already described. Commencing with various kinds of cast iron, it was shown that their structure was sometimes chiefly modified by the presence of crystalline plates of graphite, over which was deposited what was probably free iron, and the interspaces filled by what were probably two distinct compounds of iron and carbon, in other cases the structure was mainly dependent on the crystallization

of the iron itself, the graphite being thrown off towards the close of the process. In the case of white refined iron the principal constituent probably was an intensely hard white refined iron with much carbon, associated with which were one or more of the other compounds of iron and carbon present in grey iron. The microscopical structure of this white iron was exceedingly curious and beautiful. The next illustrations were of various kinds of wrought iron. The hammered bloom was shown to consist of an irregular mixture of crystals of iron and portions of slag. When rolled out into a bar those portions of slag not squeezed out were thrown out into long threads, but the crystals of iron seen in the bar were not the original crystals of the bloom, but fresh crystals formed on the cooling of the bar, since they exhibited little or no tendency to elongation in the line of the length ôf the bar, as would occur if the original crystals were drawn out by the process of rolling. The fiber seen on fracturing such specimens of wrought iron was mainly due to the elongation which occurred during the fracture, and was not a characteristic of the unaltered iron. In connection with this, illustrations were shown of the structure of armor plates, of welded joints, and all of those kinds of iron which are employed in the manufacture of steel by the cementing process. The change of structure produced by this cementing process was, the lecturer pointed out, very striking, the most characteristic feature being the development of a net-work of flat crystals of an intensely hard compound of iron and carbon, scarcely acted upon at all by dilute acid, so that the rest of the steel may be dissolved away, and this compound left in sufficient relief for exquisite prints to be taken as from a woodcut. Numerous illustrations thus taken direct from the iron and steel were exhibited with a lantern; and few microscopical objects are more beautiful than some of the preparations of this cemented blister steel, since, when specimens are prepared in the manner already described, some of the constituents gave rise to the most exquisitely beautiful colors by interference of light. The difference between the structure of the outside of the converted bars where this hard compound of iron and carbon had been developed and of the interior of the bar was shown to be very great, this latter being mainly due to re crystallization of the original iron. Ingots of cast steel produced by melting such blister steel had a totally different structure, which depended, in the first place, on large crystals, and in the second place, on the minute microscopical structure of these crystals. The principal difference between the structure of such an ingot and that of hammered bars was that the whole mass was made more uniform and the grain very much finer. This was still more the case when the hammered steel was hardened, in which case the constituent crystals were so small that it was very difficult to learn much about them by microscopical study. The structure of Bessemer steel ingots was materially different from that of the varieties of steel containing more carbon, and though of coarser grain, closely approached the structure of some varieties of Swedish irons. This structure, upon hammering, was greatly altered and became of finer grain and more uniform. In conclusion, the lecturer exhibited several illustrations of the structure of meteoric iron. This differed so much from that of most varieties of artificial iron that it was a long time before any point of similarity could be discovered. Alloys of iron and nickel of the same composition as meteoric iron were melted and slowly cooled, but nothing at all resembling the structure of meteoric iron was obtained. At length it was found that the closest approach to this structure was in the case of iron which has been kept for a long time at a high temperature, but not actually melted, under which conditions some varieties of iron containing little carbon crystallized in large crystals, having some of the important characteristics of meteoric iron, whilst iron containing a certain amount of carbon crystallized in a manner imperfectly resembling the very perfect crystallization of meteoric irons, only that in these artificial preparations there was crystallization of varying compounds of iron and carbons, whereas in the meteoric iron there were varying compounds of iron and nickel. The inference to be drawn from these facts was probably that meteoric iron crystallized very slowly at a temperature below fusion.

DEEP SEA SOUNDING.

By LIEUTENANT COMMANDER THEODORE F. JEWELL, U. S. N.

Record of the United States Naval Institute.

It is within the last thirty years only that any material addition has been made to our information as to what lies beneath the surface of the sea. For centuries men had speculated as to the depth of the ocean, but so far as accurate knowledge is concerned, they were completely ignorant. It was long held for instance, that analogy indicated that the deepest parts of the ocean were not deeper than the height of the highest mountains. But early experiments with long lines and heavy weights, seemed to show that the ocean's depths were unfathomable. Indeed, many exceedingly intelligent persons, among whom I may cite the late Vice-Admiral Fitzroy, of the Royal Navy, were of the opinion that a lead could not be made to sink to the bottom of the deepest seas, on account of the increased density of the water under the enormous pressure;—notwithstanding the fact that water had been shown, long before, to be practically incompressible. Irrational conceptions, such as this, of the difficulties attending deep-sea soundings seem to have prevented the accomplishment of much that, otherwise, might have been done. Yet Scoresby, writing in 1817 or 1818, recognizes clearly that the principal difficulty is the uncertain intimation given when the lead strikes the bottom; and he even suggests that this could be remedied if some method could be devised for determining the tension of the sounding line throughout its descent. The earlier devices for sounding all involved the use of a heavy sinker, with a correspondingly large line, and while they answered very well in depths not exceeding a few hundred fathoms, beyond a thousand fathoms they were valueless. If a large line were used the sinker would not carry it rapidly and vertically downward, while a lighter line was incapable of drawing up its own weight along with that of the lead. With a large line no impulse was felt when the lead reached the bottom, and the line would go on running out by its own weight, coiling itself over the lead. Indeed, this would happen with any line, and, in most cases, any attempt to check it was attended by its parting. The record of deep sea soundings previous to 1850, is consequently not only meagre, but entirely untrustworthy. It was but a natural sequence to such uncertainty that all sorts of devices should have been resorted to in order to dispense with the use of a line as a measuring instrument. The impregnation of wood by water under the greatly increased pressure was one of these devices, but it was found that the impregnation was practically complete at three hundred fathoms. Ericsson invented a lead in which air was compressed by the pressure of the water, and by the amount of compression the depth was to be estimated, but the instrument failed at great depths. Explosions were resorted to, the velocity of sound in water to be the means of measurement, but alike unsuccessfully.

But great strides have been made in the last thirty years in the development of deep-sea work, and instead of being unable to sound at all, we can now not only sound in deep water, but can do so with ease, certainty, and astonishing rapidity. Among those to whom we owe this extraordinary advance, officers of our Navy stand in the foremost rank. I am, therefore, about to present a short and, necessarily, an imperfect account of the process of this development, confining myself generally to the achievements of our own countrymen.

In October, 1849, the schooner Taney, Lieut. J. C. Walsh commanding, sailed from New York equipped for deep sea research. Her arrangements for sounding seem to have been made with great care, but she was small and unseaworthy —a vessel of but one hundred tons, that could not keep at sea to complete her cruise. In this little vessel steel wire was first used as a sounding line. I do not know whether the idea of using wire was original with Walsh or not, but it is interesting to note that this, the first

attempt at the employment of this material with which the greatest feats of deep sounding have since been done, was by an American Naval officer.

The Taney was supplied with 14,300 fathoms of the "best English steel wire," in five sizes, Nos. 5, 7, 8, 10, and 13, Birmingham gauge. The wire was tested to one-third more strain than it was estimated would be brought upon it. I give Walsh's language in describing how it was marked and prepared for use. "Of this an extent of 7,000 fathoms, weighing eighteen hundred lbs. (the remainder, consisting of the smaller sizes, Nos. 10 and 13, being stowed away as spare wire), carefully measured and marked with small copper labels, was linked into one piece, and wound upon an iron cylinder 3 feet in length and 20 inches in diameter—the larger sizes being wound first so as to be uppermost in sounding. Two swivels were placed near the lead, and one at each thousand fathoms, to meet the danger of twisting off by the probable rotary motion in reeling up. The cylinder with the wire was fitted to a strong wooden frame, and machinery attached,—fly-wheel and pinions, to give power in reeling up. Four men at the cranks could reel up with ease, with the whole weight of the wire out. Iron friction bands, which proved of indispensable importance, were connected to regulate the running off the reel. One man, with his hand upon the lever of one of these friction bands, could preserve a uniform, safe velocity, checking or stopping the wire as required. The whole apparatus could be taken apart, and stowed away in pieces (being so large and massive, this was indispensable in so small a vessel as the Taney). When wanted for use the frame was put together and secured to the deck by iron clamps and bolts, near amidships, the reel hoisted up from below and shipped in its place; a fair leader was secured to the taffrail, being a thick oak plank, rigged out five feet over the stern, having an iron pulley, 18 inches in diameter, fitted in its outer end, and two sheet-iron fenders 3½ feet long, of semicircular shape fitted under it to guard the wire from getting a short nip in the drifting of the vessel. The wire was led aft from the reel, over the pulley which traversed freely in the fair leader, and passed between the fenders into the water."

The first trial of this apparatus was made on Nov. 15th, 1839. The sinker used was a ten pound lead, and attached to the wire was an instrument, weighing six pounds, invented by Maury for recording the depth descended by the sinker. The cast was made under the most favorable circumstances; the sea was smooth and there was hardly a breath of wind. The wire went down vertically, "preserving," says Walsh, "the exact plumb-line throughout the sounding." When fifty-seven hundred fathoms had run out, the wire broke at the reel, but from what cause we are not informed. It was probably owing to the imperfection of the method used in joining the different lengths of wire together.

Walsh considered that in this sounding he had proved that the depth of the ocean at the point where it was made (Lat. 31° 59′ N. Long. 58° 43′ W.), about four hundred miles east of Bermuda, was not less than fifty-seven hundred fathoms. This depth was marked upon the chart with the sign of "no bottom." After a few years, however, Maury caused the sounding to be marked "doubtful," and finally in 1857 it was erased from the chart. Walsh's mistake was in using a sinker of too little weight for such large wire. A weight of sixteen pounds became insignificant in comparison with that of the wire (which in water was more than two hundred pounds to the thousand fathoms) when two or three thousand fathoms had run out, and consequently the shock when the sinker touched the bottom was inappreciable. The depth at the point where the sounding was made is between twenty-five hundred and twenty-eight hundred fathoms.

Several other unsuccessful attempts were made by the Taney to sound with the wire, but in every instance the wire broke when about two thousand fathoms had run out. This material was, therefore, deemed unfitted for the purpose and its employment was discontinued. Lieut. M. F. Maury, who was very active in the study of the ocean at that time, came to the conclusion that no reliance was to be placed upon soundings made in great depths with wire. But

the experiments of Walsh incited the Navy to new exertions. Sounding twine was substituted for wire; instructions for its use were prepared and issued, and a supply of twine furnished to every vessel in commission. This twine was of two sizes—the smaller was capable of supporting a weight of seventy pounds, the larger would bear one hundred and fifty pounds. Ten thousand fathoms of the small twine and five thousand of the large was supplied to each ship. The sinkers to be used were one or more 32 pdr. shot. The smaller twine was to be employed in great depths where there was little probability of recovering the shot; the larger, when it was probable the sinker might be recovered.

Among the first to use the twine was the sloop-of-war Albany, Commander Platt, which proceeded to sea in 1850. The officers of the ship entered heartily into the work, but the experiments were conducted under the direct supervision of the First Lieutenant, Wm. Rogers Taylor. The twine was wound upon a "delicately constructed reel which would turn with as little friction as possible." It was at first thought that this light line, weighing but a pound to 160 fathoms, would cease running out when the shot struck the bottom, or that it would, at least, move so much more slowly that the instant could be determined. This supposition was afterwards proved in error, and consequently the first soundings, which, indeed, were little better than guesses by the feel of the line, were subsequently discredited. Many discouragements were encountered, but they persevered. The twine proved of bad quality, breaking frequently when two or three hundred fathoms had run out, and we find recorded as lost in making one cast, as many as eleven shot. The line was overhauled fathom by fathom; weak portions were weeded out; and so at the end of six months they had made thirty-six casts. During this period the Albany expended forty thousand fathoms of twine, and in no instance where the depth exceeded one hundred fathoms was the shot recovered. In December, 1851, the ship was supplied with new twine, which had been very carefully made. One thousand fathoms of this weighed 8¾ pounds. It was overhauled as before, the lengths were reknotted and parts of it were waxed. Notwithstanding these precautions, the line continued to part, but still much good work was done, and the experience thus gained was of great value to those who were to follow. As a result of these soundings it was demonstrated that the line would not cease running out when the sinker touched the bottom and that the feel of the line was an uncertain indication as to whether the sinker was on the bottom or not. It was also found impossible to make a cast from the vessel in any except the calmest weather. It was shown that the waxed twine would go down more rapidly than when unwaxed; and it was found that the twine, although tested to 70 pounds, would not weigh a 32 pound shot, owing to weak places, and the frictional resistance of the water.

Near the close of the Albany's work Taylor adopted a suggestion of Maury's and noted the time of running out of each hundred fathoms. The result is remarkable as indicating for the first time a means of determining with some degree of accuracy the instant when the sinker touches the bottom. It was found that from the beginning of the cast the time of running out of each hundred fathoms gradually increased, so that if at any particular hundred fathoms the time interval was increased more than its due proportion it was an indication that bottom had been reached. This was, probably, the most important fact developed by this cruise. So important did it seem to those interested in deep sea soundings at that time that the method was immediately adopted, and in nearly all, if not all subsequent casts it has been used. In the recent Challenger expedition it was the only method adopted for determining when the sinker was at bottom. The neglect in observing this time interval properly or the failure to interpret it correctly led Lieut. J. P. Parker, of the Congress, to report, in 1852, a sounding of eighty-three hundred fathoms; Capt. Denham, of H. M. S. Herald, one of seven thousand seven hundred six fathoms, the same year; and Berryman, in 1853, one of sixty-six hundred fathoms. Denham did observe the time of running out of each five hundred fathoms, but an examination of his

record shows that the bottom was reached at between twenty-three hundred and twenty-eight hundred fathoms. Parker did not observe his time intervals regularly, nor did Berryman. Maury estimates that Parker reached bottom in about twenty-eight hundred fathoms, and Berryman was afterwards satisfied that his sounding was incorrect.

Such was the state of affairs when the first really successful deep sea sounding expedition was organized. This was in 1852, in the Brig Dolphin, under the command of Lieut. S. P. Lee. Profiting by the experience of the Albany, Lee caused his sounding twine to be carefully examined as it was received, and rejected thousands of fathoms. Notwithstanding his supervision, much of the line was still defective, and of the first seventeen casts made, only the last was successful. Lee soon repeated the experience of Walsh and Taylor—only in the smoothest states of the sea, and in calm weather could good casts be made from the vessel. He, therefore, adopted the expedient of sounding from a boat, which by means of an oar on either side could be kept over the line. By doubling his line for the first two or three hundred fathoms he prevented its carrying away, and after that there was little trouble in obtaining quite reliable casts. Lee always noted his time intervals, though he did not always correctly interpret them. With a heavy sinker and a light line the shock when bottom is reached can usually be detected by those experienced in the work if proper care be used. But this method is uncertain and the officers of the Dolphin were frequently led into error by adopting it.

The problem of deep sea soundings was so far solved. The depth of the ocean could be determined at the expense of a 32 pound shot and a little inexpensive twine. The conditions necessary were a heavy, though not excessive, weight; a smooth, light line; some means of keeping the sounding vessel over the line; and an accurate record of the time required for running out of each fifty or one hundred fathoms.* The soundings of the Dolphin under Lee, and his s cessor Berryman, are universally rec nized as the first ever made in the de sea with any degree of accuracy.

But the subject was not allowed rest at this point. Hitherto when a c had been made the line had been cut parted, so that no specimen of the b tom had been brought to the surf from any great depth. In order tha more complete knowledge of the bott of the sea could be obtained, some c trivance for bringing up a sample of soil became requisite. Heretofore t had been done only by weighing sinker, to which some form of cup attached, and, indeed, it had never b accomplished at depths exceeding a th sand fathoms. The sounding twine not strong enough to weigh the sl and some other device was necess At this stage, about 1854, Passed M shipman Jno. M. Brooke appears w his apparatus, by which the shot co be detached when it reached the bot and the instrument with a specime the bottom could be lifted again to surface. I need not describe this in tion, every one present is familiar it. With some modification of its o inal form it remains to this day the m successful instrument of the kind invented. With it Berryman obta specimens from depths exceeding

Time of descent of each 100 fathoms after the fi fathoms.

Depth.	Interval		Depth.	Interval		Depth.	Int
Fath.	m.	s.	Fath.	m.	s.	Fath.	m
400	1	41	1600	2	36	2800	[illegible]
500	1	49	1700	2	43	2900	[illegible]
600	1	51	1800	2	41	3000	[illegible]
700	1	58	1900	2	44	3100	[illegible]
800	2	02	2000	2	44	3200	[illegible]
900	2	10	2100	2	46	3300	[illegible]
1000	2	14	2200	2	51	3400	[illegible]
1100	2	26	2300	2	53	3500	[illegible]
1200	2	32	2400	2	59	3600	[illegible]
1300	2	32	2500	3	07	3700	[illegible]
1400	2	34	2600	3	07	3800	[illegible]
1500	2	42	2700	3	08		

This table shows that the line ran out for tl twenty-four hundred fathoms at a gradually i ing rate. After the twenty-five hundred fathon passed out, the rate at which the line ran o mained almost constant for nearly a thousand oms. This indicates either that the under s current was carrying the line out, or that the of the line was doing so. In my opinion botto reached at about twenty-five hundred fa Soundings of the Challenger, made near the re position of this cast, show from twenty-five h to twenty-eight hundred fathoms. It is abou hundred and thirty miles south and west fr position of Walsh's cast of fifty-seven hundre oms, and this circumstance may not have been out its influence in causing the error.

* To show the absolute necessity of this last requirement, when rope is used. I subjoin a record of a cast made by Lee in Lat. 26° 32′ N. Longitude, 60° 06′ W., and reported by him as 3825 fathoms.

thousand fathoms, and Belknap has repeatedly brought samples of the bottom from more than four thousand fathoms.

Brooke's invention gave a new impetus to the work of sounding. Equipped with this instrument Berryman again (in 1856) put to sea in the Arctic, and sounded all over the North Atlantic. One result of this cruise was the discovery of what has since been called "the telegraphic plateau." Berryman's soundings showed that between Newfoundland and Ireland, there existed a remarkably uniform depth of water not differing much from two thousand fathoms. As soon as this discovery was announced the project of connecting the two countries by a submarine telegraph cable was agitated. Berryman made twenty-four casts on a great circle between St. Johns and Valentia, with a view to determining the practicability of the scheme. He was followed, in 1857, by Capt. Dayman, of the Royal Navy, who in H. M. S. Cyclops, went over the same ground, making thirty-four casts. Dayman used Brooke's detaching apparatus with Massey's sounding machine, by which the depth was recorded. Brooke's original invention had already been modified by Berryman, who replaced the shot by a long leaden cylinder thus to diminish the resistance to the descent, and also adapted a valved cup to the end of the sounding rod. Capt. Dayman made similar modifications, and in addition replaced the rope slings of Brooke's original device by wire ones which were more readily detached. Massey's sounding recorder was found to be useful as a check upon the soundings, but for some reason, difficult to explain, it is not a reliable instrument in deep water.

From the time of Berryman's soundings in the Arctic, the United States Navy took but little part in deep sea work, for several years The English Navy, however, continued the work with great activity. Brooke's detaching arrangements was universally employed. Its use had gradually introduced the intervention of larger lines, but in 1860 we find H. M. S. Bulldog adopting the old plan of a cod line and an iron sinker, the line being cut at each cast. In this cruise, however, the soundings were usually repeated with a detaching sinker and larger line in order to obtain bottom specimens. Many efforts were made to invent a machine which would bring up larger bottom specimens, but the methods of sounding hardly varied. The use of steam rendered the lowering of boats unnecessary, as a steamer could be kept over the line when sounding. Small engines were also introduced for reeling in the line, thus diminishing greatly the labor of obtaining a cast.

The progress which had been made in deep sea sounding, in 1870, can best be indicated by a description of the process as practised on board the Porcupine in that year. This vessel, commanded by Staff-Commander Calver of the Royal Navy, was operating under the auspices of a committee of the Royal Society, with Prof. Wyville Thomson as scientific director. The following extract from Prof. Thomson's book, entitled "The Depths of the Sea," describing a cast made in two thousand four hundred and thirty-five fathoms presents the art of sounding in its most perfect development at that period.

"The Porcupine was provided with an admirable double cylinder donkey-engine of twelve horse-power (nominal) placed on the deck amidships with a couple of surging drums. This little engine was the comfort of our lives; nothing could exceed the steadiness of its working and the ease with which its speed could be regulated. During the whole expedition it brought in, with the ordinary drum, the line, whether sounding line or dredge rope, with almost any weight, at a uniform rate of a foot per second. Sometimes we put on a small drum for very hard work, gaining thereby additional power at some expense of speed.

"Two powerful derricks were rigged for sounding and dredging operations, one over the stern and one over the port bow. The bow derrick was the stronger and we usually found it the more convenient to dredge from. Sounding was most frequently carried on from the stern. Both derricks were provided with accumulators, accessory pieces of apparatus which we found of great value. The block through which the sounding-line or dredging-rope passed was not attached directly to the derrick, but to a rope which passed through an eye at the end of the spar, and was fixed to a 'bitt'

on the deck. On a bight of this rope, between the block and the bitt, an accumulator was lashed. This consists of thirty or forty or more of Hodge's vulcanized india-rubber springs fastened together at the two extremities, and kept free from one another by being passed through holes in two round wooden ends like the heads of churn staves. The loop of the rope is made long enough to permit the accumulator to stretch to double or treble its length, but it is arrested far within its breaking point. The accumulator is valuable in the first place as indicating roughly the amount of strain upon the line; and in order that it may do so with some degree of accuracy it is so arranged as to play along the derrick, which is graduated from trial to the number of cwts. of strain indicated by the greater or less extension of the accumulator; but its more important function is to take off the suddenness of the strain on the line when the vessel is pitching. The friction of one or two miles of cord in the water is so great as to prevent its yielding freely to a sudden jerk such as that given to the attached end when the vessel rises to a sea, and the line is apt to snap. A letting-go frame, a board with a slit through which the free end of the sounding machine passed, and which supported the weights while the instrument was being prepared, was fitted under the stern derrick. The sounding instrument was the 'Hydra' weighted with three hundred and thirty-six lbs. The sounding line was wound amidships just abaft the donkey engine on a large, strong reel, its revolutions commanded by a brake. The reel held about four thousand fathoms of medium No. 2 line of the best Italian hemp, the No. of threads 18, the weight per hundred fathoms 12 lbs. 8 oz, the circumference 0.8 inch, and the breaking strain, dry, one thousand four hundred and two lbs. soaked a day, one thousand two hundred and eleven lbs., marked for fifty, one hundred and one thousand fathoms.

"The weather was remarkably clear and fine; the wind from north-west, force = 4; the sea moderate, with a slight swell from the north-west. We were in Lat. 47° 38′ N., long., 12° 08′ W., at the mouth of the Bay of Biscay. The sounding instrument, with two Miller-Casella thermometers and a water bottle attached a fathom or two above it, was cast off at the letting go frame at 2h. 44m. 20s, P. M. The line was run off by hand from the reel and given to the weight as fast as it would take it, so that there might not be the slightest check or strain."

(Here follows a table showing the time of running out of each one hundred fathoms, the time intervals varying from 45 seconds for the first hundred to 1 m. 52 sec. for the last.)

"In this case," continues Prof. Thomson, "the timing was only valuable as corroborating other evidence of the accuracy of the sounding, for even at this great depth, nearly three miles, the shock of the arrest of the weight at the bottom was distinctly perceptible to the commander, who passed the line through his hand during the descent. This was probably the deepest sounding which had been taken up to that time which was perfectly reliable. It was taken under unusually favorable conditions of weather, with the most perfect appliances, and with consummate skill. The whole time occupied in the descent was 33 minutes 35 seconds; and in heaving up 2 hours 2 minutes. The cylinder of the sounding apparatus came up filled with fine, grey Atlantic ooze."

This was the sum of our experience in deep sea sounding when, in 1873, the Tuscarora, Comdr. Geo. E. Belknap, was ordered to prepare for soundings in the Pacific Ocean, a field hitherto unexplored. The object of the Tuscarora's cruise was, primarily, to determine a practicable route for a submarine cable to connect the United States and Japan. The original plan comprehended a line of soundings on a great circle, as nearly as might be, from Cape Flattery, Washington Territory, to No-Sima, at the entrance of Yeddo Bay, Japan. Returning, a line was to be run from Cape No-Sima, by the Bonin Islands, to Honolulu, and thence to San Diego or San Francisco. But a short coal supply prevented the completion of the first line, and bad weather, due to the lateness of the season, rendered it advisable to return. On the passage back to San Francisco lines of soundings were run on and off shore to determine the conformation the bottom near the coast line. This was continued afterwards as far south as San Diego. The southern line was then

run, the ship returning by the great circle route to the northward. The results of this cruise, together with a description of the apparatus employed have been published by the Hydrographic Office, in a fully illustrated volume, to which you are referred for more complete information.

The Tuscarora was supplied with about fifty thousand fathoms of sounding lines. Of this, some fifty thousand fathoms was 1¼, 1½ and 1¾ inches Manilla rope, which had been treated with carbolic acid, after some patented process, with a view to its preservation. About five thousand fathoms was 1¼ inch whale line, and there was also four thousand or five thousand fathoms of Albacore line made of untarred hemp, ¾ inch in circumference. Fitted on the forecastle was a steam reel and a dynamometer, for use with this rope. The want of some instrument for regulating and measuring the tension of the line in sounding had long been felt. As the line runs out the tension is continually increased by the weight of the increasing length overboard, until it is suddenly diminished by the sinker resting on the bottom. If then the tension could be measured at every instant, any sudden diminution of the strain would be an indication that bottom was reached. The dynamometer, designed by Passed-Assistant Engineer T. W. Rae, was for that purpose. It consisted essentially of two fixed pulleys, elevated several feet above the deck, midway between which was a third pulley attached to a rod which was capable of motion in a vertical direction, through guides which it traversed freely. The lower end of this rod, which passed through the deck, terminated in a piston, which worked in a cylinder filled with water. The sounding line passed from the drum of the steam reel over one of the fixed pulleys, under the movable pulley, which was thus made to ride upon the line, over the second fixed pulley, and thence, by means of a fair leader, over the ship's side. The rod attached to the movable pulley could be weighted and by means of a scale behind the rod the strain upon the line could be determined. By gradually increasing the weights on the rod as the line ran out, it was intended to keep such a strain on the line, that it would cease running, or nearly so, when the sinker rested on the bottom. The object of the piston in the cylinder filled with water was to prevent violent motion of the rod, when the ship was rising or falling with the sea. This machine, although constructed upon perfect mechanical principles, and notwithstanding the fact that similar instruments are used with accuracy in the laying of submarine cables, gave hardly satisfactory results. It was found that the oscillations of the rod were violent and incontrollable when there was any rolling motion, and it was impossible to estimate with any degree of accuracy the tension of the line. It is possible that a more thorough acquaintance with this apparatus would make it valuable in sounding with rope. By an unfortunate accident it was not properly arranged in the Tuscarora, and hence did not obtain a very thorough test of its value, before the soundings with wire become so successful that its employment was unnecessary.

We have seen with what success the attempt to sound with wire had been attended in the Taney. Walsh's failure, together with that of other efforts made at about the same time, led to the conclusion that wire soundings were impracticable. Nevertheless that material offered great advantages and possessed the very qualities which experience had shown to be desirable in a sounding line. The small, smooth wire meeting with but little resistance from the water in descending, would sink rapidly, nor would it be deflected to any great extent by submarine currents; it would require a sinker of comparatively little weight; it could be made sufficiently strong to bear any ordinary strain; it was compact and portable, and would occupy but little room on board ship, a consideration which only those who have been embarked with great quantities of sounding rope, can properly appreciate. There were those, therefore, who did not accept the verdict rendered upon the evidence of former trials. All that was required to render the method practicable, it was maintained, was some contrivance for so regulating the strain upon the wire, that when the sinker reached the bottom, the wire should no longer run out, or should do so with such a diminished

velocity as to be readily perceptible. This was the object of a machine invented by Sir Wm. Thomson, in 1872, for sounding with steel piano wire.

One of these machines, with a supply of wire, was furnished to the Tuscarora. At that time but a single cast had been made with the apparatus, and that by the inventor, who sounded in twenty-seven hundred fathoms from a schooner yacht in the Bay of Biscay. The cast had not been satisfactory; the reel was crushed in the operation, and it was with great difficulty that the wire was hauled in. The original machine differed but little from that furnished to the Tuscarora, so that the method was almost absolutely untried when it was placed in Capt. Belknap's hands for experiment. The weight of opinion both in this country and in England was against the method. When the Challenger was fitting out in 1872, it was reported that she would be furnished with the machine. But she went to sea without it, the reason being, according to Sir Wm. Thomson, that "innovation is very distasteful to sailors." Prof. Wyville Thomson, who was the director of the scientific staff of the Challenger expedition, in a paper read before the Asiatic Society of Japan, at Yokohama, said, "When we started from England this wire had only been tried once. . . . I had been some years at sea and my colleagues were all sailors, so we had great sympathy with hemp." According to Sir William, the British Admiralty would not try the wire method because it was new. He states that he received a semi-official letter to the effect: "When you have perfected your apparatus we may be willing to give it a trial."

Whether or not it was sailor's prejudice that opposed the use of wire in this country, it is certain that few had any faith in its success. Fortunately, however, among the few, was the Chief of the Bureau of Navigation, Commodore Ammen. It was his determination in the matter that enabled the Tuscarora to put the method to the test. He facilitated the preparation of the ship in every way; he ordered Capt Belknap to make an experimental cruise to detect defects in his apparatus, which was the foundation of future success; and it is to his aid and counsel and constant interest throughout the work, supplemented by the ingenuity of Capt. Belknap, that the Navy owes its prestige in having made wire soundings practicable.

Thomson's machine as furnished to the Tuscarora, consisted of a reel for holding the wire, and an arrangement for regulating and measuring its tension. The reel was a hollow cylinder of galvanized sheet iron, with an iron axle passing through its center, and soldered to its sides. The drum of the reel was about six feet in circumference and three inches long. The ends extended two inches beyond the circumference of the drum, thus forming an annular space two inches deep and three inches wide in which the wire was wound. To one side of the drum was fixed a projecting ring of galvanized sheet iron, which formed with the side of the reel a V shaped groove in which an endless rope passed. The axle of the reel was a small iron shaft, six or eight inches long, which revolved in bearings on two iron standards, bolted to a plank of hard wood $3\frac{1}{4}$ ft. long, 15 inches wide and $2\frac{1}{2}$ inches thick. The shaft carried on one side of the drum an endless screw, which gave motion to a train of wheel work by which the revolutions of the reel were counted; on the other side was attached a ratchet in which worked a pawl by which the revolutions of the reel was prevented when necessary. Both ends of the shaft projected beyond its bearings and were squared, so that cranks might be applied in putting the wire on the reel.

The arrangement for controlling the tension of the wire consisted of a grooved friction wheel of iron ten inches in diameter, the groove being wide enough to carry two parts of the endless rope passing around the reel. It was capable o motion in a vertical plane, in an iron crotch fixed to the bed of the machine, but was prevented from turning by a cord passing from the lower part of its circumference to the dynamometer. The friction wheel was connected with the reel, when the wire was running out, by means of an endless rope of 9 thread untarred hemp, which passed around the V groove of the reel, then up, over, and once around the friction wheel, and then around a pulley placed several feet in the rear of the reel. This pulley

was attached to a small tackle by means of which the endless rope could be made more or less taut, thus increasing or diminishing the resistance of the friction wheel The tackle was shortly afterwards replaced by a pendant rove through a tail block, and carrying hooks to which weights could be attached, an arrangement of much greater utility.

The dynamometer was an instrument similar to one form of the spring balance, secured to the bed of the machine. The force exerted upon it, which depended upon the tension of the wire, was indicated in pounds, by a pointer which moved over a graduated scale. For this dynamometer was afterward substituted an ordinary spiral spring balance, which gave more satisfactory results.

The wire furnished for this machine was steel piano wire, No. 22, B. W. G., of the best English make. Its weight in air was about 14 lbs., and in water 12 lbs., to the thousand fathoms. It would support a weight of 230 lbs. It was supplied in lengths of from two hundred to four hundred fathoms which were spliced together on board ship. One great objection which had been urged against the use of wire was the impossibility of making the splices strong enough. The method of splicing adopted on board the Tuscarora was to lap the ends about two feet, solder one end and lay the other end up in turns of about an inch in length until it was all expended, when the second end was soldered. The two parts of the splice were also soldered together at intermediate points, and the whole splice served with well waxed twine. The splice thus formed was very strong, not one of them having broken down during the cruise. The wire was received packed in sperm oil in cases of sheet tin; it was kept in these cases covered with oil until wound upon the reel for use. The operation of winding was one requiring the utmost care, the wire having a great tendency to kink. When, by accident, a kink did occur it was found best, as a rule, to break the wire and splice the ends. In winding, the coil of wire was taken from the oil and slipped over the end of a wooden reel, from which it was wound on the sounding reel, being kept hand taut all the time. It was carefully measured as it was wound, the lengths between the splices, as well as the number of revolutions of the drum, being noted at each splice. These were recorded in a note-book and by them the depth was determined when a cast was made. The reel held readily between four thousand and five thousand fathoms, and this quantity was usually wound upon it. When it was all on, to the free end of the wire was attached a small grommet made of 1½ or 2 inch rope. The grommet was secured by sticking the end of the wire between the strands of the rope and then taking several round turns against the lay, the whole being finally served. A piece of cod line attached to the grommet and tied around the reel prevented the wire from unwinding.

The required length of wire being wound, the reel was unshipped and placed in a galvanized iron tank containing a solution of caustic soda, which served to protect the wire from rust. The philosophy of this method of preservation, as explained by Sir Wm. Thomson, is as follows: "The preserving effect of alkali upon steel is well known to chemists. It seems to be due to the alkali neutralizing the carbonic acid in the water, for the presence of carbonic acid in water is the great cause of iron being corroded. The fact is well established that iron will remain perfectly bright in sea-water rendered alkaline by a little quicklime. Caustic soda is a more sure material, because with it we can make more certain that the water is really alkaline. * * * All that is necessary in order to make sure that the pickle will be a thorough preserver of the wire is that it should be found to be alkaline when tested with the ordinary litmus test paper."

I give Sir William's language as nearly as may be because I do not think he estimates at its true value the action by which the wire is preserved from rust. In the Tuscarora the lye in which we kept the wire was much more strongly alkaline than he says is necessary. It was found, however, that while the wire was perfectly preserved, the caustic soda solution attacked the zinc of the galvanized iron of which the reel was made, as well as the soldered splices. Now zinc and iron, or iron and tin solder, when placed in contact in caustic soda

form a galvanic couple of which the zinc or the tin solder is the electro-positive element. A galvanic action is, therefore, set up by which the iron is preserved at the expense of the zinc or tin. It is probable that this action would be less with a more dilute solution of soda than with a stronger one, but still great enough to protect the sounding wire from rust. This, I think, is the true explanation of the preservative action. If so, it seems to me that a more suitable liquid than soda-lye might be substituted for it. Caustic-soda is a very disagreeable substance to use on board ship. It spoils the clothes and hurts the hands of those working with the wire; and the lye, washing over the sides of the tank in the rolling of the ship, kills the wood of the deck, and then running through the scuppers takes the paint off the ship's side. I am inclined to think that slightly acidulated fresh water, or even sea-water alone might be substituted for it with advantage. So serious has the objection to the use of soda become that it has been discarded in English vessels using the wire, and sperm oil is now used in its stead.

The Tuscarora's soundings were always made under steam and with the ship stern to wind. This was found to be the best method of laying the ship and it was resorted to even when the force of the wind was as great as 8. Usually the screw held the stern of the ship up to the wind, but when the bows showed a tendency to fall off to one side or the other, which was the case only when the wind was light, the jib being set with the sheet hauled flat aft effectually prevented it. After numerous experiments Capt. Belknap fixed upon the gangway as the most convenient point from which to sound. There the motion of the ship was least sensible, the wire was consequently more manageable, and accordingly nearly all the soundings were made there. A bridge across the deck was constructed, so arranged that it could be unshipped and stowed away in port. The machine was placed upon a slide so that it could be run out or in and compressors were applied to secure it at any point. The upper grating of the accommodation ladder being shipped, this slide was supported by a railing and securely lashed. The grating allowed room to the men who were handling the sinker, &c., and served to carry the wire well out from the ship's side.

In preparing to sound, the reel was placed on its bearings at the gangway, and the bed run out to the end of the slide. The endless rope was arranged as already described. To the grommet was hitched a piece of Albacore line, twenty-five fathoms in length, and this was also wound on the reel. The other end of the Albacore line carried a small iron rod six feet long, to which was seized the upper end of the swivel link of the Brooke detaching apparatus. The Albacore line was to prevent the wire itself from going to the bottom, thus avoiding all danger of kinking, and the rod was for the purpose of throwing the line clear of the specimen cylinder so as to prevent fouling. The sinker which was usually an 8 in. shot weighing 55 lbs., was placed on the apparatus for obtaining the bottom specimen, an invention of Capt. Belknap, shortly to be described. When all was ready the sinker was eased down by hand into the water, and a Miller-Cassella deep-sea thermometer, for obtaining the bottom temperature, was attached to the stray line above the iron rod. The stray line was then allowed to run out slowly until the grommet in the end of the wire was reached. To this was attached a lead weighing four pounds, which prevented the end of the wire from flying up and kinking when the sinker reached the bottom, as experience had shown it would sometimes do. Weights were now hooked to the pendant carrying the pulley around which the endless rope passed, and the wire was allowed to run out. When it had fairly started, the weight on the pendant was diminished so as to allow it to run more rapidly. The time the wire started was noted, as well as the instant at which the drum completed each hundred revolutions. When it was thought that the sinker was nearing the bottom, the weights on the pendant were increased in order to diminish the speed of the reel and to make sure that the instant of reaching the bottom should be properly indicated. This indication was usually unmistakeable; the pointer of the dynamometer would fly back on the scale, and, except

in very deep water, the revolution of the drum would almost instantly cease. The fact that bottom had been reached could be noted as well from the poop or the forecastle as at the gangway.

Bottom having been found, the cord holding the friction wheel was detached from the dynamometer so that the endless rope might be employed in reeling up the wire. The officer in charge of the sounding then laid hold of the endless rope and, unaided, hauled in a few fathoms to make sure that the shot had been slipped. If not, as was the case in but few instances in the early part of the cruise, fifty or sixty fathoms were reeled in and again let go, the second effect almost invariably detaching the shot. The reeling in was at first done by putting men on the bridge who hauled in hand over hand on the endless rope, an operation both tedious and laborious. For this arrangement was afterwards substituted a fly-wheel, carrying a grooved disk of wood from which a belt passed to the V groove of the reel. By turning the fly-wheel by means of long cranks, which could be manned by four or six men the wire was wound in much more rapidly and uniformly than by the old system. When reeling in the wire was guided fair on the reel by two petty officers, round sticks being used for the purpose.

In Brooke's original apparatus the device for obtaining specimens of the bottom consisted of a number of open quills placed in the lower end of the sounding rod. This arrangement, however, did not secure adequate samples, and the ingenuity of those engaged in deep sea work has ever since been directed toward its improvement. Berryman soon replaced the quills with a valved cup, which gave much better results. Many subsequent improvements in the form of the cup have been made, not a few of which have been by officers of the Navy. In the Challenger, the apparatus usually employed was the "Hydra," so called because it was invented on board an English surveying vessel of that name. The Hydra consists of a long cylinder of brass, closed at its lower end by a valve opening upwards. An iron piston-like rod, carrying a short arm projecting at right angles to the rod, is fitted into the upper end of the cylinder. Over the projecting arm is a curved steel spring, one end of which is fast to the rod, the other end movable. This may be pressed flat against the rod, a hole in the spring allowing the projecting arm to pass through it. The sinker is supported on the rod by a wire sling which passes over the arm and is kept there in opposition to the spring, by the weight of the sinker. When the sinker rests on the bottom the sling is pushed off the projecting arm by the spring, and the rod and cylinder are hauled up. The piston-like arrangement of the rod consists in closing the valve in the lower end of the cylinder.

This apparatus has been used very successfully in the Porcupine, and was highly thought of in the Challenger. Its weight, however, made it objectionable for use with wire, and it necessitated the use of a separate instrument, the water-bottle, when samples of the bottom water were required. In order to overcome these objections several new forms were devised by Capt. Belknap. Of these the most important and most successful are those designated as specimen cylinders, Nos. 1, 2 and 3. In each of these the Brooke plan of detaching is adhered to.

Cylinder No. 1 consists of two cylinders, and the inside diameter of the one being very slightly greater than the outside diameter of the other, so that the larger slides over the smaller with but little friction. The inside cylinder terminates in a cone, above which, on two opposite sides, are openings, by which the bottom mud or sand can enter. The upper end of this cylinder screws on the iron detaching rod. The outside cylinder travels on this rod by means of an opening in its top. It is long enough to go entirely through the shot used as a sinker, and, when in its lowest position, it covers the openings in the inner cylinder. A stud on one side prevents its slipping through the shot. The inner cylinder has in its upper part a chamber, to the top and bottom of which are fitted valves, opening upward, intended to enclose a specimen of the bottom water. In slinging the sinker this apparatus is passed through a hole in the shot, and a metal washer is put on, which by means of wire slings is suspended to the detaching arm of the rod. When the shot

rests on the bottom, the detaching arm falls, the outer cylinder slips down and retains whatever has entered the inside cylinder.

Cylinder No. 2 is of quite different design. To the lower end and inside of the cylinder is fitted a hollow frustum of a cone, its base downward. The upper end of the cone is closed by a valve, kept in place by its own weight in addition to a light spiral spring. Into the bottom of the valve is screwed a plunger extending beyond the end of the cylinder. When the shot strikes the bottom, the valve is forced open and the mud or sand enters. The valve then closes, retaining the specimen. This cylinder is also fitted with a water space.

Cylinder No. 3 consists of an auger-shaped piece of iron, over which slides a brass cylinder. The brass cylinder is kept up by a stud on its side, until the shot is detached. The auger-shaped iron engages the bottom specimen which is retained by the cylinder. In the use of this cylinder it was sometimes found that the metal washer which supported the shot was caught by the cylinder in falling, and prevented the shot from slipping off. This was remedied by lacing on the shot two wire grommets of smaller diameter than the shot, to which the slings were attached.

Cylinder No. 1 was most successful in soft bottom, at moderate depths. No. 2 brought up the best specimen in hard, sandy bottom, and was always good. No. 3 answered best at great depths, when the shot on striking the bottom was not moving with great velocity.

In depths less than two hundred fathoms the sinker used was an 8-inch shot, weighing about 55 lbs. In deeper water one or more lead castings, which fitted over the top of the shot, were added, thus increasing the weight to 70 or 75 lbs., and making the total weight sent down, including the specimen cylinder and the small safety lead, about 80 lbs.

The time required for a sounding with this apparatus is much less than that required for rope, even when the rope is reeled in by steam. The deepest sounding made by the Porcupine in 1869 and '70 was the one of twenty-four hundred and thirty-five fathoms, a description of which I have read. The sinker used on that occasion weighed 336 lbs. The time required for the descent of the line was 33½ minutes, and in hauling in, 2 hrs. 2 min., or 2 hrs. 35 minutes for the cast. In the Tuscarora we sounded in twenty-five hundred and sixty-five fathoms, the line running out in 31 minutes, and being reeled in in 40 minutes, the whole time occupied being but 1 hr. 11 min., a gain of 1 hr. and 24 minutes, although the depth was over a hundred fathoms greater. This is a very fair example of the speed with which a wire sounding could be made. A cast of three thousand fathoms usually required an hour and a half, and one in four thousand could be made in about two hours.

The Tuscarora ran lines of soundings aggregating sixteen thousand six hundred and twenty miles in length, and made in all four hundred and eighty-three casts, of which perhaps fifty, in depths generally less than five hundred fathoms, were made with rope. The depth found in one hundred and sixty of the casts was over two thousand fathoms; thirty-two were in depths of more than three thousand, and nine, in depths of more than four thousand fathoms. The greatest depth from which a bottom specimen was obtained was four thousand three hundred and fifty-six fathoms. The deepest sounding made was in four thousand six hundred and fifty-five fathoms, or 5¼ statute miles. The circumstances under which this cast was made were all the most favorable. The sea was smooth, the ship steady, and the up and down direction of the line was maintained throughout. The indication of the dynamometer that bottom had been reached was perfect. But the wire had been down four thousand fathoms three or four times on the preceding day, and it finally gave way under the strain when about four hundred fathoms had been reeled in. This was the fifth and last time during the cruise that the wire was lost.

The soundings with the wire were invariably made by the navigator of the ship, Lieut. Geo. A. Norris, and it was to his industry and intelligence that their success was in a large measure due. To Capt. Belknap, however, we owe the perfection which this method has attained. He was indefatigable in the

work and every cast was made under his direct supervision. Both Commodore Ammen and Sir Wm. Thomson were, naturally, much gratified at the results of the cruise. The latter has repeatedly publicly referred to the fact that while the British Admiralty hesitated, the American Navy perfected his apparatus in its own way. In his presidential address before the Section of Physics of the British Association, he speaks enthusiastically "of Commander Belknap and his great exploration of the Pacific depths by piano-forte wire, with imperfect apparatus supplied from Glasgow, out of which he forced a success in his own way, * * * and of the admirable official spirit which makes such men and such doings possible in the United States Naval service."

The great difficulty that was experienced in the Tuscarora, and which the resources of the ship could not overcome was the crushing of the reel, especially when the soundings were in deep water. The steel wire stretching under the strain to which it was subjected, was reeled tightly on the drum. The crushing force thus brought upon the drum by the elasticity of the wire was very great, and the drum, being made as light as possible in order to keep its inertia small, invariably broke down. A drum would stand, perhaps, a dozen casts in depths below two thousand fathoms, but when the depth was much greater the life of a drum was much less. The reels on board the Tuscarora were constantly in the hand of the tinsmith. They were strengthened by radical pieces of wood, placed inside of them, but the crushing went on almost as fast as though the wood were not there. Other devices were resorted to but unavailingly, and the only way of keeping at work was to shift the wire from reel to reel as they broke down. Capt. Belknap recommended a steel drum for deep soundings, but I do not know that any have been constructed. Sir Wm. Thomson has overcome this difficulty by the introduction of an auxiliary wheel in reeling in.

The bed of the machine, rests on two rails so that it can be run out or in, and clutches at either end of the bed keep it on the rails. The galvanized sheet iron reel is retained, and the soundings are made directly from it as before. Under the rails and midway between them are two pulleys, and one of which projects over the taffrail, supposing the sounding to be made from the stern as Prof. Thomson prefers. This outermost pulley is called the "castor pulley" from the manner in which it is mounted. Its bearings are in an oblique fork which turns about a horizontal axis, like the *castor* of a piece of furniture laid on its side. The other pulley is inboard. The two bearings of its axle are both on the same side of the pulley so that a turn or two of the wire can be taken around it. The ends of its axle are squared so that handles may be applied in reeling in, or a wheel having a sharp V groove can be shipped, carrying an endless rope by which the reeling in is done. When the sounding has been made the wire is stopped and the reel run in about five feet on the slide, until it is just over the auxiliary pulley. The wire is then led over the castor pulley and a turn, or two turns, are taken around the auxiliary pulley. The reeling in is performed by the auxiliary pulley which takes from two-thirds to nine-tenths of the strain off the wire before it reaches the sounding reel on which it is wound as the reeling in proceeds. The castor pulley, by turning about its horizontal axis, in case the ship drifts during the operation, prevents the wire from leaving the groove; it performs the same office if the ship is rolling heavily. Additional security is obtained by a guard to catch the wire if it should get off the pulley.

In the machine, as thus modified, the inventor has introduced a friction arrangement different from that used in the Tuscarora, and has abandoned the spring dynamometer. A rope fastened to the bed of the machine passes over the V groove of the reel and then over two small pulleys. To the end of this rope weights are applied. These are added as the wire runs out so that the friction brings the drum to rest as soon as the sinker touches the bottom. The rule is to apply a resistance greater by ten pounds than the weight of the wire out. This makes the moving force on the sinker ten pounds less than its weight in water and a sinker of thirty-five pounds is thought to be sufficient in depths less than three thousand fathoms.

The machine, as thus modified, was used in the cable ship Hooper in laying the submarine cables on the coast of Brazil; it was also used in the Faraday, in laying the direct Atlantic Cable. In these ships the sinker was always recovered, and Sir Wm. Thomson recommends that this should be done whenever the depth is not over three thousand five hundred or three thousand fathoms.

One difficulty remained to be overcome. When the ship is lifting in a heavy sea there are times when a very great strain is imposed upon the wire and when it would be dangerous to haul it in too fast. Prof. Jenkin, of the University of Edinburgh, who accompanied Sir Wm. Thomson in the cable laying expeditions, has invented an arrangement by which the hauling in may go on as rapidly as possible, without bringing more than a certain safe strain upon the wire. The wire will come in fast when the strain is easy, and not come in at all when it is too great. By this arrangement steam can be applied to reeling in the wire. I regret that I do not know the details of this exceedingly important device.

A most valuable and ingenious improvment in the Thomson sounding machine has been made by Lieut.-Commander Sigsbee, United States Navy, who has been engaged in sounding in the Coast Survey steamer Blake. In the Tuscarora, whenever the ship was rolling during a sounding, the revolution of the reel would almost cease when the ship rolled toward the wire; on the other hand when the roll was in the opposite direction the reel would move so rapidly that it would have thrown the wire off, if the motion had not been controlled. This was done by pressing with the hand on the endless rope, but this brought an undue strain upon the wire. Sigsbee's invention controls the motion of the machine automatically.

One either side of the bed of the machine, at its outer end, is an upright stanchion. Between the two stanchions is a horizontal bar which moves up and down in slots or scores in the uprights. A fixed cross-piece joins the upper ends of the two uprights, and the movable bar is connected with the cross-piece by two spiral springs. Attached to the under side of the movable bar is a pulley. To the strap of this pulley is secured one end of the brake rope. This is led around a small pulley, on the bed of the machine, then carried around the V groove of the sounding reel, and its end made fast to a small eye bolt. When there is no downward strain on the bar the springs cause the rope to press tightly against the reel, and keep it from revolving. The wire, instead of passing directly from the reel into the water, is led over the pulley attached to the movable bar. Consequently when considerable strain is on the wire the springs are stretched, the brake rope is slackened and the reel may turn freely. Any tendency of the reel to pay out the wire more rapidly than the sinker will take it, is attended by a diminution of the strain on the wire; the springs contract and the brake-rope is applied to the reel. The changes in the tension of the wire being thus self-regulating it is evident that no dangerous strain can be brought suddenly upon it.

Although not strictly within the scope of my paper, I have thought it well, at this point, to speak of the most recent application of the piano wire in sounding. I refer to its substitution for rope in sounding in shoaler water for the ordinary purposes of navigation. The uncertain process hitherto adopted, involving the necessity of stopping or heaving-to the ship, and depending upon the shrewdness in guessing of the quartermaster, is no longer necessary. In laying the telegraph cables on the coast of Brazil, Sir Wm. Thomson used the ordinary deep-sea machine in making approximate soundings in depths of from fifty to one hundred and fifty fathoms while the ship was running at the rate of four or five knots. The depths in these cases were arrived at by noting the length of wire out, and knowing the distance run by the ship while it was going out, and were therefore only approximations. He has, however, recently modified his apparatus so as to make it available for "flying soundings," and they have been made with great accuracy. The reel used for this purpose is of the same form as that for deep soundings, but it is only twelve inches in diameter and holds only three hundred or four hundred fathoms of wire. The brake arrangement differs somewhat in form

from that of the larger machine. As soon as bottom is reached, which is shown by a sudden decrease in the speed of the drum, a brake is applied and prevents any more wire running out. A counter is used to show the number of revolutions of the drum. The wire is reeled in by applying cranks, worked by two men, to the axle of the drum.

The sinker is a slender cylinder of iron, three feet long, weighing twenty-two pounds, attached to the wire by a fathom and a half of log line. The depth is determined by the compression of a column of air contained in a glass tube, closed at one end, two feet long and about three-eighths inch internal diameter. The tube is coated on the inside with a colored substance, which is discolored by contact with sea-water. The glass tube, to secure it from breaking, is inserted with its open end downward in a slightly larger tube of brass, made fast to the log-line above the sinker. As the sinker descends, the pressure of the water reduces the volume of the air in accordance with Boyle's law, the water rises in the tube, and the height to which it rises is marked by the discoloration of the chemical preparation within the tube. A graduated scale is applied to the tube by which the depth in fathoms is determined by the extent of the discoloration.

There is no reason why such an apparatus should not be carried on board every ship. The frame of the machine could be secured to the taffrail when the ship is on soundings. "Two men," says Sir Wm. Thomson, "can take a cast with ease every quarter of an hour." It is not necessary to stop the vessel or heave her to in making a cast. The depth indicated by the tube depends only on the vertical distance descended and is independent of the inclination of the wire. Nor is it necessary to use the chemically prepared tube each time. If the speed of the ship be uniform, the reading of the counter when the tube is used will show the relation between the depth and the length of wire out, and the succeeding three or four casts could be estimated entirely by the counter. This apparatus has been used successfully in H. M. S. Minotaur when the ship was going ten knots, and t is reported that a sounding bas been made with it from one of the White Star Steamers when the speed was sixteen knots.

The Tuscarora having demonstrated the feasibility of the wire method, the art of deep-sea sounding has been revolutionized. The days of rope soundings terminated last year with the cruise of the Challenger. With the exception of those made in that ship every deep sounding within the last three years has been made with wire. The inconsiderable labor necessary in making a caste, the short time required for the purpose, the accuracy of the result, are such that, henceforth, the bed of the ocean lies but at our hand. "International coöperation alone is necessary in order that the principal features of the bottom of the sea may be mapped out with almost the same accuracy as that with which the geographer now depicts the land surface." In every step toward this wonderful result—wonderful, when we consider the few years in which it has been accomplished—those to whom we are united by ties of official brotherhood have largely contributed. It is to preserve the recollection of this fact that I present this imperfect record of their achievements.

The range of the changes of level in the rivers of Russia in Europe has become, since 1876, the subject of accurate measurements, and M. Tillo has just published an interesting paper on this subject, being the result of measurements made at eighty different places. The highest range is reached by the Oka at Kaluga, the difference between the highest and lowest levels being as much as 45 ft.; the average range for the same river from its source to its mouth being 32.2 ft.; the average for the Volga from its source to its mouth is 33.6 ft., 30.1 ft. for the Kama, 25.2 ft. for the Duna, and 23.1 ft. for the Don. For all other rivers the range is less than 20 ft. Of course this range diminishes very much towards the mouth of each river; but still it reaches 12 ft. for the Volga at Astrakhan, and 9 ft. for the Duna at Riga. The highest range observed in the lakes of Northern Russia was only 2.1 ft. A map prepared by M. Tillo shows the distribution of hydrometrical stations on Russian rivers, their numbers having been increased in 1880 to 341 stations.

SILT MOVEMENT BY THE MISSISSIPPI—ITS VOLUME, CAUSE AND CONDITIONS.

By ROBT. E. McMATH, Member of the Engineers' Club of St. Louis.

Journal of the Association of Engineering Societies.

The volume of solid matter borne by rivers, the mode of its conveyance and the conditions of deposit and removal are topics of interest to the engineer who has to do with works at the margin or in the bed of silt-conveying streams. These topics have special interest at the present time, because of their bearing upon the improvement of the Mississippi.

I propose to state in brief the result of my personal study or observations made under my immediate charge, as assistant to Gen. J. H. Simpson, U. S. Engineers, adding data from other sources when needed to complete or support a conclusion.

The observations were made under favorable conditions, using apparatus of improved design. The locality chosen was a few hundred feet above the foot of Grand avenue, St. Louis, where the width and sectional area of the river approximated a mean value.

Samples of 100 grammes each were taken daily, 1 foot below surface, at mid depth, and 1 foot above bottom, at positions dividing the width into 8 parts—making 23 daily samples. The daily samples from each position were combined for periods of 6 and 3 days, as shown in the table on the opposite page.

Perhaps a better apprehension of the amount of turpidity may be gained by remembering that 1,000 parts in 1,000,000 by weight is very nearly one ounce of solid mtter to a cubic foot of water. To reduce to ounces per cubic foot cut off three decimal places in the column headed "sediment."

Specimens were weighed dry after continued exposure to a temperature of 180° F. A cubic yard of compacted sediment was taken at 1.6 tons (3,200 lbs.).

During the 87 days between March 31 and June 25, 1879, 62,363.009 cubic yards of solid matter passed the observation station in suspension. A quantity sufficient to cover a square mile to a depth of 60$\frac{4}{10}$ feet.

Observations were made the same season by Maj. C. R. Suter to determine the volume of solid matter borne by the Missouri River near St. Charles. The general results have been reported as

		Cubic yards per day.
Mean volume	for the year 1879....	475,457
" "	" June and July..	1,755,423
Maximum "	on July 3	4,118,600

The stage at St. Louis for June ranged from 13.2 to 21.0; mean, 16.6. For July the range was 21.0 to 16.1; mean, 18.0. The "danger line," or beginning of damaging overflow, corresponds to a stage of 30 feet. Extreme known flood to 41.38.

The observations, therefore, do not include a flood or even what would be called a high stage.

Observations for 250 days at Fulton, Tenn., made by the Mississippi River Commission, gave a maximum of 3,232,209 cubic yards on July 10, 1880. The aggregate for the period January 1 to October 8, 1880, was 217,728,125 cubic yards, or sufficient to cover a square mile to the depth of 210$\frac{8}{10}$ feet. The minimum discharge for any day at Fulton was 136,458 cubic yards, confirming the minimum observed at St. Louis.

The comparatively small volume of sediment at low stages is of itself sufficient proof that important changes, by way of deposit in the bed, cannot occur at low stages except as a result of local movements.

Considering the moving mass as among the material resources of the engineer on one hand, or as a possible opposing force on the other, it appears from the foregoing figures that at one season the quantity is scant and the forces feeble, at another the quantity is enormous and the forces overpowering unless skillfully handled. It is readily seen how necessary to an intelligent dealing with the river is a thorough knowledge of how, when and why the silt movement occurs.

If one examines the material found in suspension he will discover that a consid-

Dates.	Stage.		Sediment Parts in 1,000,000.					Mean velocity of river. Ft. per sec.	Quantity of sediment in cubic yds. Per day.
			Mean of all stations.			Near sides.			
	Range.	Mean.	Surface.	Mid. depth.	Bottom.	Mo ordinate 281′.	Ill. ordinate 1291′.		
1879.	Ft.	Ft.							
March 31–April 5.	11.0 12.5	11.9	533	493	581	1003	270	4.34	138,182
April 7 " 12.	12.3 17.3	14.5	1998	2363	2398	3100	1173	4.58	782,434
" 14 " 19.	18.0 14.9	16.5	3601	3731	3809	4200	3400	6.13	1,555,646
" 21 " 26.	14.0 12.9	13.4	1581	1657	1814	2190	1298	5.20	449,985
" 28 " 30.	12.6 12.3	12.5	1509	1568	1577	2100	1017	4.93	396,585
May 1–May 3.	12.2 11.2	12.1	1351	1377	1378	1825	951	4.56	349,903
" 5 " 7.	11.8 11 6	11.7	996	1066	1096	1690	560	4.14	282,892
" 8 " 10.	11.8 12.1	11.95	1332	1378	1456	1951	945	4.00	343,475
" 12 " 14.	12.2 11.8	12.0	2014	2118	2166	2771	1605	4.14	508,521
" 15 " 17.	11.75 11.8	11.8	2267	2449	2467	2948	1973	4.07	559,457
" 19 " 21.	11.5 11.3	11.4	1408	1516	1639	2362	1082	4.12	360,335
" 22 " 24.	11.4 11.5	11.25	1556	1685	1793	2432	1297	4.11	391,404
" 26 " 28.	11.5 12.0	11.7	1226	1311	1646	2302	992	4.17	357,160
" 29 " 31.	12.3 12.9	12.6	1055	1232	1250	2408	653	4.55	378,960
June 2–June 4.	13.4 15.3	14.8	1274	1429	1530	3188	652	5.52	560,681
" 5 " 7.	15.5 15.9	15.7	1863	2213	2327	3453	1210	5 81	932,666
" 9 " 11.	16.5 16.0	16.3	1567	1791	2056	3103	1097	6.23	886,090
" 12 " 14.	16.0 16.1	16.0	1553	1765	1948	2878	1165	5.86	889,490
" 16 " 18.	16.3 16.4	16.3	2281	2596	2752	3245	2057	6.31	1,134,475
" 19 " 21.	17.0 17.7	17.3	3347	3651	3818	3882	3693	6.93	1,656,553
" 23 " 25.	17.9 17.5	17.8	4477	4796	4963	4425	4883	7.21	1,983,467

erable proportion is in a state of subdivision so minute that it separates from the water very slowly. The experiments of Col. Flad made in 1865 are in point. He found that of 1,000 parts of matter in suspension at the beginning of the experiment,

944.50 parts settled to the bottom in 1st 24 hrs.
22.35 " " " " " 2d 24 hrs.
2.92 " " " " " " 48 hrs.
30.23 remained in suspension at end of 96 hrs.

The practical problem of river engineering has little to do with this very fine matter. Its presence and transport is analagous to that of impalpable dust in the air and requires no comment.

Without drawing arbitrary lines of classification as to weight or size of particles transported, which may vary from impalpable dust to the rock fragment, occasionally rolled over by the current, we can recognize three grades or species of movement and material.

First. Some of the traveling material never loses contact with the bottom.

Second. Material which, though heavy and in grains of notable size, is detached for a time by some energetic impulse and describes a longer or shorter free path, moving in or with the surrounding water.

Third. Material which, once mingled with the water, remains in continuous suspension until it reaches the sea.

No measure of the first grade movement has yet been satisfactorily made, being so far as manifested by progress of sand waves, undistinguishable from movements of the second grade. It is probable that the ordinary impression of its amount is an exaggeration.

The muddy appearance of the river is mostly due to the fine or third-grade matter. The proportion of such matter present is in close accordance with the appearance. But appearances give slight clue to the more important second-grade movements below the surface, which are manifested to the ordinary observer only by their results in bars deposited or removed.

Recurring to the figures stated, at first thought the transportation of so great a mass seems to present an important dynamical question.

Mass transported is work done, and work absorbs force. Water is matter, and its transport in a running stream is work. We say readily that the force is gravity, the body falling.

A chip floating with the current is a solid body being transported; it is, however, but a representative of the volume of water it displaces, and its motion is due to the same cause. It is not moved by, but moves with the water. So far from its transport being at the expense of the stream, the quantity of motion (MV.) is increased by its presence. The like may be said if the solid be of the same density as water, and immersed at any point of the depth. The effect upon MV. is the true test; therefore, if a pebble be thrown into a stream the quantity of motion will be increased or diminished, as the direction of its projection is with or against the current. It is thus seen that transportation in a running stream is an incident to the state of suspension, immersion or flotation. The inquiry that interests us turns to the cause of suspension; or how bodies heavier than water can be sustained clear of the bottom for an indefinite time. If heavy bodies, when cast into the stream, always reached the bottom after an interval of time and distance, and remained there, the possible movement past any station would be measured by the contributions of matter from outside sources within a very moderate distance. But, since the quantity moving and the supply from local sources do not correspond, we are compelled to conclude, that a force exists within the stream capable of resisting the descent of injected solid bodies, so that transport of contributions is continuous, or else the force is capable of the more serious work of detaching such solids from the bed, or banks, and projecting them upwards even to the surface, or laterally to considerable distances. The facts demonstrate the existence of such an upward and lateral force.

Three hypotheses have been suggested to account for or define the force.

First Hypothesis. It is due to the differences of velocity with which contiguous layers or fillets of fluid move, the submerged or floating body being impelled toward the fillet of greatest velocity.

Second Hypothesis. It is due to a general vertical component of the stream's motion, by which the water at the bottom is continually being brought to the surface.

Third Hypothesis. The power of erosion and suspension is due to the irregular movements, which arise in all bodies of moving water, and are in complex proportion to the velocity of the stream and the character and condition, or, as we may say, to the accidents of the bed.

To consider these hypotheses would extend this paper beyond due limits. It is sufficient to say that the gradation of velocities in a vertical direction, so as to present a curve of velocity regularly increasing from bottom to surface, is not verified by observation. As the velocities in a vertical approach a regular curve silt conveyance ceases; and conversely, as silt movements increase the vertical curve of velocities becomes an irregularly waving or serrated line. I therefore reject the first hypothesis.

The second hypothesis, of a general upward tendency of water and immersed bodies, implies a converse proportion of

a downward movement in equal volume. Since movements in opposite directions cannot coexist in place and time, a division of the stream into ascending and descending areas is needed, or the movements must alternate at short intervals. In either case, the facts would come within the range of observation. Failure to detect the supposed movement in the general form required by the hypothesis leads to its rejection.

The third hypothesis differs from the second by a prefix only. It attributes suspension to irregularities of motion. We can conceive a stream flowing in a polished bed with a gliding, straightforward movement in every part, and such an idea underlies theoretical discussions in which fillets or layers are imagined. In real channels the roughness of bottom is sufficient to secure a thorough mingling of the waters; but the interior movements will be too feeble to sustain solid matter if the bed be smooth and the area large.

Irregular motions manifest themselves in boils, eddies and whirls. That boils bring mud as well as water to the surface is a matter of common observation. At a position where the normal quantity of sediment was, at surface 2.278, mid depth 2.427, and one foot above bottom 2.880 ounces per cubic foot, samples taken from a boil gave at surface 3.006, at 15 feet depth 3.755, and at 37 feet depth (one foot above bottom) 2.706 ounces to a cubic foot. Of course it is not certain that any except the surface specimen was taken from the heart of the boil, for the rising path must necessarily be guessed at in attempts to reach it at lower depths.

A boil originates at the bottom and may be caused by any obstruction which deflects the impinging current.

The irregularities of bottom which may deflect curents are numberless, and the deflections may be in all possible forward directions, and the impulses may have any degree of intensity within the limit of the stream's velocity. Of the whole number it is certain but a small proportion will have a direction and intensity that will bring them to the surface; but the number of boils appearing at the surface is probably in proportion to that of the unseen, so their visible increase may be taken as marking an increase of the forces and motions below.

In bringing forward this hypothesis nothing new is involved, for all more or less distinctly admit that the irregular motions share in the work. They who hold the theory of relative velocity in any form are forced to ascribe to the whirls and boils the mingling of material equal in size but differing in density, or of different sized grains of equal density at a given level; also the suspension of all matter whose density is greater than water above the axis of greatest velocity. This third hypothesis goes but one step further in ascribing to the irregular movements the whole work of suspension, upon the ground that a cause, known to exist wherever the fact to be accounted for occurs, and admitted to be efficient, must be considered the sole cause unless a coworking agency is known, or the cause is insufficient to produce the observed result. Observation readily detects whirls, boils and eddies in the act of bringing water and suspended material from the bottom to the surface, and laterally from the side to the center of the river. Observation has never detected any other cause incident to the flow of streams which produced these effects.

Once impressed by the fact that a body of moving water is an involved network of irregular movements or jets (so to speak), a person can easily understand that the power to separate solids from the bed and maintain them in suspension is dependent upon the direction and energy of the jets. The jets have their origin at irregularities of the bed, by which parts of the stream are deflected from their regular forward course. Their number and energy will ordinarily increase or diminish with the velocity of the stream. So, in a restricted sense, we may say that power of suspension varies with the velocity; but, since the relation is not direct, being through an intermediate agency, there are many exceptions to the apparently general rule that a quickened current takes up an additional load, and its converse that a check of the current will determine a deposit. If, for instance, the stream pours over a reef, with gentle backward and steep forward slope, there is in approaching the crest a quickening of velocity without increase of burden, for the crest is not worn down. In passing the crest the sensibly horizontal movement becomes a plunge, the for-

ward component of movement is materially reduced, the vertical is greatly increased, the deflected current impinges upon the bottom and does work in excavating a deep hole, expending upon that work a portion of its force. The reflected current returns toward the surface with an increased load of sediment which may be borne to a considerable distance before the subsidence of the agitations caused by the plunge will allow it to fall to the bottom. We thus see that the lessening of silt burden depends upon the manner of the slackening, rather than on the fact of slackened current. If the motion is diminished because of increased area without abrupt change of direction, deposit will ensue, but if the slackening be the effect of change in direction increased irregular motion is the first result, and with it increase of suspending power.

Erosion at the origin of a jet is the natural consequence of impact and reflection of currents if the bottom be yielding. Detached matter is kept in suspension by the interfering currents through a succession of impulses. The earlier and rising part of a jet, whether it be a mass projected by a momentary or a stream by a continuous impulse, is concentrated and energetic; later it becomes diffused and weak; therefore the resultant of impulses originating at the bottom is always upward.

We have now reached a more definite idea of the cause of suspension. It is a result of work done upon the bed and banks. The internal movements which prolong suspension are another form of work. Work may be done by a body whose motion is diminishing or increasing. It is, therefore, well to keep in mind the stricter definition of the cause of suspension, and consider it as associated with, rather than caused by velocity. Failure to observe the distinction will often lead to unsound reasoning, and to disappointment in results realized when projects are executed.

We have seen that transportation of silt (up to the point of impaired fluidity) is not at the expense of the stream's motion. The work of erosion and suspension is done by the stream, whose velocity must be diminished compared with flow under a like head in a smooth channel, but if the now yielding bed should suddenly become rigid the same, or even greater, force would be expended upon the obstructing roughness. Therefore, though suspension consumes a part of the stream's force, the velocity is not necessarily lessened beyond what it would be in the only alternative condition that can be considered, a rigid bed equally rough.

The burden of suspended matter is not the same for a given velocity when found at different places, nor at the same place at different times, because irregular movements increase or diminish under local and temporary conditions. The maximum load was found in nearly every observation several hundred feet nearer the Missouri shore than the line of greatest velocity; therefore equal velocities on either side of that line were attended by dissimilar silt burdens. It may be well to add, that in any river section the profiles of velocity and depths follow each other so closely that one may be taken as the reflection of the other; hence supposed variation of suspension with depth cannot account for the difference observed.

In attributing suspension of solid matter to irregular internal motions, advance is made from the somewhat vague idea of unsteady motion or pulsation of currents suggested by some observers to a more definite interlacing of stream lines, whose resultant is an upward thrust.

In a great river the inequalities of the bed act an important part in developing scouring power at the bed, and sustaining power throughout the extended mass of considerable depth and width. But this statement must not be understood as implying that roughness of channel would facilitate the transport of sewage matter. In the limited channel of a sewer or water conduit, the intermingling required to maintain suspension is sufficiently provided by contact with the comparatively large wet surface. Removal of deposits requires additional force.

Having now shown the volume approximately, and the immediate cause of silt movements, we are prepared to consider the consequences of variation in this cause, or in the ability of the river to carry its burden.

Clearly this variation may be due to season, comparing high and low stage, or to locality, comparing areas and form of cross section, also to the varying inclination of surface.

This division of the subject is directly practical, for a man's judgment, as to the merits of rival proposed methods of dealing with the Mississippi, will turn according as he accepts or rejects the following statements of fact, and the legitimate inference from them.

"The History of the St. Louis Bridge," by our fellow member, Prof. C. M. Woodward, records the fact that the river bed at the bridge site deepened in time of high water. The engineers, who have been engaged at Horsetail Bar, 10 miles below the bridge, have repeatedly stated that the bottom there rises during the flood and is cut out by the falling river.

These apparently conflicting statements put us upon the inquiry why opposite results should attend a high stage at the two localities?

The known variation of bed at Horsetail is ten feet, measured in the channel. That is to say, a channel 4 feet deep was found at lowest fall and winter stage. A moderate rise of 20 feet following, the river after falling 6 feet afforded a channel depth of but 8 feet, whereas 4 + 20 − 6 = 18, a difference of ten feet. The former channel was not only obliterated, but every square yard of area in a limited length of river, which was covered by water at the low stage and all the area then dry, was filled in during the rise to a height of at least 6 feet above the low-water surface of the preceding season.

The observed vertical variation of bed at the bridge site is said to have been 15 feet.

Observations made in 1878-1879 at a series of sections in front of St. Louis and below the bridge gave the following results:

Section.	Width.	Change in Area.			Change in Mean Depth.		
		Sept. 17 to Mar. 31.	Mar. 31 to May 10.	Sept. 17 to May 10.	Sept. 17 to Mar. 31.	Mar. 31 to May 10.	Sept. 17 to May 10.
	Feet.	Fill sq. ft	Fill sq. ft.	Fill sq. ft.	Ft.	Ft.	Ft.
Pine street	1,604	2,583	2,255	4,838	−1.61	−1.41	−3.02
Chestnut street	1,609	5,141	575	5,716	−3.45	−0 85	−3.80
Market "	1,650	3,306	1,091	4,397	−2.00	−0.61	−2.61
Walnut "	1,744	1,143	2,920	4,063	−0.65	−1.62	−2.27
Elm "	1,815	4,119	4,082	8,201	−2.26	−2.23	−4.49
Myrtle "	1,885	5,523	2,622	8,145	−2.91	−1.40	−3.31
Spruce "	1,942	4,520	4,546	9,066	−2.27	−2.32	−4 59
Almond "	1,982	3,695	2,907	6,602	−1.49	−1.46	−2.95
Poplar "	2,032	4,523	547	5,070	−2.17	−0.26	−2.43
Plum "	2,033	3,930	1,291	5,221	−1.69	−0.63	−2.32
Cedar "	1,958	5,792	− 327	5,465	−2.96	+0.17	−2.79
Mulberry "	1,893	1,978	5,206	7,184	−0.86	−3.06	−3.92
Pittsburgh Dike	1,547	3,180	8,268	11,448	−2.02	−5.18	−7.20
Means	1.822	3,802	2,768	6,573	−2.02	−1.56	−3.58

The stage, from September 1 to May 31, was by means of 10-day periods:

Days.	1878.				1879.				
	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Apr.	May.
1—10	10.9′	9.6′	9.8′	9.4′	8.0′	9.3′	7.6′	12.2′	11.9′
11—20	9.7′	10.0′	.′	8.2′	8 6′	8.9′	11.3′	16.4′	11.0′
21—30-1	9.2′	10.7′	.	6 3′	9.2′	7.5′	11.4′	13.1′	11.0′

The fill which occurred between September 17 and March 31 must have been made when the stage was low and the water comparatively clear. If the fill was interrupted by the rise in April, which reached an 18-foot stage, it was resumed and the apparent result is a continuous fill up to May 10. The reported scour at the

bridge by a flood involves a converse proposition of fill at some other stage, else the section would sink to bed rock and become permanent; this inference is fully supported by the behavior of the sections below the bridge, stated above.

The area of cross section at Pine street was 41,000 square feet, stage 9 feet; the area added by a 30-foot stage would be 38,000 square feet; total, 79,000 square feet. At Horsetail the river was 4,800 feet wide, and area at 9 foot stage less than 20,000 feet, there being scant 6 feet in the channel. The area which would be added by a 30-foot stage equals 103,000 square feet; total, 123,000. Comparing areas at the two localities we have for 9 foot stage at Pine street 41,000 square feet, at Horsetail 20,000; 30 foot stage at Pine street, 79,000 square feet, at Horsetail 123,000. Since mean velocities are inversely as the areas it follows that the current at low stage must be slack in front of St. Louis and swift at Horsetail; at high stage, on the other hand, it must be swift at St. Louis and comparatively slack at Horsetail; or else alternate fills and scours must occur at the two localities to equalize areas. Since the cause of suspension commonly varies with the velocity, fills and scours will occur, and a partial compensation of areas be made. Considering the material by which the compensation is effected, we see that material scoured by the flood current from the narrow section passes to the succeeding wide reach and is there deposited; at a low stage the area in the wide reach becomes deficient, velocity increases and scour ensues; the material is borne only to the succeeding and sluggish pool.

I have presented the facts in front of St. Louis and at Horsetail at large because they are, when taken together, typical of what does and must take place elsewhere wherever like contrasting conditions occur.

The movement of coarse, heavy material is now seen to be a succession of steps; out of the pool into the wide reach in time of flood, and out of the wide reach into the succeeding pool during the subsequent low stage, with a period of rest intervening for individual particles, but a continuity of movement in the aggregate.

It has been thought by some that deposit cannot occur if the velocity be greater than that usually believed to be capable of moving a given material. And, since the velocity everywhere at high stages exceeds the supposed limit required by sand, it is argued that flood time is a period of universal scour, and the deposits observed to occur must be made at the turn and during the falling stage. In answer, it may be said that the change of velocity at transition from rising to falling stage is much less than occurs from variation of section at succeeding localities at high and low stages indifferently. But it will more effectually dispose of this idea to ask where does the material eroded by a flood go to, if scour be general and several feet in depth? And, if fill be general at falling stage, where does the material come from? The alternate movement between pool and shoal alone meets this difficulty about source and storage of material.

Summing up the results of the study it appears:

1st. The cause of suspension is found in the irregularities of motion resulting from work done by the stream upon the bed and banks. These irregularities in general vary with the velocity of current and accidents of bed and bank.

2d. Variation in the power of suspension as the water passes along its course is largely due to relative values of sectional area, and these depend chiefly upon width and form of section.

3d. Scour and deposit are, under the conditions now existing in the Mississippi, local, not general occurrences, which alternate at a given locality as the river passes from one extreme of stage or volume to the other.

To enforce these conclusions I add this final fact: The volume of matter in suspension which passed the foot of Bullerton towhead between November 13, 1879, and January 3, 1880, was 702,000,000 cubic feet. The amount which passed Fulton (nine miles below) during the same time was 1,011,000,000 cubic feet; wherefore the difference, 1,011,000,000 − 702,000,000 = 309,000,000, must have been obtained by erosion of the intermediate bed and banks. Between March 6, 1880, and April 11, the volumes were: Past Bullerton, 907,000,000 cubic feet; past Fulton, 832,000,000 cubic feet, 75,000,000 cubic feet being deposited in the intermediate bed.

Bullerton is at the foot of a wide shoal reach. Fulton is at a narrow part of the river. During the first period the river rose 26.6 feet, during the second it fell 6 feet.

The results, it will be observed, are similar to those at St. Louis and Horsetail, but the measured quantities show the variation in suspension directly. The succession of contrasting local conditions is reversed, the pool following the shoal.

Variation of width and form of section have appeared to be important factors in determining the alternate movement of heavy material from pool to shoal, and from shoal to pool, as stage or volume increases and diminishes. It is also certain that an indefinite volume of heavy material is brought into the bed of the river every year by tributaries, and a like volume must be discharged at the mouth, in order that a stable or permanent condition may be maintained. Under the alternations, arising from varying width, the river, taking its whole length, is encumbered with the accumulated detritus of a number of years, all traveling. If a summation be made in cubic units of the whole mass moved in a given time, it will exceed the actual discharge of solid matter manifold, but if it be measured as mass moved over space, the summation will be equal to the movement of the incoming material from the point of entrance to the sea.

If we consider that the material passing a given point can travel but a short distance in a day at the farthest, we see that the sum total of moving material must be enormous. For instance, the summation on July 10, 1880, must have been several hundred times the quantity observed at Fulton, 3,232,209 cubic yards, or a total measured by hundreds of millions of cubic yards.

Considering the movement in time of flood we must remember that the arrest of motion due to variation of section is not of the whole mass, but of the coarser part only, and the arrest of this part need not be an absolute stoppage, in the sense that no coarse material passes the wide reach during the period of accummulation. If a column of men be marching along a street at an unequal rate, the center moving more slowly than the extremes, the advance files will widen the intervals, and the rear will at the same time close in upon the slower center. This center illustrates the wide places in time of flood. If again the front and rear slacken step and the center hastens, the massing will be in front, and intervals will increase at the center and toward the rear. The illustration now becomes parallel to motion from wide to narrow sections at low stages.

The changes from accumulation to waste (deposit to scour), therefore, do not imply a cessation of movement at any place or time, but may be accounted for by a varying rate of progress.

If the variation of width be suppressed by "regularization," one of the local complications of silt movements will be removed, the rate and volume will still vary with the motive forces. The movement of heavy bar-forming material is now continued during low stages, but in the regulated channel there will be no concentration of force to enable low stages to work. Will it be possible to secure the through conveyance of the incoming material by the flood? or must we expect the movement to be in wave masses, moved only at high stages? Experience abroad seems to indicate the latter. The problem will then be to reduce these masses to harmless proportions.

Blast Furnaces in Great Britain.—The following is the number of blast furnaces at work in different parts of Great Britain at the present time: Cumberland, 42; Derbyshire, 41; Durham, 26; Gloucestershire, 2; Hampshire, 0; Leicestershire, 2. Lincolnshire, 15; Lancashire, 34; Northamptonshire, 15; Nottinghamshire, 2; Northumberland, 4; North Staffordshire, 27; South Staffordshire, 48; Somersetshire, 1; Shropshire, 9; Wiltshire, 3; Yorkshire, West Riding, 29; Yorkshire, North Riding, 90; North Wales, 6; South Wales, 65; Scotland, 107; total, 568. The total number of furnaces built September 30th, 1882, 940; total number of furnaces in blast September 30th, 1882, 568; increase in the number of furnaces built since June 30th, 1882, 4; decrease in the number of furnaces in blast since June 30th, 1882, 2; furnaces blown out since June 30th, 1882, 19.

ON THE PROPOSED FORTH BRIDGE.

Communication by Sir GEO. B. AIRY, and a Rejoinder by Mr. BENJAMIN BAKER.

An interesting account of the plan of the railway bridge for crossing the Forth at Queensferry, as designed by our distinguished engineer, Mr. Fowler, with the association of Mr. Baker, was given by Mr Baker to the British Association at their late meeting at Southampton. Supported as it was, to the advantage of those present, by the exhibition of the model of the proposed bridge, it must have given extensive information on the character of the structure. Yet it seems to me that, amidst many valuable particulars, on the strength of materials, their mode of application in this instance, and similar important subjects—it would hardly impress sufficiently, upon the minds of hearers or readers, the vastness of the scheme, the novelty of its arrangements, and the dangers (yet untried) to which, conjecturally, it may be subject. I have thought therefore, that I might, without impropriety, offer to the editor of *Nature* some remarks on points which after careful consideration have suggested themselves to me. For some particulars I am indebted to the courtesy of Mr. Fowler himself, and I greatly value this kindness.

It is known that at Queensferry the separation of the river banks, or rather that of the piers next to the banks at the elevation required for the railway, approaches to a mile. This space is divided by three piers (for which there are excellent foundations on rock and hard clay) into four parts, but only the two middle parts concern us now. They are exactly similar, and are treated in exactly the same way; and subsequent allusions, referring ostensibly to one, are to be considered as applicable to both. Each of the three piers is an iron frame, 350 feet high, the central pier 270 feet wide (in the direction of length of the bridge), and each of the others 150 feet. These lofty frames are braced, each upper angle on one side to lower angle on the other side, with no other diagonal bracing, but with a single tie at mid-height. The lengths of the diagonal bracing are respectively about 430 and 360 feet. The water-spaces between two piers are each about 1700 feet, and the engineering question now is, how this space of 1700 feet (roughly one-third of a mile) is to be bridged for the passage of a railway.

The plan proposed is, to attach to each side of each frame (that is, to each side which will face a traveller entering upon the bridge) a framed cantilever or bracket about 675 feet long (that is, exceeding in length an English furlong by 15 feet), attached at top and bottom to the iron frame above mentioned, but having no other support in its entire length of 675 feet. To give the reader a practical idea of the length of this bracket, I remark that the length of St. Paul's Cathedral, outside to outside, is exactly 500 feet; and thus this bracket, which is to project over the water without any support whatever, is longer than the Cathedral by 175 feet. This in itself is enough to excite some fear, supposing the bracket to support merely its own weight. But further, the bracket bears also the very considerable weight of the roadway and rails. It is also heavily loaded on its point. The two opposing brackets from the two iron frames cover 1350 feet, but the whole space to be covered is 1700 feet, leaving 350 feet yet to be supplied for the support of the railway. To furnish this, a lattice-girder carrying a railway is provided, rather more than 350 feet long, whose extremities rest upon the tips of the two brackets.

This statement is enough, I think, to justify great alarm. No specimen, I believe, exists of any cantilever protruding to a length comparable, even in a low degree, to the enormous brackets proposed here. The only structures of this class, in ordinary mechanics, known to me, are the swing-bridges for crossing dock-entrances, and the like, and these are absolutely petty in the present comparison.

I now advert to the weights of the principal portions of the bridge, and the strains which they will create. I understand that the weight of the two parallel

braced sides of one bracket is about 3360 tons, to which is to be added the weight of roadway and rails for 675 feet, on which I have no information. I proceed to inquire what strains, in the nature of horizontal pull at the top of the pier and horizontal push at the bottom of the pier, will be caused by this weight. If the weight were evenly dispersed over the triangular bracket, its centre of gravity would be distant from the pier by one-third of the distance of the point from the pier. But as no vertical bar near the pier is included in the weight above, I must take a larger factor, say $\frac{2}{5}$. The vertical weight being 3360 tons, acting at a distance from the pier of $\frac{2}{5}$ x 675 feet, and the separation of the points of connection with the pier being 350 feet, it is easily seen that the horizontal pull at the top and push at the bottom are each about 2600 tons. The inclined tension along the great upper bar of the cantilever and the inclined thrust along the great lower bar of the cantilever are therefore each about 2670 tons. The extremities of the great upper bar and the great lower bar being connected at the point of the bracket, and (for a moment) no other weight being supposed to act, there is no tension or thrust at that point, and therefore the tension and the thrust increase gradually, according to the attachment of their loads, from nothing at the point of the bracket to 2670 tons at connection with the pier.

But the point of the bracket is permanently loaded with half the weight of the intermediate 350-feet railway, or 363 tons, and occasionally loaded with the whole weight of a railway train, say for a passenger train 150 tons (a mineral train would be heavier). The vertical weight of 513 tons thus introduced would be met by a tension of 1004 tons through the whole length of the great upper bar, and a thrust of 1004 tons through the whole length of the great lower bar. Thus we have—

For the great upper bar, a tenison increasing from 1004 tons near its point, to 3674 tons near the pier.

For the great lower bar, a thrust increasing from 1004 tons near its point, to 3674 tons near the pier.

The second of these statements particularly requires attention.

Mechanical students and professional engineers are accustomed to estimate by numerical measure the magnitude of a horizontal or nearly a horizontal thrust but persons in ordinary life scarcely attach a clear meaning to such a phrase. I am therefore compelled to make a somewhat violent explanatory supposition, with the hope that it may convey a practical impression as to the meaning of the statements just given.

The great lower bar is in fact a nearly flat frame, braced from side to side, about 120 feet wide at the bottom, and about 40 feet wide at the top, and 690 feet long. Suppose this structure to be planted vertically, say in St. Paul's Churchyard, without any bars, chains, or any thing else, below its vertex, to prevent motion edgewise, but with bracing (which, under ordinary circumstances, would suffice, but which will be the subject of further remark) to prevent its moving flatwise. Its top would be 310 feet higher than the top of the cross of St. Paul's Cathedral. Suppose a weight of 1000 tons to be placed on its very top, and additional weights (if necessary) to be placed at its sides, till the whole weight pressing the ground is 3600 tons. In this state its condition is exactly that of the great lower bar, as regards the crushing and distorting tendency of the weights (although the upper weight itself ought to be considered as partially protected from lateral movement by the great upper bar). With this enormous load at this stupendous height, would the citizens of London in the Churchyard below feel themselves in perfect security? I think not; and I claim the same privilege of entertaining the sense of insecurity of the proposed Forth bridge.

The danger arising from the endwise action of so large a force on so long a bar or frame, is produced by the curvature technically called "buckling," and there appears to be fear of its occurrence in various parts of the bracket, and in some parts sequentially, that is to say, that a buckling of a minor order might lead to a buckling of a more important order. Thus, proceeding from the pier, the first support of the great lower bar is by a suspension rod from the great upper bar, to which, as regards merely the suspension-rod, there can be no objection. But the upper attachment of this suspension-rod is supported by a

thrust-rod about 340 feet long. Can this rod be considered safe against buckling? In the total absence of experiment or explanation, I may be permitted to express a doubt of safety. And if that rod fail, the corresponding part of the great lower bar will sink, it will buckle under its enormous end-thrust, and the bridge will be ruined. The second support of the great lower bar depends, in like manner, on a thrust rod whose length is 240 feet; considerations of the same kind apply to it, though probably in a minor degree.

Experienced engineers must have known instances in which buildings have fallen from want of consideration of buckling. The following occurred within my knowledge. When the Brunswick Theatre was built, the construction of its trussed iron roof was greatly extolled, and Mr. Whewell and myself, then residing at Cambridge, and proposing to visit London about the same time, had arranged to inspect the truss. But before we reached London it was ruined. There was no adequete bracing of the principal rafters in the plane of the roof; the suspension of a very slight weight on the great tie caused the rafters to buckle sideways, and the roof fell, destroying the building.

I am not aware whether a theory of buckling finds place in any of the books which treat of engineering in somewhat mathematical form. But there ought to be such. It can be formed with no difficulty and little trouble, giving such a form of result, that all that will be required in any case, to determine the end-pressure which can safely be applied to the end of a bar, will be expressed in terms of the length of a bar, and the curvature caused by a transversal strain (determined by simple experiment). This theorem ought to applied in every instance.

I need scarcely to remark that every construction is liable to chance-errors of unforeseen character, and I think that the proposed construction, which depends for its safety entirely on the maintenance of the thrust-principle in perfection, is more liable than any other to danger from these causes. A rivet-head may slip, or a screw may strip, and all may be imperilled. Robert Stephenson, when the Menai Bridge, used every caution that an active mind could invent: in particular he provided that the masonry for final support of the tubes should be raised as quickly as possible to take the bearing of the tubes at every moment. Yet an accident, though a small one, did happen. The ends of the tubes were raised by the power of hydraulic presses; the cylinder of one of these presses burst, and the end of the tube fell three or four inches. This minute fall, in the judgment of the attendant engineers, gave a strain to the tube such as it never sustained before or since. (This accident came first to my knowledge in a singular way. With the assistance of my friends, Captain Tupman, R. M. A., and James Carpenter, Esq., and before having heard of the accident, I made experiments on the state of permanent magnetism of the great iron tubes. One of these showed an anomaly, somewhat similar to that of iron heavily struck. On my mentioning this to Mr. Edwin Clark and others, the phenomenon was at once referred to the accidental shock which I have described.)

Much has been said on the action of the wind, and on the difference of that action upon a suspended bridge, and upon a girder bridge. In regard (first) to the amount of pressure, I refer to a former letter of mine, correctly cited in the evidence before the Committee on the fall of the Tay Bridge, in which I state that the maximum pressure may be more than 40 lbs. on the square foot (I should say more than 50 lbs. for Scotland), but that this action is so limited, both in time and in local extent [and is, I add, so continually varying in direction], that the average of direct pressure probably would not exceed 10 lbs. on the square foot. In regard (secondly) to the difference of wind-action in the two systems of construction;—the immediate effect of the wind appears to me to be a shock, of limited extent, which is much less likely to be injurious on a comparatively flexible frame suspended from above, than on a jointed frame where every joint must be tight, and where ruin will follow disturbance. In the proposed Forth Bridge, however, there is a risk of danger of the most serious kind, which may perhaps surpass all the other dangers. It arises from the horizontal action of the wind on the great pro-

jecting brackets, and its tendency to wrench them laterally from their attachments. The ruinous force depends, not simply on the magnitude of the wind's pressure, but also on its leverage; as measured by the proportion of the height of the Tay Bridge or the length of the bracket of the Forth Bridge, to the separation (in each case) of their horizontal attachments to the solid piers. The leverage is considerably greater in the instance of the proposed Forth Bridge than it was in that of the unfortunate Tay Bridge, and we may reasonably expect the destruction of the Forth Brdge in a lighter gale than that which destroyed the Tay Bridge.

I may now collect the heads of my remarks on the proposed Forth Bridge:

I. The proposed construction is, as applied to railway bridges, entirely novel.

II. The magnitude of its parts is enormous.

III. There has been no succession of instances of the construction, with the rising degrees of magnitude, which might furnish experimental knowledge of some of the risks of construction.

IV. The safety of the bridge depends entirely on a system of end-thrusts upon very long rods; a system which appears generally objectionable, but particularly so when the length of the rods is very great.

V. No reference is made to theory applied to the buckling of rods under end thrusts.

VI. The liability to ruinous disturbance by the lateral power of the wind acting with the leverage of the long brackets appears to be alarmingly great.

My own impression is, that the proposed construction is not a safe one, and I should be happy to hear that it is withdrawn.

I refer unhesitatingly to "the Suspension Bridge" as the construction which I should recommend. On this system generally I remark: (1) that I am incredulous as to the oscillation of 8 feet in extent, or any sensible part of it; (2) that if the railway is slightly arched upwards to the degree corresponding to depression caused by an average train, such a train will run on a horizontal plain; (3) that a stiffening lattice may be used with very good effect against vertical oscillations from all causes.

The considerable height of the piers, and the great length of the suspension-chains, are matters to be viewed carefully.

To reduce them as far as possible, I would suggest for examination the following proposals:

1. Suppose the stone or iron piers to be much lower than the plans hitherto proposed, and suppose that the top of a pier carries a bracket on each side, so that the great suspending chain passes over the points of the brackets, and its suspending action begins at those points. The bracket frame may be horizontal where it passes the top of the pier; or it may be raised in a horn on each side, and thus adapted to a smaller height of pier. By this construction, with brackets 150 feet long (a trifle compared with those of the proposed cantilevers), the piers may without difficulty be shortened 200 feet, and the acting-length of suspending chain may be reduced 150 feet at each end, or 300 feet over each water-channel. This would leave much liberty in regard to the curvature of the chain.

2. It is very desirable, if possible, to reduce the specific weight of the chain per yard, corresponding to a specified suspension strain. This has been attempted on the Continent by the use of wire, and it has been highly praised for its combination of lightness and strength. The longest carriage-bridge that I have passed (that of Freyburg, 890 feet span) is a wire bridge. I have also crossed the Rhone at Montelimart by wire arches of considerable span. I know not whether this construction has been tried in England.

G. B. AIRY.

I have read Sir George Airy's criticism of the design for the proposed Forth Bridge with interest. So far as engineers are concerned the letter calls for no reply; but as others pardonably ignorant of the present state of engineering science may feel the same difficulties as Sir George Airy, I propose with your permission to offer a few explanations.

Sir G. Airy summarizes his remarks under six heads, but I think two would have sufficed, viz.: that the bridge was too big to please Sir George, and that

the engineers were presumably incompetent. As to size, for example, Sir George considers the fact of the cantilever being "longer than the Cathedral by 175 feet is in itself enough to excite some fear," and even to "justify great alarm." But when I look for some justification for this bold statement I find that Sir George does not advance any reason whatever, nor make use in any way of his high mathematical attainments, but simply shifts the responsibility for this alarm on to the shoulders of the "citizens of London," asking "would they feel themselves in perfect security? I think not; and I claim the same privilege of entertaining the sense of insecurity for the proposed Forth Bridge."

If Sir George had alleged that the stresses on the cantilever could not be calculated, or that the strength of the steel ties and struts could not be predicted, or that the cantilever could not be erected, I might have replied by publishing diagrams of stresses, results of experiments, and the names of the firms who have tendered for the work. I cannot, however, answer an argument based upon the supposed fears of the "citizens of London."

To prove that Sir George's criticisms imply a charge of incompetency on the part of the engineers, I need only point out that in one sentence he remarks that "experienced engineers must have known instances in which buildings have failed from want of consideration of buckling," and in another, that "there appears to be a fear of its occurrence in various parts of the bracket," when "the bridge will be ruined." Sir George's conclusions on this head are, however, as he fairly enough states, "made in the total absence of experiment or explanation," and in ignorance whether "a theory of buckling finds place in any of the books which treat of engineering." To assume, however, that an engineer is similarly ignorant, clearly amounts to a grave charge of incompetency. Again how incompetent must the engineer be who required to be informed that the "horizontal action of the wind on the great projecting brackets depends not simply on the wind's pressure, but also on its leverage," or who neglected to provide for the consequent stresses. Yet Sir George does not hesitate to say in reference to this, that "in the proposed Forth bridge there is a risk of danger of the most serious kind, which may perhaps surpass all other dangers."

As Sir George in the whole of his letter does not produce a single figure or fact in support of his very serious charges, I must, in justice to Mr. Fowler and myself, explain that it was from no want of data. At Sir George's request he was furnished with every necessary detail for ascertaining the maximum stress on each member, and the factor of safety. I stated in the paper referred to by Sir George at the commencement of his letter, that under the combined action of an impossible rolling load of 3400 tons upon one span, and a hurricane of 56 lbs. per square foot, the maximum stress upon the steel would in no case exceed $7\frac{1}{2}$ tons per square inch. Any useful criticism must be directed to prove that such load is not enough or that such stress is too great. Nothing can be decided by appeals to the citizens of London.

Sir George's remarks about what he terms "buckling," and the "total absence of experiment," I can hardly reconcile with his having read my paper, because I have there devoted six pages to the question of long struts, and have given the results of the most recent experiments on flexure by myself and others. When he asks whether a tubular strut 340 feet long would be safe against buckling, he has evidently overlooked the twenty years' existence of the Saltash bridge, which has a tubular arched iron strut 455 feet long, subject to higher stresses than are any of the steel struts in the proposed bridge. Reference is made to the fall of the roof of the Brunswick Theatre, which is attributed to buckling. This accident occurred about fifty-four years ago, and consequently considerably before my time; nevertheless I have heard of it often; and if I am not mistaken, the verdict of the jury was to the effect that the fall of the roof was due to a carpenter's shop weighing about twenty-five tons having been built on the tie-rod, which sagged under the weight, and so pulled the feet of the principals off the wall. However that may be matters little, as engineers are in possession of more recent and trustworthy data than the personal reminiscences of Sir

George Airy. American bridges invariably have long struts, and consequently there is no lack of practical experience on the subject.

The late Astronomer Royal thinks that "the proposed construction is not a safe one," and hopes to see it withdrawn. When he wrote his letter it probably did not occur to him that rival railway companies might be only too glad to seize hold of anything which might prejudice the Forth bridge project and alarm the contractors who were preparing their tenders for the work. I do not complain of Sir George's action, as it involves a matter of taste of which he is sole judge. I would only mention that when he penned the above sentence he had been furnished by the engineers with the parliamentary evidence and other documents necessary to inform him of the following facts:—(1) That a wind pressure of 448 lbs. per square foot upon the front surface would, as stated in my paper on the Forth Bridge, be "required to upset the bridge, and under this ideal pressure, though the wind bracing would, it is true, be on the point of failing, none of the great tubes or tension members of the main girders would even be permanently deformed." (2) From the evidence given before the Tay Bridge Commissioners, Sir George, being a witness, would know that, even supposing the workmanship had been good, a wind pressure of about one-tenth of the preceding would have sufficed to destroy the Tay Bridge. (3) He would also remember, no doubt, his own report of 1873, wherein he says that "the greatest wind pressure to which a plain surface like that of the Forth Bridge will be subjected in its whole extent is 10 lbs. per square foot." (4) The Parliamentary evidence would have informed him that the proposed design was the outcome of many months consideration by the engineers-in-chief of the companies interested, representing a joint capital of 225 millions sterling, and that it was referred to a Special Committee of the House of Commons and to a Special Committee of the Board of Trade inspecting officers for examination and report, and that the reports of engineers and committees were alike unanimous in testifying to the exceptional strength and stability of the proposed bridge. As a sample of foreign opinion, I would quote that of Mr. Clarke, the eminent American engineer and contractor, who has built more big bridges himself than are to be found in the whole of this country, and who has just completed a viaduct 301 feet in height, by far the tallest in the world. Referring to the proposed bridge, he writes: "If my opinion is of any value I wish to say that a more thoroughly practical and well considered design I have ever seen." I need hardly say that the opinion of such a man has far more weight than that of an army of amateurs.

Sir George Airy refers "unhesitatingly to the suspension bridge" as the construction which he should recommend. He has clearly learnt nothing on that head during the past ten years. In a report on the late Sir Thomas Bouch's design for the Forth Bridge on the suspension principle, dated April 9, 1873, he says: "I have no doubt of the perfect success of this bridge, and I should be proud to have my name associated with it." Chiefly on this recommendation, and in spite of numerous warnings from practical men, the bridge was commenced, but it had to be abandoned after spending many thousands, because having reference to the fate of the Tay Bridge, it was pronounced by the Board of Trade and every engineer of experience at home and abroad to be totally unfit to carry railway trains in safety across the Forth.

Sir George Airy stands alone in his advocacy of a suspension bridge for high speed traffic, and in his views as to the force and action of the wind on such a structure. That being so I may be permitted to say that I should have felt no little misgiving if he had approved of the substituted girder bridge, because it has been the aim of Mr. Fowler and myself to design a structure of exceptional strength and rigidity, differing in every essential respect from that with which Sir George evidently would still be proud to have his name associated. B. Baker.

During the half-year ending 30th June last the engines of the London, Chatham and Dover Railway made 1,627,283 train miles, and the total cost for locomotive power was £68,053.

ON THE DRAUGHT OF CHIMNEYS.

From "The Builder."

The attention of the Mathematical and Physical Section of the British Association was, on the 30th of August, called by their president, Lord Rayleigh, to the very humble practical inquiry as to the effect of wind on the draught of chimneys. It would be difficult more aptly to illustrate the true connection between the pursuit of pure science, and the most everyday annoyances of domestic life (with a view to the removal of the latter), than is afforded by this announcement. As a prime feature in the architecture of dwelling houses the chimney demands a chapter to itself. And it is well that the basis of this chapter should be mathematical. With regard to draught, we have more than once had occasion to remark that it is a function of the height of the chimney tubes. We do not, of course, apply the remark without modification to the details of the ill-constructed chimneys. Many chimneys smoke from positive ill-construction, having a narrow part, an ugly bend, or a downward portion in their conduit, which the normal draught is not strong enough to overcome. Others smoke because they form the shortest channel by which air can enter the house to supply the draught of the loftier chimneys. Where there are two chimneys in one room, and the doors and windows are closed, lighting a fire in one chimney will ordinarily cause down-draught in the other, as the readiest mode of maintaining the atmospheric equilibrium. In most cases, however, the addition of a yard or so to the height of the chimney stack, supposing that to be already above the highest part of the interior of the house which it ventilates, will prove more useful than the addition of any of those curiously contorted and wholly non pneumatic contrivances, the endless variety of which tortures the eye of the mechanically-minded man as he whirls along the suburban railways.

Apart from the questions of normal draught, and of internal regularity of construction, is the effect of wind on the draught of chimneys. This varies very much with the locality of the house. It may be said to be primarily a question of sight. Neither is it a question always to be settled, *a priori*, by the architect. The action of wind, although, of course, always determined by physical laws, is so subtle, and so delicately affected by slight causes, that there are cases in which it is all but impossible to foretell it. What is more common than to have a chimney that answers perfectly well, except when the wind is in one particular point of the compass? And why does it then smoke? To be able to reply to this question is to be able to cure the defect; but how often is the difficulty regarded as insuperable? It may be useful to cite an instance of the deflection of wind in a manner that it is difficult to explain, and that could certainly never have been anticipated. On the lovely plain of Sorrento, sloping towards the Bay of Naples, in the midst of its own gardens and orange groves, stands a stately palace, which, in the days when the name of Nelson was a terror on the seas, was a favorite haunt of the Queen of Naples, and was not unfamiliar to our great sea captain. There are no rocks, lofty buildings, or overhanging trees in the immediate vicinity, and nothing that is apparently likely to interfere with the free movement of the wind, from whatever quarter it may blow. The aspect of the front of the palace is northerly, looking over the Bay towards Naples, and the hills behind it. On a few occasions—some three or four times, perhaps, in the year—a strong east wind sweeps across the peninsula, and is clearly indicated as to its course by the straight and level stream of smoke that it drives from the cone of Vesuvius across the Bay. But on these occasions, the current is so deflected by the northern shores of the Bay, fully twenty miles distant, as to drive on the front of the palace; that is to say, in a direction pointing nearly due north with such force that it becomes necessary to close the *persiani*, or outer window-shutters, in order to preserve the great drawing-

room windows from being blown bodily in. Full familiarity with the spot does not afford any very satisfactory explanation of the phenomenon, of which, however, we can speak from repeated personal observation.

This, perhaps, may be regarded as an extreme case. Nevertheless, it is typical of not a few instances in which currents may be detected after a house is built, the existence and effect of which the architect cannot be blamed for not having foreseen, but the influence of which on the draught of his chimneys will be, for so many days in the year, at once irresistible and intolerable. There is now absolutely nothing for it, in such a case, but to put out the fires. We do not say that a cowl may not be of use, but, on the other hand, we cannot speak from any experience of the action of a cowl under such conditions.

Here, then, comes in the scientific part of the question as attacked by Lord Rayleigh. What is the effect of a cross current of wind on the draught of a chimney? As the direction of the wind is more and more downward, his lordship replies, the up-draught of the chimney diminishes, but does not turn backwards, that is to say, become down-draught until the inclination amounts to about 30°. This is an important point at which to arrive. It is, however, obviously necessary to ascertain the precise features of the cases where the experiments were made, before formulating any absolute rule on the subject. The height of the top of the chimney, both above the sea level and above the basement of the house, is one fact needful to determine. The force of the wind is another. Thus, a wind of the velocity of ten or twenty miles an hour blowing downwards at a measured angle would produce a very different effect on the draught of a chimney 40 ft. high from that which it would have on the draught of a chimney 100 ft. high. If our recently published table on wind pressure (*ante*, p 200) be referred to, it will be seen how important are these data for the solution of the problem.

Again, the maximum up-draught, we are told, is not with a direction of wind vertically upward, but with one making an angle of about 30° with the vertical. This is a result which it is without our experience either to confirm or to question. But it is clear that the cases must be extremely rare, if ever they occur, in which the top of a chimney would be exposed to a wind blowing upwards at a more acute angle than 30°. The determination of the same angle as the limit of downward and of upward efficiency is as elegant as it is unexpected.

A yet more practical part of the subject is the remark of Lord Rayleigh, that a "chimney with a T-piece at the top never produced an unfavorable effect on the up-draught, and only in one case failed to produce a favorable one." Professor de Chaumont thought vertical ends would increase resistance to the up-draught. So do ninety-nine people out of a hundred. We have no hesitation in saying that Lord Rayleigh is right, and that general opinion is wrong, provided, that is to say, that the protection of the chimney-top be rightly designed. And we say this absolutely and unhesitatingly, because our judgment is based on the two independent and concurrent sources of evidence afforded by observation and by direct experiment. In Southern Italy chimneys, no doubt, are comparatively few, but they exist in many buildings, and no Italian architect would dream of leaving a vertical flue or pipe open at the top to receive the tremendous downpour of rain which occasionally occurs in Italy with almost tropical violence. The aperture of the chimney is always roofed over, the smoke escaping by side apertures. The practice of the master builders of the world is thus in accordance with the theory of the English scientific noble in this respect.

Experience, we have said, confirms the remark. And here, if we seem for a moment to depart from an admirable rule—that of not mentioning in a scientific paper anything that may look like an advertisement—we must explain that such appearance is deceptive. We have to refer to an admirable invention, which was made and sold some forty years ago under the name of Day's Patent Wind Guard. As to the validity of the patent it is not needful now to inquire, as in any case it must have long since expired. But what we lament is, that, so far as we can discover, this admirable invention has been entirely forgotten. In recent inquries which we have instituted in

order to obtain one of these wind-guards for our own use, we have been unable to discover that they are now made, although figured in some old trade-books, It may, therefore, be of much use to give such particulars as will guide any one in constructing an appliance of the kind.

Let us take, for example, the case of a chimney two bricks and a half, or 22½ inches square, with a flue of 9 inches square. A frame is to be constructed which would lie upon this hollow square, and allow bricks to be built on it to the height requisite to hold it steady. From this frame the guard itself rises as an octagonal shaft. Four pieces of sheet iron, or of slate, each 9 inches wide and 18 inches high, are fixed on the lower frame, 16 inches apart, so as to form four sides of an octagon, and the other four sides are formed of four similar plates, set at angles of 45° with the former, but so much within them as to allow 1½ in. to 2 in. clearance between the edges. These inner plates will just touch the outer angle of the 9 in. flue. A top of iron or of stone, holds the upright plates together, and both top-plate and base-plate may be finished with a simple moulding. Sheet iron is perhaps the best material, but it is obvious that the contrivance may be made also in slate.

As to the perfect security which this guard effects from wind, we can speak not only from the testimony of those who have used the plan, but from careful experiment. With models of the wind-guard, made on the scale of 1 in. to a foot, set on small tubes of proportionate size, representing chimneys, which were fed with smoke from the combustion of paper, we have experimented by means of powerful jets of wind driven in various lines of force; vertically downwards on the top of the guard, horizontally at right angles to either side or angle, or in any other direction. In no case have we found it possible to produce down-draught in the chimney by force of wind. On the contrary, the greater the force the better the draught of the chimney.

The *rationale* of this action may be illustrated by that of a much later invention, now in common use on our railways—the steam injector. Let those of our readers who desire to master the subject, and have no more ready mode of doing so, refer to the diagram and description of this appliance which is given by Sir F. J. Bramwell, on p. 335 of a work recently published by Messrs. Longmans, under the title, "Railways and Locomotives." It will there be seen that "steam, escaping at high velocity through a circular orifice, induces a current in a body of water in connection with that orifice, and carries it forward at the less velocity corresponding to the increased weights of the combined stream of water and steam, but still at a high velocity." This stream passes into an expanding channel, where its velocity is further reduced, and so into the boiler, which is thus fed with water.

In the case of the wind-guard, the wind striking on the outer plate causes a current of proportional velocity between the edges of the outer and inner plates, and thus creates an induced current, which comes in aid of the draught of the chimney. The difficulty to be overcome is far less than in the case of the two fluids of unequal specific gravity. But the effect is so much the more powerful. We are familiar with the contrivance mentioned by Professor de Chaumont, which has been used for the protection of lamps from the dash and spray of the sea, as we believe, with admirable results. But it is a much more complicated, and therefore more costly contrivance than the very simple one which we have endeavored to rescue from oblivion. And we think that it is purely protective in its action, while the construction that we describe is not only protective, but absolutely aids the up-draught; and, moreover, the cone is more likely to become choked with soot. The brush of the sweep can easily ascend into the interior of the wind-guard, in case of any soot lodging there.

We have little doubt that any enterprising manufacturer who will discover one of these old wind-guards (they were made and sold somewhere in Pimlico, in 1842), make some to pattern, and introduce them to the trade, will at the same time do a good stroke of business, and confer a very great benefit both on the architect who has to build in exposed and gusty situations, and on many a householder who now knows too well, and that to his cost, when the

wind is in the east—or in that quarter, wherever it may be, that is hostile to the good behavior of one or more of his chimneys.

We are not acquainted with any mathematical formulæ intended for the guidance of the architect as to the height or draught of chimneys. It may, therefore, be of some service to provide a simple table for the use of the architect and of the builder, stating, in the first instance, the principles on which it is calculated.

The only cause of that upward movement which we call the draught of a chimney is the difference between the weight of the column of air and vapor in the shaft, and that of a column of equal height of the exterior atmosphere. This difference of weight must be enough to overcome the friction within the chimney shaft and any resistance to free discharge at the top. It is obvious that the ascending volume consists of matter which, at equal temperatures, is heavier than atmospheric air, as it contains soot, or finely comminuted carbon, carbonic acid, sulphur, and other matters naturally heavier than air. Calling attention to the existence of these two sources of retardation, viz., friction and the loading of the column, it will be safe to neglect them in tabulation, as it is very difficult to determine the absolute heat of the ascending column, and a few degrees more or less will amount to quite as much as the elements thus neglected. With this explanation, the formula that regulates draught may be thus stated:

$$D=(a.t.-a.t'.)h.$$

where D=the draught, $a.t.$ the weight of a cubic foot of atmospheric air at the external temperature, and $a.t'.$ the weight of a cubic foot of atmospheric air at the mean temperature within the shaft, and h the height of the chimney. It is obvious from a glance at this formula of how much importance the addition of a very few feet to the height of a chimney must be.

To increase the draught, we must increase either h, the height of the chimney, or t', the temperature of its contents. This explains at once the efficacy of the best appliances for increasing draught—such as the bi-valvular registers, in this country, and the iron blind that is drawn down within the chimney aperture in the wood-burning fireplaces of France and of Belgium. The larger the aperture below—the greater the quantity of cool air that enters the chimney—the lower is t', and the less the draught. The more the air that enters below is compelled to pass through in close proximity with the burning fuel, the higher does t' become, and the more powerful the draught. This is the simple key to the whole theory and practice of draught produced by heat. Of course, in different conditions of the barometric column equal amounts of heat will have somewhat different effects. Hence we can see in a moment why our fires should "draw" so much better in clear cold weather than when the air is warm, or loaded with damp. The weight of a cubic foot of air at 32° Fahr. is 0.080728 lbs. avoirdupois.

If the top of a chimney-flue were closed by a flap, or hinged valve (the weight of which we take as counterbalanced), the effect of the draught, or rather of the downward pressure of the external atmosphere which causes the upward draught, would be to lift the valve in proportion to the force of the current. If at the same time a strong wind be blowing over the top of the chimney (in the direction of the hinged end of the flap), its effect would be to force down and close the valve. The stronger current of the two will obtain the mastery, to the degree of its excess of pressure. This is also the effect, it will be seen, of cross current where there is no valve, but where the two currents more or less interfere with the movement of one another in the way shown by the action of the valve.

It is possible wholly to eliminate the ill effects of this cross current, and even to utilize it in aid of draught, by any contrivance that causes the external blast to produce an induced current from within the chimney; and this, it is easy to see, is the effect of the wind-guard.

In calculating the following table we purposely use round and approximate figures, as quite sufficiently accurate for practical purposes, and as more readily intelligible to many readers than more complicated mathematical expressions. We have also used the pressures proportionate to velocity as allowed by Smeaton. Now, assuming, for conveni-

ence of calculation, an increase of temperature of 108° Fahrenheit (that is, from 62° to 170°), as imparted by a fire to the ascending column of air that passes over it up the chimney, we shall have an upward pressure of a fraction over 0.04 lb. for every foot of ascent in the shaft. This is represented in the following table of comparative heights of chimney, pressures, and velocities of current, calculated for that rise of temperature.

Table for Calculation of Chimney Draught:

Height of Chimney in feet.	Pressure due to 116° Fahr. in pounds per foot.	Velocity of draught in miles per hr.
30	1.2	16
40	1.6	18
50	2.0	20
60	2.4	22
78	2.8	24
80	3.2	26

In rising from 15 feet to 50 feet there is an increase in the local velocity of the same wind from 60 to 70.8 miles per hour. In raising a chimney from 15 feet to 50 feet we should obtain an increase of draught pressure of from 0.6 lbs. to 2.0 lbs. per foot, as against an increase of wind pressure, in a great storm, of from 20 lbs. to 27 lbs. per foot. The gain is more than threefold—the loss less than 33 per cent. We mention this, not as a difficulty, but to show that proper consideration must be given to the whole subject.

It is, of course, apparent that if we have an ordinary fireplace, with a chimney 50 feet high, and with that proportion of fuel and of draught which gives a current of twenty miles an hour to the ascending column of smoke, the aperture at the top of the chimney would be closed by a cross-current blowing at the rate of seventy miles an hour, almost as efficiently as by a valve weighted to 27 lbs. per foot. The fire, in fact, would have to be extinguished. But the pressure of 27 lbs. per foot, or 0.19 lb. per inch, is very trifling in comparison to the pressure of steam in a boiler; and as the action of the injector by induced current is an ascertained fact, it is plain that it is within the province of the engineer to make the hurricane, blow it ever so wildly, produce an induced current through a chimney, so that the draught shall in no way be reversed by storms. As to that, there can be no doubt, although this may be the first time that the statement has been definitely made. We have given what our own experience has led us to believe to be a simple and effective means of producing this induced current. If any of our readers can recommend a better plan, we shall be happy to make it known.

We had put on paper some remarks on one or two other matters discussed at the British Association which appeared to us of striking industrial importance; but the novelty of a scientific investigation of a subject which most men are accustomed to regard with a hopeless shrug of the shoulder, as well as the importance of a thorough appreciation of a matter which comes so directly home to every householder and to every housebuilder, is such that we have thought it well to devote to this question of draught all the space at present available. We think that it cannot be too strongly insisted on that when, first, the subject of up-draught, and, secondly, that of wind-protection — or rather wind-induced draught—are seen to be thoroughly within the control of science, it will be felt to be unpardonable to be condemned to the great nuisance of a smoky chimney.

P. Weiskopf has given the following formulæ for the frit or mass used in Bohemia for making imitations of some of the precious stones: Imitation agates—10 kilos. quartz, 17 kilos. red lead, 3.2 kilos. potash, 2.2 kilos. borax, and 0.1 kilo. arsenic. The quantity of chloride of gold added is equal to that obtained from 0.4 of a ducat. Agate glass—10 parts of broken glass are melted, and to it are added 0.15 part suboxide of copper, the same quantity of the oxides of cromium and of manganese, 0.02 part each of oxide of cobalt and nitrate of silver, 0.01 oxide of uranium, 0.4 red argols, 0.3 part bone meal. Each oxide is added alone and at intervals of ten minutes. After heating the mixture for an hour, 0.3 or 0.4 part of fine soot is put in. Red marble—80 parts of sand, 40 of potash, 10 of lime, 2 of table salt, 1 of saltpetre, and 0.1 of arsenic. The mixture is melted and then 25 parts of suboxide of copper and 1 part of saltpetre mixed in.

THE CONSTRUCTION OF ELECTRO-MAGNETS.

By TH. DU MONCEL.

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I.

THE complaint is general and not without reason, that the subject of electro-magnetism is so obscurely treated by scientists, and the conclusions reached by them are of so little practical value, that the inventor or constructor can derive no profit from them. It is certain that the mathematical physicists regard such problems of too high an order to permit themselves to be diverted by a consideration of the practical applications, and it may be said furthermore that the theories of magnetism are not as yet easily expressed in mathematical symbols, and many of the so-called laws are still matters of controversy with the more conservative.

For ourselves, having made many experiments, we are less skeptical, and although we have not been able to verify rigorously the laws as propounded by Lenz, Jacobi, Dub and Müller, we have obtained results so nearly approaching to verification, that we admit them as guides in the construction of electro-magnets. Ohm's Law relating to electric currents is on the same footing, for it is difficult to take cognizance in the formula of a multitude of secondary reactions which interfere more or less with the calculated results. But these laws are faithful guides which enable us to place ourselves in conditions favorable for success, and that is the essential point.

In order to render such a scientific fact practically useful, it is necessary to be unencumbered with the hypotheses of the higher physics and those terms which but few electricians understand, and to start with experiments made under ordinary conditions. It is certain that if, to appreciate a magnetic force it is necessary to begin with the oscillations of a magnetic needle, or with the currents induced by such force, to arrive at the necessary formulas, the practical constructor can say that though such formulas afford no mental conception of magnetic force, he knows that they relate to a definite weight attracted or supported, and that the real object being to attain the greatest force under given conditions, other considerations are of little importance. It is under such conditions that it is necessary to approach the problem in order to arrive at practical results.

Now I have always admitted that the known laws of electro-magnetism were sufficient for the purposes of the artisan, and upon this idea I have based the different papers published on the best conditions of construction of electro-magnets. Wishing to be sure of my deductions, I have made many experiments in order to verify my formulas, and it was only after the most careful experimental research the writer established the formulas given.

I.—FORMULAS OF ELECTRO-MAGNETISM.

In order to establish my formulas, I start from the elements entering into the construction of the electro-magnet; knowing the dimensions of the core, the size of the wire of the helix, its length; the number of spiral turns in this helix, and its thickness.

Let a=the thickness of the helix.
b=the total length of the two spools or the two branches of the electro-magnet.
c=the inside diameter of the helix, which is presumably the same as the core or magnet.
g=the diameter of the wire of the helix, including its insulating envelope.
A=the attractive force of the magnetic system.
E=the electromotive force of the pile employed.
F=the force of the electro-magnet or its *magnetic moment*.
H=the length of the helix.

I = the intensity of the current in the circuit.
t = the number of turns in the spiral.
R = the resistance of the circuit, including the battery.

It is easily seen that we can represent the number of spiral turns of each layer by $\frac{b}{g}$, and as there are as many layers of wire as g is contained times in the thickness a, we shall have for the total number of turns t

$$(1) \qquad t=\frac{b}{g}\times\frac{a}{g}=\frac{ab}{g^2}.$$

The length of a single turn of the wire in the first or inner layer is $2\pi\frac{c+g}{2}$, and for the outermost $2\pi\frac{c+2a-g}{2}$, and consequently the total length of these layers is $\frac{b}{g}.\,2\pi\frac{c+g}{2}$ and $\frac{b}{g}2\pi\frac{c+2a-g}{2}$.

The intermediate layers form with these two an arithmetical progression of which the above expressions are the extreme terms, and the number of terms is $\frac{a}{g}$. The sum of this series, or the total length of the wire, will be given by the formula:

$$(2) \qquad H=\frac{b}{g}\,\frac{2\pi(c+g+c+2a-g)}{4}\,\frac{a}{g}=\frac{\pi ba(a+c)}{g^2}.$$

The values of t and H are thus obtained in terms of the different elements entering into the construction of an electro-magnet.

These formulas are useful, as they enable us to determine the length and number of turns of the wire of a helix, from the dimensions of the wire, and the thickness and length of the helix.

An expression for the electro-magnetic force is obtained by starting from the laws of Jacobi, Dub and Müller, which express the value of the force F of an electro-magnet or its magnetic moment, in terms of the intensity I of the electric current and the number of turns of the helix; the formula being

$$F=\frac{Et}{R+H};$$

and the value of the attractive force A by the square of this expression; or

$$A=\frac{E^2t^2}{(R+H)^2}.$$

By substituting for t and H in these formulas the values previously obtained, we get:

$$F=\frac{Eab}{Rg^2+\pi ba(a+c)}$$

and

$$A=\frac{E^2a^2b^2}{[Rg^2+\pi ba(a+c)]^2}.$$

The formulas enable us to determine different conditions of maximum effect according as we change the values of a, b, c and g. These conditions are dependent on, 1st, the resistance to be given to the helix; 2d, the ratio between the size of the helix and the magnetic core; and, 3d, the dimensions of the electro-magnet itself.

The different parts of the circuit are composed of different conductors, and it becomes necessary in determining the resistance to consider the wire of the helix, apart from its covering. As g represents the diameter of the covered wire, we may take $\frac{g}{f}$ for the diameter of the naked wire; f having a value of 1.6 for very fine wire, and 1.4 for the medium sizes.

Now in modifying the value of R in the formula so as to involve the above condition, the different conducting powers of the helix and the remainder of the circuit must be taken into account. If q be taken to represent the ratio of conductivity, we shall have for the modified value of R the expression $\frac{qRg^2}{f^2}$. The unit section of conductivity being

$0^m.000016$.

Now if the diameter $\frac{g}{f}$ of the wire of the helix is made to vary while the thickness of the helix remains constant, the length will vary also, so that the resistance of the helix will vary inversely as g^4.

As the value of H in the preceding formulas represents the resistance of the helix, the denominators of the values of F and A become:

$$\frac{qRg^4 + \pi ba(a+c)f^2}{f^2g^4} \text{ and } \left(\frac{qRg^4 + \pi ba(a+c)f^2}{f^2g^4}\right)^2$$

and the values themselves:

$$(3) \quad \begin{cases} F = \dfrac{f^2g^2Eab}{qRg^4 + f^2\pi ba(a+c)}; \text{ and} \\ A = \dfrac{f^4g^4E^2a^2b^2}{[qRg^4 + f^2\pi ba(a+c)]^2}. \end{cases}$$

II.—Conditions of Maximum Effect of Electro-Magnets on a Simple Circuit.

1st. Conditions as determined by the resistance of the helix.

The values of A and F as given above may be discussed from different points of view. The conditions of maximum effect, for instance, may be required for an electro-magnet of *fixed dimensions*, and it is desired to adapt its resistance to that of a given resistance R of the exterior circuit; or it may be required to adjust the resistance of the helix to that of the external circuit without being restricted in the dimensions of the helix. In the first case the variable quantity is the diameter g of the helix wire, and in the latter it is the thickness a of the helix itself.

When we consider that in the preceding formulas the diameter of the helix wire is not g, but $\frac{g}{f}$, in which f is a constant, we see that the calculation is not as simple as it appears at first sight, and for this reason, those who first employed these formulas neglected the value of f and employed g to represent the true diameter of the conducting wire. In taking into account the value of f, we shall not be able always to consider it constant while g is variable, and it is evident that on some occasions it will be necessary to regard this fact. But assuming only the more simple conditions of working, we easily determine from the preceding formulas that the conditions of maximum effect correspond to the equation

$$\frac{qRg^2}{f^2} = \frac{\pi ba(a+c)}{g^2};$$

that is to say, R=H; which signfies that for electro-magnets of the same dimensions, having spools or helices of the same diameter, *that size of wire for the helix is most effective which renders the resistance of the helix equal to that of the exterior circuit.*

If we take the thickness of the covering of the wire into consideration, the formula proves that the most effective helix *is that whose resistance is to the resistance of the exterior circuit, as the diameter of the naked wire is to the diameter of the covered wire.*

If the thickness a of the helix be made to vary, and suppose the action of the spirals to remain the same (an admissible hypothesis under ordinary conditions when we take into account the difference of resistance arising from their greater distance from the soft iron core), the conditions of maximum effect as exhibited in the formula properly modified indicate that R should be equal to $\frac{\pi ba^2}{g^2}$. That is to say, equal to the length H of the wire of the helix divided by the ratio $\frac{a+c}{a}$, or what amounts to the same thing,

$$H = R\left(1 + \frac{c}{a}\right).$$

Translated into ordinary language, this deduction signifies that, among several electro magnetic helices wound with wire of the same size, but having a different number of spiral turns, that which affords the best results upon a circuit of given resistance, *is that helix whose resistance is to the resistance of the external circuit as the thickness of the helix plus the diameter of the core is to the thickness of the helix alone.*

As in applications of electro-magnetism we generally start with a soft iron core of fixed dimensions, and as the conditions of maximum effect, of which we will presently speak, require that the thickness of the helix should be equal to the diameter of the core, and as, on the other hand, the thickness of the insulating cover varies and is thus far undetermined, it is one of the first things to be considered in the solution of the problem of construction. But for an experimenter who desires to know what resistance a circuit should have in order to produce the best effect with a given electro-magnet, the conditions explained in the following section indicate that the resistance of the external circuit should be *less than half that of the elec-*

tro-magnet, if the thickness of the helix is equal to the diameter of the core, as it should be.

2d. Conditions of maximum effect depending on the ratio between the thickness of the helix and the diameter of the iron core.

A second very important point in construction of electro-magnets is the determination of the limit of thickness to be given to the spools of wire to insure the best results. It is understood in a general way that the force of electro-magnets augments with the diameter of the core, and the resistance of the helix increasing in consequence of this increase of diameter, it is evident that there must be a limit at which the advantages gained by increasing the size of the core are counterbalanced by the increase of resistance in the helix. This limit may be determined by calculation.

In equations (3) expressing the values of the magnetic moment and the attractive force, F and A, we will make the quantity c (the diameter of the soft iron core) to vary, and establish an algebraic relation between it and a, the thickness of the helix. This is easily done, since we can suppose the helix wound directly upon the core, and its interior diameter will then be$=c$; and we shall be able in placing the electro-magnet in a condition of maximum effect in relation to the resistance of the exterior circuit, to obtain an expression susceptible of a maximum value, and the ratio of R to H will also be one or the other of those values established in the previous sections.

In representing by λ the coefficient by which to multiply the length of the helix to place the total circuit in one or the other of the conditions of maximum, and supposing the thickness of the helix a and consequently the number of turns of the wire t invariable; the attractive force A and the magnetic moment F have for values, according to the law of Müller relative to the increase of force with the diameter of the iron core,

$$(4)\quad \mathrm{F}=\frac{g^2\mathrm{E}\sqrt{c}}{\lambda\pi ba(a+c)} \quad \text{and}$$

$$\mathrm{A}=\frac{g^4\mathrm{E}^2c}{[\lambda\pi ba(a+c)^2]^2}$$

The expressions become maximum values for a certain value of c, but the quantities R and H are supposed to vary at the same time in proportion as the helix is elongated by reason of the increase of the magnetic core.

If we take the derivative of the preceding expression in relation to c considered as a variable, and equate it to zero, we find that the maximum corresponds to the condition $a=c$. That is, the thickness of the helix equals the diameter of the soft iron magnet. Now this is just the conclusion demonstrated by the experiments of the writer, also the conclusion to be reached at the end of this essay.

The advantages of the laws stated above are readily seen, as they enable us to simplify the calculations in determining the relative dimensions of the parts in constructing electro-magnets. The formula for the length of the helix wire, for instance, becomes, under the conditions for maximum effect, $=\frac{2\pi bc^2}{g^2}$ and if we regard the length b of the electro-magnet as a function of its diameter c, making $b=mc$, the above expression becomes

$$(5)\qquad \frac{2\pi c^3m}{g^2} \text{ or } \frac{75.4c^3}{g^2}$$

making $m=12$ for both branches of the magnet, a proportion which accords well with good practice.

In this formula only two quantities, c and g, are required to be known, and which may be determined by means of relations which will be given further on. On the other hand, we have for the value of the number of turns of the wire in the helix,

$$(6)\qquad \frac{12\,c^2}{g^2}.$$

When the electro-magnet satisfies the conditions of maximum effect, R=H, $a=c$, and $b=cm$; as g is indeterminate it is necessary that R should be made a function of g. This is accomplished by putting the value of H equal to the length of wire required for R; whence

$$\frac{2\pi c^3m}{g^2}=\frac{q\mathrm{R}g^2}{f^2}$$

from which we get

$$g^4=f^2\frac{c^3}{\mathrm{R}}\cdot\frac{2\pi m}{q},$$

and as the last fraction is composed of known terms, this value reduces to *

$$(7) \quad g=\sqrt{f\sqrt{\frac{c^3}{R}}.0.00020106}$$

3d. Conditions of maximum effect depending on the length of the iron core.

From the deductions already made it is easy to see that it is a matter of some importance to make the length of the magnet bear a different relation to its diameter. But the question that remains yet to be answered is—can we determine this length by calculation? It is certain that if we consider the length b only, no condition of maximum effect can be deduced from the values of F. and A. For if, according to the law of Müller, the forces increase as the square root of the length of the magnetic core, these formulas are not susceptible of maximum values when b varies. But if we make b a function of the diameter c the attractive force becomes proportional to

$$c \times \sqrt{cm} \text{ or } c^{\frac{3}{2}}$$

and we obtain for the value of A, taking the other conditions of maximum value into consideration:

$$(8) \qquad A=\frac{E^2m^2c^4c^{\frac{3}{2}}}{[Rg^2+2\pi c^2m]^2}$$

in which R is supposed expressed in terms of a given length of wire of the helix. Now from this manner of reasoning we can readily see that there should be a limit to the value of m, for the magnetizing helix having a giving resistance relative to the external circuit, and this helix having a thickness equal to the diameter of the magnetic core, the resistance can be made to vary more or less according to the ratio between the diameter and length of the core upon which the helix is wound. As the electro-magnetic force increases with the diameter of this core, there is an advantage in increasing this diameter up to a certain point; but on the other hand, as the number of turns of wire for a given length diminishes, as this diameter increases, it is preferable perhaps not to enlarge the diameter but to increase the length of the core. As this requires an increased number of turns of the wire, it compensates under certain conditions for the smaller diameter of the core. The theoretic formulas indicate the limit only indirectly for the conditions of maximum deduced by the preceding formula, taking c as the variable, give $\frac{2\pi c^2m}{g^2}=11$ R. That is to say, according to the hypothesis admitted, ***we can increase the dimensions of the magnetic core until the resistance of the magnetizing helix is eleven times the resistance of the exterior circuit.***

Under these conditions we have

$$m=11\frac{Rg^2}{2\pi c^2},$$

and we see that m becomes eleven times the ratio of resistance of the the exterior circuit to that of the helix when the diameter of the magnetic core equals its length; for in this case this helix has for its expression $\frac{2\pi c^2}{g^2}$; now, as the ratio of the resistance R to that of the helix should be $= 1$ to satisfy the conditions of maximum effect previously found, we deduce, from the preceding formula, $m = 11$.

In the calculations published in *Recherches experimentales sur les maxima electro-magnetiques*, by the writer, these conclusions are demonstrated.*

* In this expression q represents the ratio of conductivity or of resistances between R and H, R being expressed in meters of telegraph wire. As this wire is iron, and the helix is copper, the ratio is about 6. On the other hand, the diameter of the telegraph wire is 4 millimeters, and its cross-section is 0.m000016; q therefore equals $\frac{6}{0.000016} = 375000$, and $\frac{2\pi m}{q} = 0.00020106$ as above.

* Suppose, for example, three electro-magnets in a circuit of 64 meters of telegraph wire, the magnets having diameters respectively of 0.m008, o.m007 and 0.m006, with lengths eleven times their diameters.

The first of these, is furnished with a helix of No. 12 wire, 0.m0006 in diameter, would have a helix 99 meters in length and a resistance represented by 1056 meters. The second magnet would have a helix with 66 meters length of wire, and 704 meters of resistance. The third 41.m36 for length of helix, and a resistance of 441 meters.

The numbers of terms of spiral deduced from the formula $\frac{11c^2}{g^2}$ is 1955 for the first magnet, 1497 for the second, and 1100 for the third. The $\frac{3}{2}$ powers of the diameters being 0.000715, 0.000585 and 0.00046 respectively, we find for the forces of the magnets 0.247868, 0.252891 and 0.250307. The advantage here is plainly with the second magnet, whose resistance is 704 meters, or eleven times that of the exterior circuit. But this is no longer the case if R be made equal to 704.m. The forces then become 0.10087, 0.07524 and 0.04871, and it is clearly the larger magnet that has the advantage, because it is in this one that the resistance is so great compared with R, that this latter value does not affect so largely the value $(R+H)^2$ the divisor of $t^2c^{\frac{3}{2}}$.

As the helices are generally furnished with a disk or spool-end, and the core projects slightly, and as at the lower or connected end a little space is needed for the connections, the coefficient 11 should be slightly increased. Experience indicates that it should be raised to 12.

III.—Conditions of Maximum on Derived Circuits.

The deductions made in the preceding chapter suppose a permanent current established; that there is no reaction arising from extra currents; that the iron of the electro-magnet is in a condition of magnetic saturation, so that the laws of Dub and Müller are applicable, and finally that the exterior circuit R is perfectly insulated. When these conditions do not exist, the above conclusions are not valid, and calculation proves that the resistance of the helix should be considerably reduced. The experiments of M. Hughes have demonstrated this. And these have been furthermore confirmed by Lenoir on a telegraph line between Paris and Bordeaux, which required very rapid alterations of magnetizing and demagnetizing.

It is impossible therefore to establish for electro-magnets a formula which shall express exactly the conditions of maximum to the resistance of helices; but considering the conditions admitted in the preceding formula, and allowing as experiment shows, that the magnetic attraction increases in a ratio more rapid than the square of the intensity of the current, when the iron core is not saturated, we may conclude, as we shall see further on, that from the circumstance alone of rapid charging and decharging the magnet, that the resistance of the helix should be considerably diminished. But a still greater reduction is called for by the action of derived currents on the lines. A telegraph line of 500 kilometers from Paris to Bordeaux required a reduction in the resistance of the electro magnet to 40 km.

All the deductions we have thus far established are, therefore, at fault in such applications of electro-magnetism. But such is not the case in the mechanical applications where the circuit is protected, where the magnetism can be freely developed in the iron core, and where the iron is selected with reference to its capacity for saturation.

We shall have occasion presently to examine the condition of the magnetic core, but in order to have done with the laws previously stated, we will consider for a moment the conditions of maximum effect of an electro-magnet introduced into one of the parts of a derived circuit.

In considering the most simple case, that of a single derivation u established upon a circuit whose resistance is l with a common resistance R starting from the battery, we shall find that the attractive force of the electro-magnet interposed upon l will be:

$$A=\frac{E^2u^2t^2}{[R(u+l+H)+u(l+H)]^2}$$

And if we substitute for t and H their values previously found, we arrive at an expression, considering g as a variable, which corresponds to the conditions of maximum:

$$(9)\qquad \frac{qg^2}{f^2}\left(l+\frac{Ru}{R+u}\right)=\frac{\pi ba(a+c)}{g^2}$$

Causing the thickness a of the helix to vary while g remains constant, the conditions of maximum are represented by:

$$\frac{qg^2}{f^2}\left(l+\frac{Ru}{R+u}\right)=\frac{\pi ba^2}{g^2}$$

In the first of these equations, the second member represents the resistance of the helix, while the first member is the total resistance of the exterior circuit expressed in units of the same order as those giving the length of the wire of the helix. But this *total* resistance is not an increased value. It is represented in fact by

$$R+\frac{lu}{l+u}.$$

The total resistance here should be considered as if the part common to both derived currents were represented by l and as if the really common part R were only a derivation.

In the second equation the first number represents as before the total resistance of the circuit taken inversely; but this total resistance considered as the resistance of an insulated line, should be less than that of the helix in the proportion of 1 to $1+\frac{c}{a}$ in order to satisfy the conditions of the maximum effect.

We may conclude then that the laws of maximum electro-magnetic action on circuits subjected to derivations, are the same as those relating to simple circuits, by supposing that the resistance R stands for the total exterior resistance, with its branches, and admitting that this total resistance is considered *as though a battery were substituted in the circuit of the electro-magnet.* Now as the total resistance of a circuit thus branched is less than the resistance of the original line, the helix should have a less resistance than this latter. All the formulas have been verified by experiment, as we shall see.

When the derived circuits have a feeble resistance, and when the elements of the battery may be arranged at pleasure, calculations will lead to a conclusion analogous to that just reached, but of a converse kind. It is when the combination of cells is such as to make the battery resistance equal to the total resistance of the circuit, that the best results are obtained. But if after establishing a permanent arrangement of the battery, a series of branches are started from the poles of the battery, and the maximum effect upon the separate circuits is sought, as often happens in practice, the conditions of resistance of electro-magnets interposed in the derived circuits are quite different from those heretofore considered. In this case the magnets, in place of having a resistance less than that of the battery, will have a greater resistance, and greater also in proportion to the number of branches. This is easily understood when it is considered as was said above, that the resistance in the battery should always be equal to the sum total of the derived circuits, and that this total resistance becomes less in proportion as there are more branches, so it is necessary to increase the resistance of each individual branch to compensate for the number. If the derived circuits, with their electro-magnets, were all equal, this increase would be proportioned to their number, for the formula for intensity of the current on each branch would then become

$$\frac{n\mathrm{E}}{xn\mathrm{R}+\mathrm{H}},$$

in which x represents the number of branches. But if the derived circuits are of unequal resistance, and if b stand for the number of cells united for *quantity*, and a the number joined for *tension*, the preceding formula will become for two branches, u and H:

$$\frac{a\mathrm{E}}{\frac{a}{b}\mathrm{R}\left(1+\frac{\mathrm{H}}{u}\right)+\mathrm{H}},$$

and the conditions of maximum correspond to

$$\frac{a}{b}\mathrm{R}\left(1+\frac{\mathrm{H}}{u}\right)=\mathrm{H}, \text{ or to } \mathrm{H}=\frac{a\mathrm{R}u}{ub-a\mathrm{R}}.$$

When $u=\mathrm{H}$ then $\mathrm{H}=2\mathrm{R}\frac{a}{b}$. We shall find later an application of these principles.

IV.—Application of the Laws of Maxima to the Construction of Electro-Magnets.

The different laws and formulas which we have established enable us to readily solve the problems relating to electro-magnetic attractions. But it becomes necessary here to introduce the law of Müller relative to the saturation of magnets; a law which establishes that in order to develop in two electro-magnets the same fractional part of their maximum force, it is necessary that the products of the intensity of the currents which charge them multiplied by the number of turns in their helices respectively, should be to each other as the $\frac{3}{2}$ power of their diameters.

This may be thus formulated:

$$\frac{\mathrm{I}t}{\mathrm{I}'t'}=\frac{\sqrt{c^3}}{\sqrt{c'^3}}$$

It is easy to see how, from this formula, it becomes easy to determine the required conditions for an electro-magnet of given diameter or power, not only for furnishing all the force of which it is susceptible, but so that the laws of Jacobi, Dub and Müller shall be applicable. It will suffice if two of the terms in the last formula be determined by experiment. There can be obtained from a *standard electro-magnet* in which the power is varied by varying the current, and the forces produced are proportioned to the squares of the intensities of the current.

Experiments made by the writer with an electro-magnet, the iron core of which had a diameter of 0.^{m}01 and the helix a

resistance of 200^{km}, and which was replaced in a circuit of 118600^{m}, it was found that the ratio in question could be obtained when the battery consisted of 20 Daniell cups. As the conditions of this magnet were known, it was easy by means of the formulas which have been given to establish the constants, and from that time to have terms of comparison to be used in calculations.

Now we will proceed to show how the preceding equation united with the equations

$$t=\frac{mc^2}{g^2},\quad H=\frac{2\pi c^2 m}{g^2},\quad I=\frac{E}{2R},\quad R=H$$

aid us to solve the problems of which we have spoken in determining the values of the diameter c in relation to the quantities E and R.

If in the first equation we consider the accented terms to be known as belonging to the standard electro-magnet, aud if we replace the quantities I and t by their values deduced from the maximum conditions previously discussed, we shall have

$$\frac{\sqrt{c'^3}}{I't'}\cdot\frac{Emc^2}{2Rg^2}=\sqrt{c^3}$$

And as the diameter g is indeterminate and should be satisfied by the condition R=H on the one hand, and $a=c$ on the other, it should be determined in terms of these two quantities. The equation

$$R=\frac{2\pi c^2 m}{g^2}$$

will serve the purpose.

Inasmuch as g is indeterminate and constantly variable, the term R should be reduced to a function of g. This is afforded by the value

$$g^2=\frac{f\sqrt{2\pi c^2 m}}{\sqrt{q}R}.$$

Now substituting this for g^2 in the preceding equation, it becomes:

$$c=\frac{E}{f\sqrt{R}}\left(\frac{\sqrt{c'^3}qm}{2I't'\sqrt{2\pi}}\right)$$

in which the quantity in parenthesis is a constant which varies according to the system of measurement employed, but in which $m=12$, $q=375000$, and $\pi=3.1416$, and in which the other quantities may be regarded as given, since they relate to an electro-magnet working under known conditions.

On the other hand, in applying the same transformations to the value $\frac{\sqrt{c'^3}}{I't'}$ that we have to $\frac{\sqrt{c^3}}{It}$ we arrive at the value

$$c=c'\frac{E}{E'}\frac{\sqrt{R'}}{\sqrt{R}}\frac{f'}{f}.$$

As $\frac{f'}{f}$ is practically equal to unity, we can relieve the expression of the factor f' and consequently the formula simplifies to

$$(10)\qquad c=\frac{E}{\sqrt{R}}K$$

K being a constant with different values according to the unit adopted.

If E be expressed in terms of the ordinary standard the single Daniell cell, and if R be expressed in meters of telegraph wire, then K=0.172175 and the value is expressed in fractions of a meter.

In referring K and R to the system of British Association units, K = 0.015957.

The diameter c being known, all the other elements can be easily determined for maximum conditions by means of the formulas previously given, which express the values of g, b, t and H.

To know the value of the attractive force A, it will suffice to determine the value of $I^2t^2c^{\frac{3}{2}}$, remembering that $I=\frac{E}{2R}$ and that the values of t and c are given by the formulas (6) and (10); and that to get this force expressed as weight it is necessary to refer to the standard electro-magnet which gives for a constant 0.0000855.*

The value becomes

$$(11)\qquad A=\frac{I^2t^2c^{\frac{3}{2}}}{0.0000855}\text{ grams.}$$

The important results deduced from formula (10) may be thus enumerated:

* This coefficient is the force of the standard magnet as expressed by the above formula, measured by grams; the force for a distance of 1 millimeter being 26.85 grams.

1. For equal resistances on the circuit, the diameters of electro-magnets of maximum effect should be proportional to the electro-motive forces employed.
2. For equal electro-motive forces, these diameters should be in the inverse ratio of the square roots of the resistances of the circuit and battery.
3. For equal diameters, the electro-motive forces should be proportional to the square roots of the resistances of the circuit.
4. For a given electro-magnetic force, and with electro-magnets under conditions of maximum effect, the electro-motive forces of the batteries should be proportional to the square root of the resistances of the circuit. (See formula 12.)

The preceding formulas also lead to the easy solution of many problems which frequently present themselves in practical applications of electricity, and particularly in calculating directly the force of a battery, and the dimensions of an electro-magnet to afford a given attractive force on a circuit of given resistance. It is true that the results obtained are not always in accord with the calculation, by reason of the nature of the iron core which may be more or less suitable for ready magnetization, and by reason of the more or less complete saturation of the magnet; but we can always get a fair approximation, and that is considerable gained.

In order to solve the problem in question, we will determine first the fourth deduction above mentioned, and proceed to demonstrate it. This is to determine a value of the electro-magnetic attractive force in terms of the electro-motive force of the battery and the resistance of the circuit.

If we consider the expression $I^2 t^2 c^{\frac{3}{2}}$ which represents this force, we find that by substitutions* in the values of I, t and c, it becomes

$$\frac{E^{\frac{9}{2}}}{R^{\frac{9}{4}}} \cdot \frac{Q}{f^2}$$

in which Q is a constant equal to 2228 (the Daniell cell being the unit of electromotive force, and the resistances being given in meters of telegraph wire).

Now, in estimating the value of the attractive force, we arrive at the relations

$$(12) \qquad \frac{E^{\frac{9}{2}}}{E'^{\frac{9}{2}}} = \frac{R^{\frac{9}{4}}}{R'^{\frac{9}{4}}} \text{ or } \frac{E}{E'} = \frac{\sqrt{R}}{\sqrt{R'}}$$

As in the values of E and R the numbers n and n' of the elements are found, we can easily calculate these numbers, knowing the values of the constants e and ρ of the element employed, for we have from the preceding equation:

$$\frac{ne}{n'e'} = \frac{\sqrt{n\rho + r}}{\sqrt{n'\rho' + r'}}$$

and if the accented quantities relate to those of the standard electro-magnet which are known, it is easy to deduce from the preceding formula the value of n, as it only requires the solution of an equation of the second degree with one unknown quantity.

If a liquid body sends vapor into an unlimited atmosphere there will proceed from each element of its surface, during a unit of time, a quantity of vapor proportional to an electric charge which is present and in equilibrium upon the element. *Les Mondes* says the lines of the vapor currents correspond to the lines of electric force, and the surfaces of equal vapor pressure to the surfaces of equal potential. The electric equilibrium of an infinitely small circular or elliptic plate is the electrostatie analogue of the problem of the evaporation of a liquid contained in a basin of circular or elliptic contour.

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* The substitutions referred to are:

$$I^2 = \frac{E^2}{4R^2}.$$

$$t^2 = \frac{E\sqrt{R}}{f} \; 128903.$$

$$I^2 t^2 = \frac{E^2}{f^2 R^{\frac{3}{2}}} \; 30976.$$

$$c^{\frac{3}{2}} = \frac{E^{\frac{5}{2}}}{R^{\frac{3}{4}}} \; 0.07195.$$

$$A = I^2 t^2 c^{\frac{3}{2}} = \frac{E^{\frac{9}{2}}}{f^2 R^{\frac{9}{4}}} \; 2228.$$

WATER-JET PROPELLERS.

By W. H. WHITE.

From "Nature."

Very early in the history of steam navigation, attempts were made to employ the "hydraulic" or "water-jet" propeller. About 1782 Rumsey began to work in this direction, using a steam-engine to force water out at the stern of a boat, the inlet being at the bow. His experiments are said to have extended over twenty years, but led to no practical result. Another American, named Livingston, applied the same principle of propulsion in a different manner. A horizontal wheel, or turbine, was placed in the bottom of the boat, near the middle of the length, the water was admitted from beneath it, and expelled from the periphery of the wheel through an opening at the after part of the boat. In 1798 a monopoly was granted to Livingston for twenty years by the State of New York, on condition that within a given period he produced a vessel capable of attaining the speed of four miles an hour. This condition was not fulfilled, and, as is well known, the first successful steamers built in this country or abroad were propelled by paddle wheels. This form of propeller alone was employed for nearly forty years, during which period steam-ships increased greatly in numbers, size and speed, proving themselves well adapted not merely for service on inland and coasting navigation, but also for ocean voyages. Just when the Transatlantic steam service had been successfully commenced by the *Great Western* and *Sirius*, both paddle steamers, the screw-propeller began to threaten the supremacy of the paddle-wheel; and the success of the *Archimedes* in 1840 led to the adoption of the screw in the *Great Britain*, as well as the construction of the screw sloop *Rattler* for the Royal Navy. Soon after came a revival of the water-jet propeller by the Messrs. Ruthven of Edinburgh. In 1843 their first vessel was tried, attaining a speed of about seven miles an hour. Ten years later a fishing-vessel was built on the same principle, and exceeded nine miles an hour. Several other river steamers and small craft were constructed with jet-propellers in the period 1853-65, but they were all comparatively slow, and the plan did not grow into favor either as a substitute for the paddle-wheel or the screw.

There were certain features in the jet-propeller which recommended it to the judgment of many naval officers who had witnessed the trials of vessels so fitted; their influence led the Admiralty in 1865 to order the construction of a small armored vessel, appropriately named the *Waterwitch*, which was to be fitted with Ruthven's propeller. Admiral Sir George Eliot was one of the principal advocates of a trial of the new system, in which he has always continued to take a great interest. In the German navy, trials of the Ruthven system have also been made on a small vessel named the *Rival*, and experiments of a similar nature have been made in Sweden. At the present time Messrs. Thornycroft are building for the Admiralty a torpedo boat, to be propelled by water-jets, the trials of which are awaited with interest, since they will furnish another comparison between the performances of the hydraulic propeller and the screw.

The Ruthven system agrees in its main features with the proposal made by Livingston forty years earlier. As an example the arrangements of the *Waterwitch* may be briefly described. Openings are made in the bottom of the ship amidships, to admit the water into a powerful centrifugal pump or turbine, the axis of which is vertical. The main engines drive the turbine, expelling the water with considerable velocity through curved pipes or passages leading to the "nozzles" placed on each side at the level of the water-surface. When the vessel is going ahead the jets are delivered sternwards; if it is desired to move astern the engines are not reversed, but valves are operated in the outlet pipes, and the jets are delivered through

the forward ends of the nozzles. These motions of the valves can be made from the deck by an officer in command. If desired, the jet on one side can be delivered ahead, and that on the other side astern, the vessel then turning without headway. This power of control over the movements of the vessel, without reversing the engines, is one of the chief advantages claimed for the system; and it is undoubtedly of value, especially in war-ships. Another advantage claimed for the jet-propeller is the power of turning it on an emergency, into a powerful pump, by which large quantities of water can be discharged from the interior of a ship that has been damaged in action. This latter feature cannot be regarded as of primary importance, however, seeing that modern war-ships are minutely subdivided into water-tight compartments, and must depend for their flotation upon the integrity of the bulkheads and other partitions, if their skins have been broken through by ramming or torpedo explosions. A further claim on behalf of the jet-propeller for war-ships is based upon the less risk of disablement in action, as compared with screws or paddle-wheels; and this claim may be admitted. On the other side must be set the fact that all the trials made hitherto in vessels fitted on the Ruthven system have shown a less speed for a given amount of engine-power than would have been obtained with the screw-propeller. It may be urged, of course, that the decrease in speed should be accepted, at least in special cases, in order to secure the undoubted benefit of the hydraulic system. But the general feeling of naval architects and marine engineers is in favor of the use of twin-screws rather than water-jets for war-ships, the duplication of machinery and propellers decreasing the risk of disablement, giving great maneuvering power, and securing higher speed than could be obtained with the jet propeller.

Recently further trials have been made with a vessel built in Germany, from the designs of Dr. Fleischer, who claims to have devised a novel and more efficient system of hydraulic propulsion. A brief notice of the invention appeared in *Nature*, vol. xxvi., p. 18,; fuller details are to be found in two pamphlets published by the inventor: "Der Hydromotor," and "Die Physik des Hydromotors" (Kiel, 1881). The first of these pamphlets contains a general description of the system, as applied in the *Hydromotor* (a vessel of 110 feet in length, and about 100 tons displacement), a summary of her trials, compared with those of earlier vessels engined on Ruthven's system, and an enumeration of the advantages to be obtained by using jet-propellers instead of screws or paddles. The second pamphlet contains a statement of the experimental and mathematical investigations conducted by Dr. Fleischer in working out his system.

Dr. Fleischer dispenses with a turbine, and allows the steam to act directly upon the water in two large vertical cylinders placed amidships. These two cylinders communicate with the ejecting nozzles which are situated on either side of the keel. In each cylinder there is a "float" or piston of nearly the same diameter as the cylinder, with a closed spherical top; when this float is in its extreme upper position, the cylinder is full of water. Steam is then admitted into the upper part of the cylinder above the float, the latter is pressed down, and the water is expelled through the nozzle-pipe with great velocity. At a certain portion of the stroke, the admission of steam is shut off automatically, the remainder of the stroke being performed during the expansion of the steam, and the velocity of ejection of the water gradually diminishing. At the conclusion of the stroke, the exhaust-valve from the steam space to the condenser is opened, the steam rushes out, forming a partial vacuum above the float, and the water enters, pressing the float up. The entry of the water at this stage is partly through the nozzle, and partly from a separate valve communicating with the water-space of the surface condenser. In order to utilize the vacuum as much as possible, and to increase the effective "head" of water during the down stroke, the cylinders are placed as high as convenient in the vessel. Two cylinders acting alternately were used in the *Hydromotor*, for larger or swifter vessels it is proposed to use a greater number of similar cylinders. As in other jet-propelled vessels valves operated from the deck enable the commanding officer to reverse the direction of outflow of either or both jets, making the vessel move ahead or astern, or turn on

her center. The position of the nozzles in the *Hydromotor* is not so favorable to maneuvering power as in the *Waterwitch*, and the difference in behavior is likely to be appreciable.

Greater interest attaches to the trials of speed than to those of turning. Unfortunately the records are too meager to enable a decisive opinion to be formed on the merits of the new system as compared with that of Ruthven. Dr. Fleischer claims that the *Hydromotor* attained a speed of 9 knots with 100 indicated horse-power; but the conditions under which this speed was attained may have differed considerably from those under which measured-mile speed trials are conducted in this country. Any exact comparison of the performances of two steam-ships with either similar or different systems of propulsion, demands as its basis the elimination of all varying conditions, the determination of the true mean speed, and the calculation of the engine power corresponding to that speed. Dr. Fleischer may have done all this, but it does not clearly appear in his publications whether he has or not. He distinctly claims for his system a very high "efficiency" as compared with that of Ruthven, but it will be shown hereafter that the formula which he uses is not absolutely correct; and what is more important to note is the circumstance that Dr. Fleischer clearly does not possess the experimental data respecting the resistance offered by the water to the motion of the *Hydromotor* when towed at various speeds, which would enable him to express the true efficiency of the propelling apparatus. On this point a few further remarks may be permitted.

Supposing a vessel to be towed at any speed, and her resistance to be ascertained by a dynamometer, the horse-power expended in overcoming that resistance can be calculated, and, in the terminology of the late Mr. Froude, is styled the "effective horse-power." Next let it be supposed that the vessel is driven at the same speed by her own machinery, and that the "indicated horse-power" in the cylinders is ascertained. The ratio of the "effective" to the "indicated" horse-power expresses the true efficiency of the propelling apparatus, excluding from the account, of course, the efficiency of the boilers. Now what has been said above respecting Dr. Fleischer's figures simply amounts to this: he does not appear to have ascertained the effective horse-power of the *Hydromotor*, and consequently cannot express the true efficiency except as an estimate.

The excess of the indicated horse-power over the effective in any steamship is to be accounted for by the waste work of the mechanism, the waste work of the propellers, and the "augment" of the tow-rope resistance produced by the action of the propellers. In good examples of screw steamers the effective horse-power at full speed has been found to vary from 40 to 60 per cent. of the indicated power. Dr. Fleischer claims for the *Hydromotor* a corresponding efficiency of about 34 per cent. at full speed; but not, it would seem, with any certainty.

Passing by this comparison with screw-propelled ships, the *Hydromotor* may be compared with the *Watewitch*. She gains upon the latter obviously in the avoidance of much waste work in the mechanism. In the Ruthven system there is necessarily more waste work in the engines which drive the turbines, and in the friction of the water in the turbines and passages to the nozzles, than has to be incurred in the Fleischer system. On the other hand, in the latter system, there must be some loss from condensation of steam in the cylinders, and the high mean velocity of ejection must be a disadvantage. The considerable variations in the velocity of ejection at different parts of the stroke must also be a disadvantage, as compared with the uniform velocity of delivery from a turbine. Respecting the condensation it is asserted, as the result of experiment, that the losses are exceedingly small, the cylinders being wood-lined, and a layer of hot water being formed below the float. Experienced engineers were scarcely prepared for this satisfactory result, anticipating that more serious losses would occur from the alternate heating and cooling of the cylinders. Of course, experience in such a matter is a true test; but it is to be observed that the *Hydromotor* appears to have very ample boiler power in relation to the indicated horse-power assigned to her maximum speed. Losses from condensation cannot be estimated from the statement

of indicated horse-power. The indicator diagrams which have been published, show a very good performance.

The varying rate of outflow through the nozzles must be a source of disadvantage in the Fleischer system. For the hydromotor it is stated that the *mean* velocity of outflow was about 66 feet per second when the speed of the vessel was about 15 feet per second. We are not informed what was the maximum velocity of outflow; the minimum velocity is said to have exceeded the speed of the vessel. This varying velocity, of course, carries with it a varying thrust, and the hydromotor in this respect must be less favorable to uniform motion of the ship than the screw or paddle or Ruthven propeller, where the thrust can be kept practically constant. With two cylinders this might be more felt than with four or more cylinders, but in all cases the drawback must exist.

The high mean rate of outflow involved in the Fleischer system is contrary to the generally accepted view as to the condition most favorable to efficiency. For a given speed of ship, neglecting the augment of tow-rope resistance which may be caused by the action of the propeller, there must be a certain thrust developed, which will overcome the resistance of the water to the advance of the ship. This thrust in the jet propeller is measured by the sternward momentum generated in the jets. No matter how the mechanism may be arranged, what has to be done by it is to impart to water which has entered the ship and acquired her forward velocity, a sternward momentum which shall have a reaction equal and opposite to the fluid resistance. Momentum, it need hardly be explained, involves the consideration both of the weight of the water acted upon and of the velocity imparted to it in each unit of time. Nor is it possible to create this momentum in the water expelled from the nozzles without doing waste work in overcoming frictional and other resistances. The magnitude of this waste work may vary greatly in different examples, and it is difficult to estimate its value apart from experiment. Hence in theoretical investigations, this waste work is usually neglected, although in practice it is of great importance.

Leaving out of account for the moment this waste work, and the possible influence upon the efficiency of the propeller exercised by the disturbance produced in the surrounding water by the passage of the ship, it may be well to explain briefly the accepted theory of the action of jet-propellers. This is done in the following equations:—

Let V=the speed of outflow of the jets from the nozzles in feet per second, v= the speed of advance of the ship, A = the joint sectional area of the nozzles in square feet, w=weight in lbs. of a cubic foot of water. Then—

$$\left.\begin{array}{l}\text{Cubic feet of water acted upon per second}\end{array}\right\} = A.\ V.$$

$$\left.\begin{array}{l}\text{Sternward velocity of jets in relation to still water}\end{array}\right\} = V-v.$$

$$\left.\begin{array}{l}\text{Thrust, or momentum}\\ \text{created per second}\end{array}\right\} = \frac{w}{g}.A.V.\ (V-v),$$

where g is the accelerating force of gravity—say 32 feet per second. For sea-water $w=64$; so that $w \div g=2$ (nearly). Hence

Thrust (in sea-water)$=2A.V.(V-v)$.

Under the foregoing assumptions, we also have

$$\left.\begin{array}{l}U=\text{Useful work of pro-}\\ \text{peller (in unit of time)}\end{array}\right\} \begin{array}{l}=\text{work done in propelling ship.}\\ =\text{Thrust}\times\text{speed of ship.}\\ =2AV(V-v).v.\end{array}$$

$$W=\text{waste work in race} \begin{array}{l}=\tfrac{1}{2}\ \textit{vis viva}.\\ =AV.(V-v)^2.\end{array}$$

$$\left.\begin{array}{l}U+W=\text{total work of}\\ \text{propeller}\end{array}\right. \begin{array}{l}=2AV(V-v)v\\ \quad+AV(V-v)^2\\ =AV(V^2-v^2).\end{array}$$

$$\text{Efficiency}=\frac{U}{U+W}=\frac{2v}{V+v}.$$

From the last of these equations it is seen that the more nearly the velocity of V approaches the speed of the ship v, the nearer will the efficiency approach its maximum value, or unit. Moreover, for given values of speed of ship and *thrust*, if the difference $(V-v)$ between the speeds of outflow and advance is diminished, the area of the outlets must be correspondingly increased. That is to say, if the value of $V-v$ is diminished, the *quantity of water* $(A.V)$ operated upon must be increased. Now, in general, it has been supposed that the inferior performance of jet-propelled vessels, as compared with screw steamers was due to the small quantities of water acted upon. In the *Waterwitch*, for example, about 150

cubic feet of water were expelled per second, whereas in the rival twin-screw vessel *Viper* more than 2000 cubic feet of water were operated upon per second. In the *Waterwitch* V=30 feet per second, and v=15.7 feet per second; so that according to the foregoing formula

$$\text{Efficiency}=\frac{2\times 15.7}{15.7+30}=\frac{31.4}{45.7}=68.7 \text{ per ct.}$$

In the *Hydromotor* V=66 feet (mean velocity) v=15.2.

$$\text{Efficiency}=\frac{2\times 15.2}{15.2+66}=\frac{30.4}{81.2}=37.4 \text{ per ct.}$$

Dr. Fleischer adopts the foregoing equations, so far as they relate to *thrust* and *useful work*, but for the *total work* he uses another formula, and it is here that we venture to think he goes wrong. According to his investigation—

Total work = $\frac{1}{2}$ *vis viva* of issuing streams.

= $\frac{1}{2}$ × Mass of water delivered per second × (speed of outflow)^2.

= $\frac{1}{2}$ × 2AV × V^2.

Hence he writes—

$$\text{Efficiency}=\frac{\text{Useful work}}{\text{Total work}}=\frac{2AV(V-v)}{2AV\times\frac{1}{2}V^2}$$
$$=\frac{2V}{v^2}(v-v).$$

In thus dealing with the total work, instead of using the expression given above, Dr. Fleischer virtually ignores the fact that the vessel is in motion ahead; and that the streams issuing from the nozzles have the velocity V only relatively to her. It is upon this questionable formula for the efficiency that his estimates above-mentioned are based. For example, in the hydromotor at 9 knots, according to Dr. Fleischer—

$$\text{Efficiency}=\frac{2\times 15.2}{(66)^2}(66-15.2)=35.4$$

per cent.

If the same formula is applied to the *Waterwitch*, at 9.3 knots—

$$\text{Efficiency}=\frac{2\times 15.7}{(30)^2}(30-15.7)=49.9$$

giving about 20 per cent. less efficiency to that vessel, than is given by the accepted formula first stated.

It has been explained that the assumptions upon which the first formula rests are not fairly representative of the conditions of practice. For example, the deduction therefrom (stated above), that it is advantageous to operate upon larger quantities of water, and to reduce the excess in speed of outflow above the speed of the ship requires an important qualification in practice. This deduction would be absolutely correct were it not for the waste-work which has to be done in giving the motion to the water; but in actual practice the growth in that waste work may exceed the gain obtained by dealing with larger quantities of water. The parallel case in a screw steamer is that wherein screws of too large diameter or too large surface may involve so much more waste work on frictional or edgewise resistances, that it is preferable to use smaller screws, which operate on smaller quantities of water, but secure a more economical expenditure of power for a given speed, or enable higher speeds to be attained with a given horse-power. In setting aside the commonly received view, and making trial of a system wherein the mean velocity of the outflowing jets is extremely great, while the quantity of water operated on is small, Dr. Fleischer has made an experiment of the greatest interest to all concerned with steam propulsion. If his figures are accepted it is obvious that his system involves much less waste work than the Ruthven system between the power indicated in the cylinders and the power accounted for in the outflowing jets. On the other hand, as we have endeavored to explain, this economy of the Fleischer system does not represent the comparative efficiency of the propelling apparatus: because the high and variable velocity of outflow must involve a considerable amount of waste work in the race. A complete comparison could only be made if in the same vessel, or in two vessels of identical form and with identical boiler power, there were fitted, first, the Fleischer hydromotor; and secondly, the Ruthven arrangement. Then with the same steam producing power a careful series of trials would settle the matter conclusively. The Swedes did something of this kind in order to compare the efficiencies of twin screws and water jets, with the result that the latter were shown to be greatly inferior. Of course it cannot be expected that Dr. Fleischer would undertake such trials unaided; on the other hand, if his system is put forward for adoption in preference

to the Ruthven system, it must, at least, be shown to be more efficient, not only in certain intermediate stages in the operations of giving momentum to the jets, but as a whole. This result does not appear to have been attained as yet, so far as can be judged from the published results of trials. The information which is accessible is not complete, and some of the proposed standards of comparison are open to doubt. It is to be hoped, however, that the zeal and ability which have been displayed already by Dr. Fleischer will be still further illustrated in the continued investigation of the capabilities of his novel system of propulsion.

COMPARATIVE TEST OF BOILERS.

MADE AT THE WORKS OF THE BRUSH ELECTRIC LIGHT COMPANY OF PHILADELPHIA.*

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

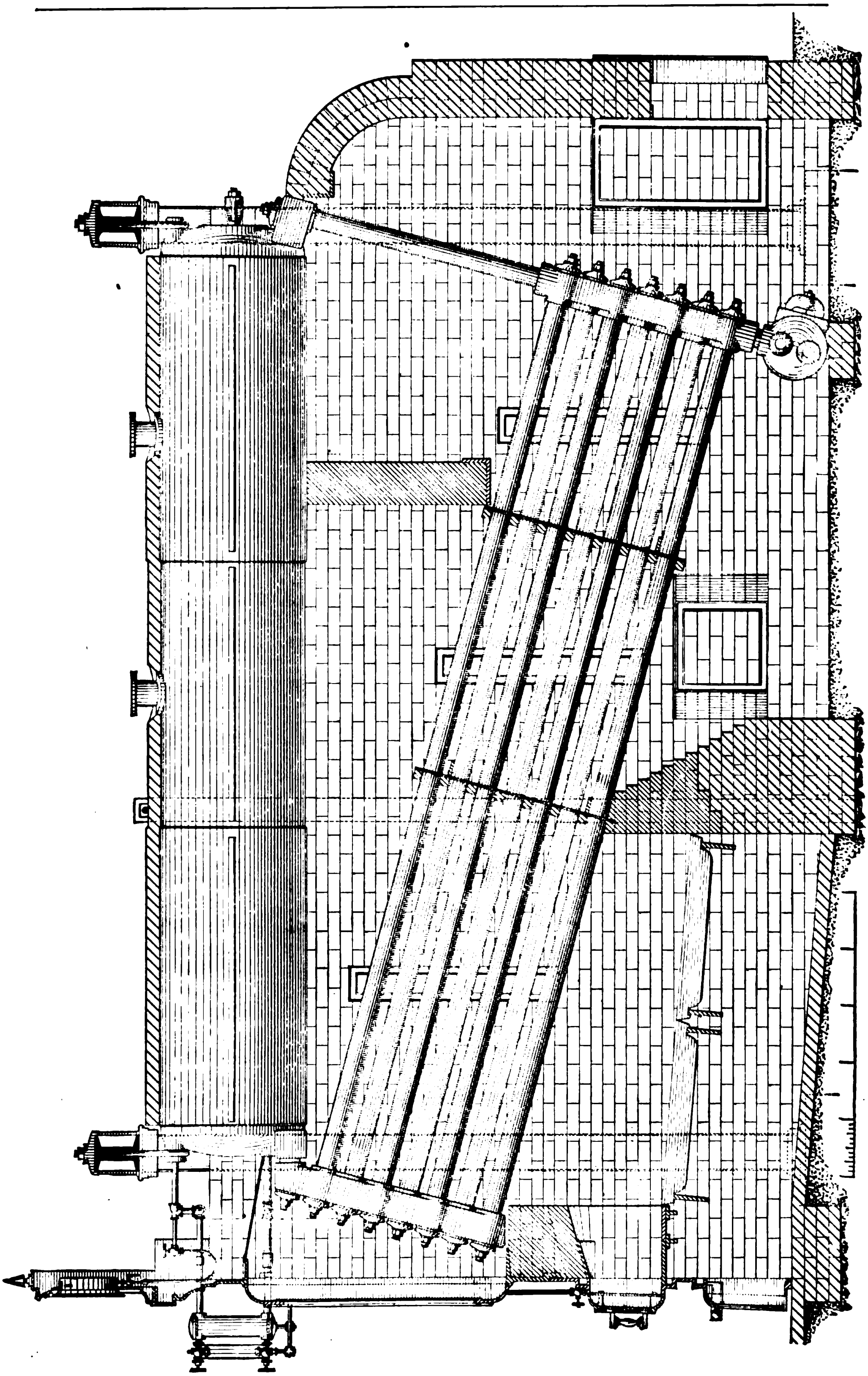
THE boiler house of the Brush Electric Light Company, where these tests were made, is situated on Twentieth street, near Chestnut street, Philadelphia. It contains ten boilers, four of the well-known sectional water-tube boilers made by the Babcock & Wilcox Company, and six return-tubular boilers, made to the specifications of the Brush Electric Light Company.

The Babcock & Wilcox boilers are set in two nests, two in each nest, but have separate furnaces, and can be used separately or in any desired combination. Each boiler contains 42 tubes 4 in. outside diameter and 16 ft. long. The arrangement of the tubes and the setting of these boilers, are too universally known to call for particular description here. The fire-grates of each boiler are 46 in. wide and 72 in. long, plain cast-iron bars, case singly, having an area of 23 sq. ft., of which about 40 *per cent.*, say 9.2 sq. ft., is open.

The heating surface is reckoned to be 800 sq. ft. in each boiler. Exclusive of some parts not very efficient, there are 796 sq. ft., giving a ratio of heating surface to grate-area of $\frac{796}{23}=$ 34.6. The steam drum is horizontal, 30 in. in diameter and 16 ft. long, and the surface of the water was carried at the middle of its height, which was at 6 inches depth in the tube of the glass water gauge. The water surface for disengagement of steam is, therefore, 40 sq. ft. in each boiler. From the top of these drums, near the back end, steam is carried to the rear in separate pipes through the wall, which divides the engine room from the boiler room and enters a pipe extending the whole length of the engine room to receive steam from all the boilers and to supply all the engines. A separate pipe supplies steam to each engine, and all these pipes are, at all times, full of steam up to the starting valves of the engines, although but a small number of the engines may be in use. All steam pipes are wrapped with hair felt and covered with cloth and painted.

The six return-tubular boilers are set in one nest, and so arranged that they can be used singly or in any combination, either with each other, or with one or more of the Babcock & Wilcox Company's boilers. Each of these six boilers is 40 in. in diameter outside of the first course in front, and as each succeeding course enters the one which precedes it, (all being cylindrical), the boiler grows gradually smaller by an amount equal to twice the thickness of the iron at each of the five 40 in. courses, the last course being 37½ in. in diameter outside. Each boiler is hung in a saddle, and is set six inches lower at the rear than in front. There are in each boiler 19 flues, 4 in. outside diameter and 16 ft. 11½ in. long over the end caulking. The fire-grate is 42 in. wide and 67 in. long, making 19.54 sq. ft. area, composed of single cast-iron bars as in the other boilers, and like them, having about 40 *per cent.* of the area open, say 7.8 sq. ft. each. The heating surface, per boiler, is 428 sq. ft.,

* Conducted by J. C. Hoadley, assisted by Fred. H. Prentiss, on the part of the Babcock & Wilcox Company; and Wm. Barnet Le Van, assisted by John W. Nystrom, on the part of the Brush Electric Light Company, Oct. 18, 19, 20, 23, 24, 25, 1882.



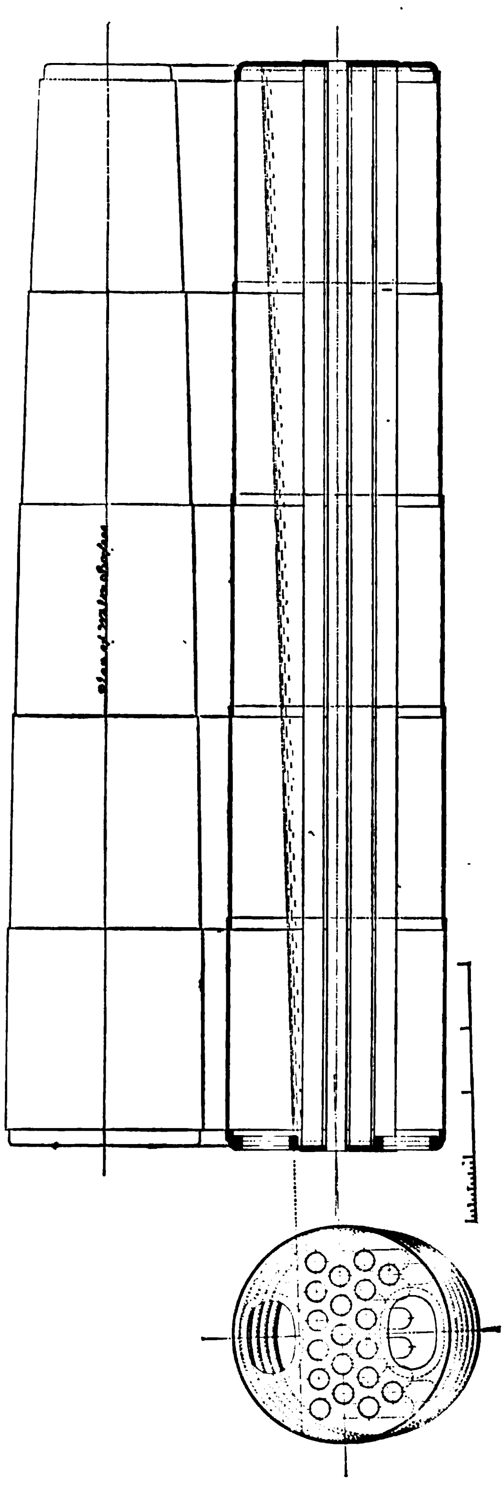
giving a ratio of heating surface to fire-grate area $= \dfrac{428}{19.54} = 21.9$. The usual height of the water was about 8 in. above the axis of the boiler at the front end, and about 2 in. above the top of the flues at the front. It was, also, about 2.5 in. above the lower end of the glass tube of the water gauge. The capacity and area of surface at each 1 in. of height visible at the glass water gauge, was ascertained by filling the boiler with water, which was drawn off and weighed inch by inch. The area of surface for disengagement of steam is 43.4 sq. ft. in each boiler. The inclination of the axis of the boiler and its telescopic form, combine to reduce the steam space at the rear, and for this reason steam is taken (there is no dome of any kind) from the front end, and conveyed in pipes extending over the boilers to and through the wall of the engine room, and discharged into the long steam pipe before described. The leading dimensions and ratios are collected in the following table:

COMPARATIVE TABLE.

	Babcock & Wilcox.		Return Tubular.
Heating surface, each....	796		428
Do. 2 B. & W. 3 Ret. Tub.	1592.		1284.
Fire-grate area, each	23.		19.54
Do. 2 B. & W. 3 Ret. Tub.	46.		58.62
Ratio heating to grate area	34.6		21.9
Sq. ft. per H.P., at 150 H. P	10.61		8.56
Ratio of heating surfaces.	1.24	to	1.
Ratio of grate areas......	1.	to	1.27
Evaporat'g water-surface, each..................	40		43.4
Do. 2 B. & W. 3. Ret. Tub. sq. ft.................	80		130.2
Ratio of water-surface....	1	to	1.63

THE ENGINES.

Eight Porter-Allen engines of 40 horse-power—8″×16″, speed 285 to 295 revolutions per minute—were belted separately to eight dynamo machines, each of which when in full work supplied electricity for 40 arc lights of nominal 2,000 candle power, and occasionally an extra "test light," making 41. As already explained, the steam-pipes of all these engines were filled with steam whenever any one of them was in use, that is to say they were always filled with steam, day and night, and the condensation being constant, was a larger proportion of the steam used during the early hours of



the day—7.30 A. M. to 3 P. M.—when only 3 engines were running, than at night—7 to 11 P. M.—when 6 to 8 engines were running.

All the distribution valves, save one of those in Engine No. 4, leaked steam very badly, and the other valve of that engine leaked nearly as much as any valve in any of the other engines run during the test. This leakage was visible (in its effects) on the diagrams during expansion and compression, and was computable during exhaust, when it was pouring directly through the exhaust valve, only slightly raising the back-pressure. About 10.43 pounds of water per horse-power per hour disappeared by this leakage, during the 18th, 19th and 20th Oct., and 12.33 during the 23d, 24th and 25th, raising the consumption of water drawn from the B. & W. Co. boilers from 36.14 lbs. to 46.57 lbs., an augmentation of 28.9 *per cent.*, which gave no power, but slightly increased the resistance. That any such leakage of these valves is wholly unnecessary and gratuitous, is within Mr. Hoadley's personal knowledge, obtained by the examination of many diagrams from engines of this construction. Indeed, some of the 8 engines were put in order by the builders before the trial began, and were said to have tight valves, but none of the engines so treated except one (No. 4) were among those used during the trials, and this was again taken apart before the trial, after it was left by the builders.

The effect upon the boilers of this leakage, was to impose on them a great increase of duty, amounting to nearly one-third—and requiring them to be forced much beyond their rated capacity. They were required to evaporate, and did actually evaporate 6054 lbs. of water per hour. Without the leakage of the distribution valves, they would have been called on for less than 4307 lbs. per hour, as the entrained water would then have fallen from 3.15 *per cent.* to about 2.36 *per cent.*, a saving of 34 pounds per hour.

Leakage of steam during exhaust, not visible on diagrams; computed from the visible leakage during expansion:

Engine No. 1=339 lbs. per hour.
" No. 2=673 " " "
Engine No. 3=424 lbs. per hour.
" No. 4=263 " " "

No. 4 leaked only at one end.

Desiring that the question of the efficiency and economy of these two sets of boilers should be settled on its merits, the Babcock & Wilcox Company arranged with the Brush Electric Light Company, of Philadelphia, that a comparative test of the two sets of boilers should be made in a manner that should be satisfactory to all concerned, and conclusive as to their relative value. Accordingly, Mr. Wm. Barnet Le Van of Philadelphia was selected by the Brush Electric Light Company as their representative, and Mr. John C. Hoadley, of Boston, was chosen on the part of the Babcock & Wilcox Company.

It was further agreed that the conditions of the tests should be as nearly as possible alike in both cases, and that a double comparison should be made in the following manner:

First. A careful evaporative test of each of the two sets of boilers, embracing a determination as accurate as possible of the quality of the steam as to "dryness;" and

Secondly, A comparison of the work done by the one set with that done by the other, as shown by the indicated power of the engines.

With this, such other information was to be obtained as might be useful or confirmatory.

It was accordingly agreed between Mr Le Van and Mr. Hoadley, with the concurrence of the engineer of the Brush Light Company, that exactly similar tests should be made of the two sets of boilers, each set to be tested for three consecutive days, between the hours of 7.30 A. M. and and 2.30 P. M., during which hours only three of the engines and three dynamos would be in use, supplying 120 arc lights for actual illumination and 1 test light, making 121 in all; and one engine running with throttled steam, at less than half full speed, driving a dynamo without current, and giving no light, merely to be in readiness for immediate use if wanted for an emergency.

Such a test of the Babcock & Wilcox boilers was in fact carried out on Wednesday, Thursday and Friday, Oct. 18,

19 and 20, 1882, save that on Wednesday the 18th, these boilers continued to supply steam until 3 P. M., half an hour later than the stipulation.

On the following Monday the task of supplying the engines with steam was put upon 3 of the Return Tubular boilers, a task which they performed on Monday, Oct. 23d, until 2.30 P. M.; but on Tuesday and Wednesday, Oct. 24 and 25, on account of an unusual demand for light in the early hours of the afternoon, and the consequent starting up of the spare engine and dynamo, with 11 arc-lights for illumination and 1 test light, making 12, and 134 in all, the test on these two days was discontinued at 12 o'clock M., and embraced only 4 hours and 30 minutes each day. The Babcock & Wilcox boilers were therefore run for trial 21.5 hours on three consecutive days, and the Return Tubular boilers only 16 hours.

The coal furnished for these tests was selected and supplied by the engineer of the Brush Electric Light Company, and so far as size alone is concerned was what is usually called chestnut. The coal used on the 18th, 19th and 20th Oct. with the B. & W. boilers was *unscreened*, dull looking and extremely dirty and wet. No objection was made to it, because we supposed that it would be no worse in the one case than in the other.

On the contrary, the coal furnished and used on the 23d, 24th and 25th Oct., with the Return Tubular boilers, was screened, bright and noticeably clean and dry; and there was besides some rescreening during the night, but Mr. Peifer, engineer of the Brush Electric Light Company, says, "not much." One hundred pounds of the former, put on top of the boilers, lost 7.37 lbs. in drying, equal to 7.37 *per cent.* of surface water. One hundred pounds of the latter, similarly treated, lost only 2.87 lbs. in drying, equal to 2.87 *per cent.* at surface water, a difference of 2.57 to 1.*

The data, observations and information obtained by Mr. Hoadley and Mr. Le Van and their several assistants, and attested by the signature of both parties to each day's record, consisted of the following particulars:

(1) Weight of water pumped into boilers.

(2.) Weight of coal and kindlings put into furnaces.

(3.) Weight of ashes and residue at end of test.

(4.) Temperature of feed water as it left the heater, recorded every fifteen minutes.

(5.) Steam pressure by standard test gauge, recorded every 15 minutes.

(6.) Temperature of products of combustion (flue gases), taken every 15 minutes by an air thermometer.

(7.) Indicator cards from both ends of each engine, taken every 30 minutes; a pair of Tabor or Crosby indicators being attached to each cylinder.

(8.) Speed of engines, taken with a speed-counter.

(9.) Occasional volumetric analysis of flue gases.*

(10.) Occasional pyrometric measurements of the temperature of the incandescent coal, by the platinum-water pyrometer.

(11.) Numerous determinations of the quality of the steam by the steam calorimeter.

(12.) Careful ascertainment by measurement and inquiry, of all particulars of dimensions and arrangement of the respective boilers.

(13.) Ascertainment of the number of arc-lights in use, supplied by the dynamo driven by each engine; obtained from the Secretary of the Brush Electric Light Company of Philadelphia.

Each and all of these observations, together with their aggregates and mean values have been compared and mutually agreed on as correct by Mr. Le Van and Mr. Hoadley through their assistants who kept the notes, respectively, and each page of transcript of such compared notes has been signed by both parties.

Upon these values, so verified, the calculations of the results which follow have been made.

Although a marked contrast in the

* On this subject of the extreme difference in the quality and condition of the coal used in the tests of the two sets of boilers reference is respectfully made to a joint statement of facts and joint protest drawn up at the time and signed by Mr. Le Van and Mr. Hoadley, Mr. Le Van confirming the allegations of said statement, by reference to his note-book, (not here given, but to be obtained on application to Mr. Hoadley.—J. C. H.

* Mr. Hoadley is alone responsible for the notes under (9) and (10).—J. C. H.

quality of the coal used with the two sets of boilers was apparent at a glance, and still more apparent on careful inspection; yet no allowance has been made on this account except for the ascertained difference in the quantity of surface water, as above explained.

The results of the determinations of entrained water in the steam, of the analysis of flue gases, and their temperature, and the temperature found in the heart of the fire, will be given below.

The height of barometer for each day, was obtained from the Chief of the Signal Service in Washington, as reported from Philadelphia.

Due allowance and correction have been made for all variations in the height of water in water-gauge glass and for all difference in steam pressure at beginning and end of each day's experiment.

Each pound of wood used for kindling has been reckoned equal to $\frac{4}{10}$ of a pound of merchantable coal, say with 10 *per cent.* of ash, and therefore equal to $\frac{36}{100}$ of a pound of combustible.

Each pound of cotton waste used for starting fires, has been considered equal to a pound of combustible.

EVAPORATIVE TEST: OCT. 18, 19, 20.

Babcock and Wilcox boilers: 21.5 *hours.*

16388.5	pounds of wet coal thrown on grate.
1207.8	pounds of surface water in coal.
15180.7	pounds of dry coal thrown on grate.
462	pounds of wood used for kindling.
72.5	pounds of cotton waste, for starting fires.
3305	pounds of ashes and residue.
11875·7	pounds of combustible (in coal) consumed.
166.3	pounds of combustible = 462 lbs. of wood.
72.5	pounds of combustible = cotton waste.
12114.5	pounds of combustible consumed.
134410015	* B. t. u. apparently received by boiler.
130176100	B. t. u. actually received —water allowed for.
10745.48	B. t. u. received per pound of combustible.
11.127	pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
74.18	*per cent.*, apparent efficiency.
1497793	B. t. u. required to dry the coal.
0.128	pounds of water evaporated from and at 212° F. per 1 lb. of combustible, equal to drying the coal.
11.255	pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
75.03	*per cent.* actual efficiency.

* British Thermal Units.

EVAPORATIVE TEST: OCT. 23, 24, 25.

Return Tubular Boilers: 16 *hours.*

13171.5	pounds of screened coal thrown on grate.
378	pounds of surface water in coal.
12793.5	pounds of dry coal thrown on grate.
319	pounds of wood used for kindling.
34.5	pounds of cotton waste, for starting fires.
2697	pounds of ashes and residue.
10096.5	pounds of combustible (in coal) consumed.
115	pounds of combustible = 319 lbs. of wood.
34.5	pounds of combustible = cotton waste.
10246	pounds of combustible consumed.
106300397	B. t. u. apparently received by boiler.
104110609	B. t. u. actually received. water allowed for.
10161.1	B. t. u. received per 1 lb. of combustible.
10.522	pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
70.15	*per cent.* apparent efficiency.

482555 B. t. u. required to dry the coal.

0.049 pounds of water evaporated from and at 212° F. per 1 lb. of combustible expended in drying the coal.

10.571 pounds of water evaporated from and at 212° F. per 1 lb. of combustible.

70.47 *per cent.* actual efficiency.

COMPARATIVE ECONOMY BY THE EVAPORATIVE TEST.

$11.255-10.571=0.684$; and $\frac{.684}{10.57}=.0648$ $=6.47$ *per cent.*

ENGINE TEST: OCT. 18, 19, 20.

Babcock and Wilcox Boilers: 21.5 *hours.*

130.41 mean indicated horse-power.

21.5 hours, duration of experiments.

12114.5 pounds of combustible consumed.

563.46 pounds of combustible consumed per hour.

4.321 pounds of combustible consumed per h. p. per hour.

130156. pounds of water evaporated.

6054 pounds of water evaporated per hour.

46.57 pounds of water evaporated per h. p. per hour.

45.1 pounds of dry steam per h. p. per hour.

10.43 pounds leakage per h. p. per hour.

34.67 pounds of dry steam *used* per h. p. per hour.

ENGINE TEST: OCT. 23, 24, 25.

Return Tubular Boilers: 16 *hours.*

137.78 mean indicated horse-power.

16 hours, duration of experiments.

10246 pounds of combustible consumed.

640.375 pounds of combustible consumed per hour.

4.648 pounds of combustible consumed per h. p. per hour.

104562. pounds of water evaporated.

6535 pounds of water evaporated per hour.

47.43 pounds of water evaporated per h. p. per hour.

46.45 pounds of dry steam per h. p. per hour.

12.33 pounds leakage per h. p. per hour.

34.12 pounds of dry steam *used* per h. p. per hour.

COMPARATIVE ECONOMY BY THE ENGINE TEST.

$4.648-4.321=0.327$, and $\frac{.327}{4.321}=.0757=$ 7.57 *per cent.*

EXPLANATIONS OF THE LIGHT TEST.

(*See Table next page.*)

1. The engines were at all times run under the direction of the engineer of the Brush Electric Light Company. No interference was at any time attempted with the selection of the engines to be run, the number to be used, or the speed at which they were to run.

2. Engine No. 1 was run on Oct. 18, 19 and 20, at rather less than half its full speed, with the throttle-valve only very slightly opened, merely driving the shaft of the dynamo, without current, consuming a quantity of steam, small in itself, but disproportionate to the work done in overcoming friction, and by so much increasing the quantity of combustible per hour for each indicated horse-power, and in a more marked degree the quantity consumed per hour for each arc light. Since this engine consumed *some* steam and produced *no* light, the combustible per light was, so far as this engine was concerned, infinite. There were doubtless good reasons for running it in this manner, as already explained.

On Oct. 23, the first day of the test of the Return Tubular Boilers, this engine was indeed kept in motion, but at so low a speed, produced by extreme throttling, that its indicated power fell off from 1.31 H.P. to 0.54—only 41 *per cent.* as much. This change affected injuriously, but in a very small degree, the economy of the engine test, but favorably, and in a greater degree (though still only a little), the light test. On Oct. 24 engine No. 1 was made to develop nearly 27 H.P. in order to supply 11 regular lights and a test light, 12 in all, and No. 5 and started at a very low speed,—throttled—giving only 0.36 H.P., only 27 *per cent.* as much as

LIGHT TEST, OCTOBER 18, 19, 20,—23, 24, 25.

1882. Number of engine.		Babcock and Wilcox boilers.			Return Tubular boilers.		
		Oct. 18. H.P.	Oct. 19. H.P.	Oct. 20. H.P.	Oct. 23. H.P.	Oct. 24. H.P.	Oct. 25. H.P.
Indicated power H.P.	1	1.34	1.30	1.30	0.54	26.86	27.04
	2	43.05	43.60	40.89	39.95	37.73	38.55
	3	42.20	41.85	42.46	40.02	39.51	41.69
	4	44.16	45.57	43.29	41.83	42.19	42.58
		..	..	..	..	0.36	0.00
Sums..................		130.75	132.32	127.94	122.34	146.65	149.86
True totals...........		130.78	132.71	127.72	123.36	147.15	150.83
Number of 2000 candle lights.	1	0	0	0	0	12	12
	2	40	40	40	41	41	41
	3	40	40	40	41	41	41
	4	41	41	41	40	40	40
	5	0	0	0	0	0	0
Totals.................		121	121	121	122	134	134
Indicated power per arc light. H.P.	1	..	..	..	..	2.2383	2.2533
	2	1.0763	1.0900	1.0223	0 9744	0.9202	0.9402
	3	1.0550	1.0462	1.0615	0.9761	0.9637	1.0168
	4	1.0771	1 1115	1.0506	1.0458	1.0547	1.0645
Mean.................		1.0808	1.0968	1.0555	1.0111	1.0981	1.1831

Grand mean:		B. & W. Co.		Ret. Tub.
	Power.........................	130.41	..	137.78
	Hours run....................	21.5	..	16.
	Lights run....................	121.	..	128.75
	H.P. per light..............	1.0703	..	1.0701
	Combustible per light per hour..	4.6567	..	4.9738

COMPARATIVE ECONOMY BY THE LIGHT TEST.

$$4.9738-4.6567=.3171\,;\ \text{and}\ \frac{.3171}{4.6567}=.0681=6.81\ \text{per cent.}$$

was developed by No. 1 on Oct. 18, 19 and 20. Further slight diminution of engine economy, and increase of light economy. On Oct. 25, the last day's test of the Return Tubular Boilers, this spare engine disappears altogether. Although it continued running at a very low speed, the diagrams were a mere line, and were discontinued.

3. On Oct. 23, the total power falls off to 123.36 I.H.P. from a mean of 130.41 I.H.P. for the first 3 days, a reduction of 5.41 *per cent.*, largely due to a reduction of speed of engines No. 2, 3 and 4. The effect of this reduction of speed on the power developed was direct and apparent. Its effect on the *lights*, merely a reduction of their intensity and whiteness, may or may not have been perceived, commented on, or complained of. Their actual illuminating power was certainly a little reduced by this lowering of speed. Of the motives for this change of speed, we know nothing, and speak only of its obvious and necessary effects. The most obvious effect was to reduce the mean power of engines No. 2, 3 and 4, per light, for Oct. 23, 24 and 25, to a little under 1 indicated H.P. (0.9952) a reduction from the mean of Oct 18, 19, 20, (1.0803 I.H.P.) of 7.92 *per cent.* But a sudden demand for extra lights during the Penn Bicentennial, made it necessary to start up another dynamo, although with only 11 lights for illumination and 1 test light making 12, just 30 *per cent.* of 40 lights, yet actually

causing the development of almost 27 horse power (26.95 I.H.P. mean of Oct. 24, 25, engine No. 1), which is 62 *per cent.* of the full power of engines No. 2, 3 and 4 on Oct. 18, 19 and 20, when producing currents to supply 40⅓ lights each at the higher speed. The inevitable result was to augment the power per light for engine No. 1 on Oct. 24 and 25, in the compound ratio of these two numbers; $.62 \div .3 = 2.067$ and $1.0803 \times 2.067 = 2.2329$, which is substantially the power per light for this engine (2.2383, and 2.2533). Low as are the several quantities of power per light for engines No. 2, 3 and 4, during the last three days, yet, adding in No. 1 brings up the mean to almost exactly the same as on the 3 first days (1.0703 and 1.0701 H.P.) — although the *light* was certainly less. It should be mentioned that the speed of No. 1 was not altered; it ran, when producing light, at the speed at which Nos. 2, 3 and 4 ran on the first 3 days.

4. It will be noticed that the total power appears, in the light test, in two lines, with slight differences. The first line gives the sums of the figures above, as they stand. The lower line gives the geometrical or true mean of the horse powers for each days run.

5. The result of the light test, is to be found in the relations to each other at the final figures. The Babcock & Wilcox boilers consumed 4.6567 pounds of combustible per hour for each light burning. The Return Tubular boilers consumed 4.9738 pounds of combustible per hour for each light burning. The difference, $4.9738 - 4.6567 = .3171$, and .3171 divided by 4.6567 gives as a quotient .0681, equal 6.81 *per cent.*, which is the loss of the Return Tubular boilers as compared with the Babcock& Wilcox boilers by the light test.

Having now applied three tests, namely: (1) water evaporated, (2) power developed, and (3) light produced, all per 1 pound of combustible consumed, it remains to apply the fourth, by comparing the heat carried off by the gases of combustion escaping to the chimney from the two sets of boilers respectively. The flue gases gave, by volumetric analysis, the following parts in 100 of dioxide of carbon (carbonic acid), resulting from the complete combustion of carbon.

CARBON DIOXIDE IN FLUE GASES.

Babcock & Wilcox Boilers.			Return Tubular Boilers.		
Day in October, 1882.	By Volume. Parts in 100.	By Weight. Parts in 100.	Day in October, 1882.	By Volume. Parts in 100.	By Weight. Parts in 100.
18	7.20	10.65	23	7.00	10.36
19	8.00	11.84	24	7.70	11.40
20	7.80	11.54	25	7.30	10.80
Mean,	7.67	11.34	Mean,	7.33	10.85

Babcock & Wilcox Boilers:

Flue gases per 1 lb. of combustible
$= \frac{95}{11.34 \times \frac{3}{11}} = 30.71.$
Mean temperature of flue gases, observed, 467° F.
$30.71 \times .238 = 7.309$ lbs. water. Equivalent evaporation at and from 212° F.
$= \frac{7.309 \times (467-60)}{965.7} = 3.081$ lbs.
$\frac{3.081}{15} = 20.54$ per cent. *loss.*

Return Tubular Boilers:

Flue gases per 1 lb. of combustible
$= \frac{95}{10.85 \times \frac{3}{11}} = 32.10.$
Mean temperature of flue gases, observed, 543° F.
$32.10 \times .238 = 7.64$ lbs. water. Equivalent evaporation at and from 212° F.
$= \frac{7.64 \times (543-60)}{965.7} = 3.821$ lbs.
$\frac{3.821}{15} = 25.47$ per cent. *loss.*

15 lbs. of water evaporated from and at 212° F., being the full evaporative power of 1 lb. of our combustible.

LOSSES OF HEAT AND EFFICIENCY.

	Babcock & Wilcox Boilers. Parts in 100.	Return Tubular Bolers. Parts in 100.
Loss of heat, carried off by heat gases..........	20.54	25.47
Loss by imperfect combustion, and radiation.....	4.43	4.06
Aggregate losses..........	24.97	29 53
Actual efficiency..........	75.03	70.47
Total heating power of combustible..........	100.00	100.00

Loss carried off by hot gases, Ret. Tub. boilers................. 25.47%
Loss carried off by hot gases, B. & W. boilers......... .. .... 20.54%

Difference; greater loss by Ret. Tub. boilers................. 4.93

This difference, or excess of heat lost by the Ret. Tub. boilers, divided by the efficiency of these boilers (70.47 *per cent.*), gives the ratio of the excess of loss to actual efficiency:

$$\frac{4.93}{70.47} = .06996 = 7.00 \textit{ per cent.}$$

SUMMARY OF RESULTS BY THE FOUR METHODS.

	Babcock & Wilcox Boilers.	Return Tubular Boilers.	Difference in favor of B. & W. Boilers.	Difference *per centum.*
Evaporative test.....	11.254	10.570	.684	6.47
Power, engine test..	4.321	4.648	.327	7.57
Light test...........	4.6567	4.9738	.3171	6.81
Test by loss at chimney..............	20.54	25.44	4.9	7.00
Mean of four tests..				6.96

Explanation of Table.—The Babcock & Wilcox boilers evaporated *more* water for each pound of combustible consumed; consumed *less* combustible per hour for each indicated horse power produced; consumed *less* combustible per hour for each arc light in use; and lost *less* heat by hot gases escaping to the chimney, than the Return Tubular boilers. The smallest ratio of difference is 6.47 *per cent.;* the largest ratio of difference is 7.57 *per cent.;* and the mean ratio of difference in favor of the Babcock & Wilcox boilers, is 6.96 *per cent.*

While doing this, they were evaporating 6054 pounds of water per hour, into steam, containing only 3.15 *per cent.* of entrained water, leaving 5863 pounds of dry steam per hour, enough at the rate of 30 pounds of dry steam per hour for each horse power, to supply 195 horse power which is 30 *per cent.* above their rated power.

This comparison leaves out of view all disparity of coal save the ascertained difference in surface water.

TEMPERATURE OF THE GASES OF COMBUSTION.

Without actual analysis of the coal used, it is impossible to say just what was the composition of the "combustible," so called, meaning thereby all the coal and the coal-value of kindlings thrown on the fire-grates, less the surface-water and the ashes and residue left at the close of each experiment. It is certain that "combustible" so ascertained, included (1) some water held in the coal in a manner not permitting it to be dried away, as surface water, and (2) some ash carried along with the escaping gases, and deposited in the flues and chimney. These are small in amount, and not being accurately ascertainable are not here taken into account. There was also, certainly a small quantity of sulphur, oxygen and nitrogen, not to be separately discriminated, amounting in the aggregate to about 2 *per cent.;* and a small quantity, probably $1\frac{3}{4}$ to $1\frac{7}{8}$ *per cent.* of hydrogen, perhaps combined in some form with carbon, but of some value as a fuel. Basing my judgment on numerous analyses of coal in other cases, I estimate the full heating power of the "combustible" to be equal to that of carbon, 14500. British thermal units equal to the evaporation of 15 pounds of water at 212° F. into steam of the same temperature under the pressure of one atmosphere; and that 95 *per cent.* of this combustible was carbon, and 1.15 per cent. free hydrogen. I also estimate that there was also a loss of 1 *per cent.* in consequence of imperfect combustion. resulting from the presence in the flue gas of a very small trace of carbon monoxide, (CO).

We have then full heating power of 1 lb. of "combustible," B. t. u.	14500.
Less for imperfect combustion, 1 *per cent*....................	145.
Heat actually produced per 1 lb. of combustible...............	14355.
*Water in coal actually ascertained, 2.87 *per cent*.............. =	.0287
Water formed by combustion of hydrogen in coal, 9×.0115... =	.1035
Water in coal, and formed by combustion of hydrogen..........	.1322
B. t. u. in 152° F. (60° to 212° F.) =	152.9
From this we must subtract the product of 152° by .48 (=specific heat of steam), as it appears in another place, later, and 152.×.48 .... =	72.9
	80.

* Test of Ret. Tub. boiler, Oct. 25. All relating to the temperature of the coal in the furnace, applies solely to this date.—J. C. H.

and $.1322 \times 80. = 10.5760$, which subtracted from the above....14355
10.6

Leaves available heat per 1 lb. combustible....................14344.4

Now we have 32.10 pounds of flue gases per pound of combustible, the specific heat of which gases is .238, and

$$32.10 \times .238 = 7.6398.$$

This is the value in pounds of water of the heat-capacity of the flue gases formed by the combustion of 1 pound of "combustible."

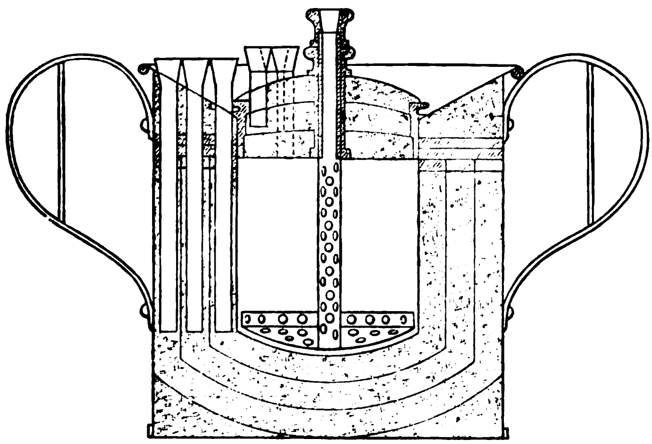
But the total number of available heat units, (B. t. u.) from 1 pound of combustible, is 14344.4; therefore the resulting *mean* temperature will be

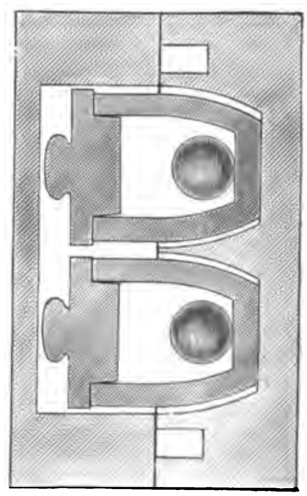
$$\frac{14344.4}{7.85278} + 60 = 1886^\circ \text{ F.}$$

60° being the initial temperature of the the air.

The temperature, however, after the fire was well ignited, the water driven off, and the hydrogen consumed, would be higher, as follows:

$$\frac{14355}{7.64} + 60 = 1939^\circ \text{ F.}$$





The air may be estimated to have contained on entering the furnace, 1 *per cent.* of moisture, and $(32.10 - .95 = 31.15) \times .01$...... 0.3115

Bringing forward the water in the coal and formed by the combustion of hydrogen, (above.)..... 0.1322

Total moisture per 1 lb. of combustible........................ 0.4437

This multiplied by .48=specific heat of steam, we have

$.4337 \times .48 =$ .21298

To which add the water value of the heat-capacity of the flue gases per 1 lb. of coal brought forward....................7.63980

Total value, in the equivalent weight (pounds) of water, of all the heat absorbents in the flue-gases per 1 lb. of combustible.7.85278

The temperature actually found in the fire, by the platinum-water pyrometer; a crucible.* containing the platinum heat-carrier being buried in the hottest part of ignited coal, was by two experiments

2281°
2259.5°

Mean..................2270.° F.

This would show that in the part of the fire where this temperature was taken, the quantity of flue gases per pound of combustible was

$$\frac{14355}{(2270-60) \times .238} = 27.29.$$

The difference between this quantity, and the quantity actually found in the flue, 32.10 is 4.81 and $\frac{4.81}{27.29} = .176 =$ say nearly 18 *per cent.*

* The fire brick shown in cut were not used: the crucible was put naked into the fire.—J. C. H.

It is not difficult to account for this. Some air always finds its way into the flue or chimney, which does not pass *through* the incandescent coal; by open doors, while firing, or cleaning fires, by leakage at fire-doors and arch-fronts, and by infiltration through brick-work—the latter quantity being much more than is commonly supposed. In this case, there was a special reason in the mode of firing, for a larger quantity of air per square foot passing through the *whole* surface of the fire, than through that portion of it where the temperature was taken. That reason was that the coal, when thrown on the grates, was not spread evenly, so as to make a fire of uniform depth, but was heaped up at a little distance from the fire-doors, so as to be noticeably thicker at that place than from there to the bridge wall. Now it was precisely in the thickest part of the fire, where the air was most obstructed, and entered through the coal in smallest quantity, that the crucible containing the platinum ball was placed, resulting necessarily in showing a temperature at that point higher than the mean temperature, and a quantity of air smaller than the mean quantity. At this point the water was all driven off, and the hydrogen all consumed. The temperature found, .2270° F., is to be compared with that of a fire in corresponding condition, but with 32.10 pounds of flue gases per pound of combustible, which was 1939° F. The difference=331° F., divided by (1939−60) =.176, the same as before.

Comparing the temperature produced by a well ignited fire, 1939° F. with the corresponding observed temperature of flue gases:

$$\frac{543-60}{1939-60}=.2570=25.70 \textit{ per cent.}$$

Again; comparing the temperature found in the heart of the fire at its thickest (and hottest) part, with the observed temperature of the escaping flue gases:

$$\frac{543-60}{2270-60}=.2185=21.85 \textit{ per cent.}$$

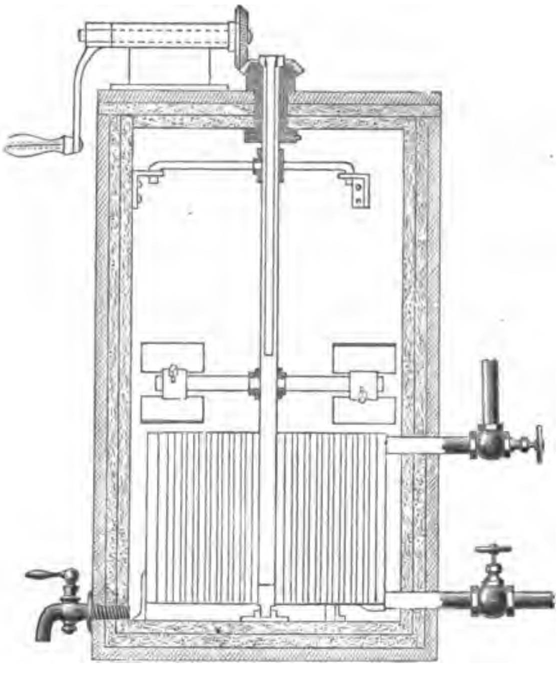
The actual loss was 25.47 *per cent.* The first, which represents mean conditions, agrees substantially with the last; the difference, 25.70−25.47=0.23, being insignificant. The second, which represents conditions far above the mean, yet agrees so well with the last as to be strongly confirmatory of it. The difference, 25.47−21.85=3.62=14 *per cent.*, is no more than must be expected from the disparity of conditions. In effect, the heat generated in the thickest part of the fire was diluted by air passing in more rapidly through the thinner parts of the fire, and leaking in about doors and arch fronts, and elsewhere, and drawn in through the occasionally opened fire door, so as to reduce the temperature of the gases by augmenting their mass; and consequently the loss was carried up from 21.85 *per cent.* which it would have been with no more air than actually passed *through* the thickest part of the fire, to 25.47 *per cent.*, as it was found to be with the quantity of gases actually present in the flue leading to the chimney.

Calorimetric Experiments.

The condition of the steam as to entrained water, was ascertained by means of Mr. Hoadley's steam calorimeter. This instrument had been very successfully used in precious work, and much care had been taken in its construction and in ascertaining its own proper heat-capacity.

Briefly described, this calorimeter consists of a cylindrical vessel of 24 oz. tinned copper encased in a jacket of heavy galvanized iron, the inner and outer vessels being concentric cylinders, leaving a space 2.25 inches thick all around and a similar space at the bottom, which is filled with two 0.75 inch layers of eider-down surrounded by 0.75 inch of hair felt. A cover of similar construction, thickness and filling, shuts in to the depth of 0.75 in., the remaining 1.5 in. extending out flush with the outside of the case.

The vessel holds rather over 200 pounds of water, and is usually charged, for an experiment, with about that quantity. An agitator, in form a two two-bladed propellor, is turned by a crank which does not interfere with the use of a thermometer placed deep down in the hollow shaft. The heat-capacity, carefully determined by three independent methods, all admitting of much accuracy, and differing but little in the results of many experiments, is equal to that of 17.2 pounds of water. The thermometer used to



measure the increase of temperature is 32 inches long, with a long bulb, and a safe-bulb a-top, graduated to degrees and tenths of a degree F., and has a range of 48° (32° to 80° F.), in about 28 inches. This gives nearly 0.6 in. to 1° and admits of accurate reading to $\frac{1}{100}$ degree. Steam was condensed in a tubular copper drum forming a surface condenser, from which the water resulting from the condensation of steam together with the entrained water was drawn off and weighed on a delicate balance. The data of one of the nine experiments with steam drawn from the Babcock and Wilcox boilers, will be found below.

Time of Readings.	Weight of calorimeter and contained water.		Weight of water in condenser.		Temperature of water.		Difference: Increase of temperature.	Pressure by steam gauge above atmosphere.
	Before admitting steam.	After admitting steam.	By difference.	By weighing after drawing off.	Before admitting steam.	After admitting steam.		Pounds per sq. in.
h. m.	lbs.	lbs.	lbs.	lbs.	deg. F.	deg. F.	deg. F.	
1	2	3	4	5	6	7	8	9
..	529.	535.25	6.25	6.305	..	..	..	101.7
12.51	..	..	..	..	44.88	..	..	..
12.52	..	..	..	..	44.92	..	..	..
12.53	..	..	..	..	44.96	..	..	..
12.53½	Steam admitted.			..	44.98	77.68	32.7	..
12.54	Steam shut off.			..	..	..	..	..
12.55	..	..	..	..	..	..	..	..
12.56	..	..	..	..	..	77.68	..	..
12.57	..	..	..	..	..	77.68	..	..

Total heat above 0° F. in steam of $101.7+14.7=116.4$ lbs. pressure absolute 1217.25

Heat in British thermal units imparted to water per 1 lb. of steam condensed:

$529.0+17.2-332.0=214.2$;

$$\frac{214.2\times 32.73}{6.305} \quad . \quad . \quad . \quad 1111.94$$

Heat in B. t. u. that 1 lb. of steam would give up in condensing, if saturated: $1217.25-77.71 = 1139.54$

Heat in B. t. u. that 1 lb. of water at t. corresponding to 116.4 lbs. per sq. in abs. would give up in cooling to 77.68° F. $=342.43-77.71$ . . . . $= 264.72$

Then,

$$\frac{1139.54-1111.94}{1139.54-264.72}=\frac{27.6}{874.82}=.0315=3.15$$

per cent.

The results of these calorimetric tests are subjoined:—

Babcock & Wilcox Company's boilers.		Return Tubular boilers.	
Date. 1882. Oct.	*Per cent.* water.	Date. 1882. Oct.	*Per cent.* water.
18	4.29	23	1.72
18	3.76	23	3.21
18	3.19	24	1.24
19	3.03	25	1.98
19	2.80	25	2.18
19	1.92		
20	2.89		
20	3.36		
20	3.15		
Mean......	*3.15	Mean......	*2.06

* Mr. Nystrom, by another apparatus, and by a method, correct in principle, but hardly admitting of the degree of accuracy attainable by the method described in the text, obtained the following results:

October 19, 1.8 *per cent.*
20, 1.4 "
21, 0.91 *per cent.*
25, 1.1 "

The mean of the two experiments with the B. & W. boilers, Oct. 19, 20, is 1.6 *per cent.*

The observation for Oct. 21, was not made while the tests were going on; but being under the same conditions, there is no good objection to taking it in. Then the mean of 0.91 and 1.1 is, 1.05 *per cent.* The ratio of these numbers to each other is the same as that of the means of the table in the test: Thus,

$$\frac{1.05}{1.6}= .656 \text{ and}$$

$$\frac{2.06}{3.15}= .654.$$

Experience with other return tubular boilers, at a lower rate of evaporation, leads me to think that the entrained water was, in both these sets of boilers, higher than is given by Mr. Nystrom's experiments, and I have preferred to insert my own determinations in the text, rather than the more favorable determinations of Mr. Nystrom. The largest result, 3.15 *per cent*, is not excessive, and when the heavy demand made upon these boilers for steam to supply the leakage of the engine valves is taken into consideration, the quantity of entrained water appears moderate.—J. C. H.

Condensation in Steam Pipes.

That portion of the steam which condenses in the steam pipes, and of the water carried over from the boilers, which settles and collects in the bottom of the pipes, is allowed to drain off through a drip pipe a quarter of an inch in diameter, and the flow is so regulated by a stop-cock as to prevent at once an escape of steam and an undue accumulation of water.

An experiment was made to determine the quantity so carried off. A bucket was filled with ice-water, weighed and set to receive the water from the drip-pipe conducted to it by a flexible tube. At the end of five minutes it was again weighed, and found to have gained 4 lbs. 3 oz. 1 drm. avoirdupois, = 4.1914 lbs. Twelve times this quantity = 50.3 lbs. would be discharged in an hour, and as the evaporation was at the rate of 6054 lbs. per hour the ratio of the drip to total evaporation $=\frac{50.3}{6054}=.0083$, is 0.83 *per cent.**

It follows that the quantity of water carried over by the steam and formed by condensation in the steam pipes, is diminished by this quantity of drip, which is equal to $\frac{83}{100}$ of one *per cent.* of the total quantity evaporated.

Surface Disturbance.

The mean steam pressure during the test of the Babcock & Wilcox boilers, October 18, 19 and 20, was 115.1 lbs. per square inch absolute.

The weight of 1 cubic foot of steam of this pressure is 0.264256 lb.

The weight of water evaporated per hour was 6054 lbs., and therefore the volume of steam generated per hour was $\frac{6054.}{.264256}=22909$ c. ft. $=6.363$ c. ft. per second. The area of water surface for the disengagement of steam in these boilers, being 80 sq. ft., the quantity of steam generated per second, per sq. ft. of water surface is $\frac{6.363}{80}=.07954$ c. ft.

This is almost 1 c. in. (0.954 c. in.) per second from every square inch and equal to 0 004 in. evaporated per second from the water, lowering its surface 0.24 in. per minute.

For the Return Tubular boilers, the mean steam pressure was, during the test, Oct. 23, 24 and 25—119.8 lbs. per sq. in. absolute, and the weight of 1 c. ft. of steam of this pressure is 0.273862 lb. The water evaporated per hour was 6535 lbs. and it follows that the volume of steam generated per hour was $\frac{6535}{.273862}$ $=23862$ c. ft. $=6.6283$ c. ft. per second. The water-surface for these three boilers being 130.2 sq. ft., the quantity of steam generated per second per sq. ft. of surface is $\frac{6.6283}{130.2}=.05091$.

This is equal to 0.6309 c. in. from each sq. in. per second, and involves the evaporation of 0.00277 in., depth of water per second, lowering the surface nearly 0.17 in per minute.

We are thus afforded a means of comparing the violence of the surface agitation in the two cases, or the upward thrust of the steam bubbles, and their tendency to carry along, in mechanical suspension, particles of *entrained water*.

The actual proportion of entrained water as found by calorimetric experiments was, for the Babcock & Wilcox boilers, 3.15 *per cent.*, and for the Return Tubular boilers, 2.06 *per cent.*, and the ratio of the latter to the former $\left(\frac{2.06}{3.15}\right)$ is as 1 to 1.53.

On the other hand, the ratio of the disturbance, that is, of the respective volumes of steam generated each second from each 1 square unit of water surface is $\left(\frac{.07954}{.05091}\right)$, as 1. to 1.567, a result substantially the same as before, the difference being less than 2 *per cent.* The two methods are therefore mutually confirmatory.

Conclusion.

The general result—a difference of about 7 *per cent.*, in favor of the Babcock & Wilcox boilers, arrived at by four independent methods of comparison, all free from objection, and together, mutually confirmatory in the highest degree, is what might have been confidently predicted upon a comparison of the respective proportions of heating surface to fire-grate area, and other data, of the two sets of boilers.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia.—At the meeting of Nov. 4th, Mr. John Haag read a paper on the number and tonnage of British ships built in 1881.

Mr. Charles Darrach read a paper on the Pollution of Water.

At the meeting of Nov. 18th, Mr. Charles W. Pusey presented a paper upon the Twin Screw Steamer "Victoria." On Tuesday, Nov. 7th, this steamer sailed from Wilmington, Del., for Greytown, Nicaragua. This vessel is a light draught twin screw steamer for service on Lake Nicaragua, and of a class that is attracting some attention from those interested in the economical transportation of freight on bays and rivers, where the draught of water is limited, and where the side wheel steamer is principally used.

The hull is of iron and is 136′ 6″ length over all, 26′ beam and 7′ deep above cross floors. The model is the same as that of several side-wheel steamers built for service on rivers and bays in South America and Mexico. She has one fore and aft bulkhead in center, and four athwartship, all made water-tight. The compartment aft is fitted for water ballast to trim the vessel. The frame is of angle iron. The machinery consists of two compound engines with cylinders 12″ and 21″ diam. x 18″ stroke, fitted with jet condensers. The engines are independent, each driving a propeller wheel 6′ diameter. She has two steel boilers of the locomotive type, fitted for burning wood and constructed for a working pressure of 100 lbs. per sq. inch. The finished draught of water with 5 tons of coal in bunker, was 4′ 6″ aft, and 3′ 6″ forward. On trial trip with a draught of 5′ 4″ aft and 2′ 10″ forward, she made a speed of 10 knots per hour, with 119 revolutions per minute, 94 lbs. 26″ vacuum; total indicated horse power 246. During the trial the ballast tank was filled with water.

When she sailed for Greytown she had a cargo of 105 tons of coal, also merchandise and stoves amounting to about 20 tons more, the draught of water being 6′ 3″ aft and 5′ forward. Under these conditions going down the bay, she made 9½ knots per hour, with 80 lbs. steam pressure and 108 revolutions per minute.

Prof. L. M. Haupt presented a profusely illustrated description, which he has been compiling for some time, of the progress of early American Wooden Bridge Building. Reference was made to the well-known structures of Schaffhausen, 1758, and Wettingen, both destroyed by the French in 1799, and to the remarkable bridges by Wernwag, between 1804 and 1812. A summary of the typical forms as

represented in the Burr, Town, Haupt, the Longs, Howe & Pratt trusses, was then given with illustrations and reference to the works of Mr. Chevallier, Lavoinne, Pontzen and others.

Attention was called with regret to the fact that the best illustration and data with reference to the engineering works of America were generally only obtainable from foreign sources.

The paper gave rise to a general discussion, in which Messrs. Cleemann and Graff cited instances of early suspension bridges (Mr. Graff presenting the Club with a photograph of a painting, showing the old suspension bridge at the Falls of Schuylkill, Philadelphia), and reference was made to the early floating draw constructed by the late Henry R. Campbell, at Rouse's Point; also to a wooden truss said to have been built about 1809, over the Terrebonne River, near Montreal, Canada, of *six hundred* feet span, but of which no very authentic data could be obtained.

A communication from Mr. M. Coryell, was received through Mr. Wm. A. Ingham, describing the early structure built by Wernwag, at New Hope, over the Delaware, with illustrations of several of the Wernwag bridges. The description was in the handwriting of Mr. Ingham's father, then president of the bridge company.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—At a Regular meeting, held on Dec. 6, Vice-President Paine in the chair, the following were elected members: Timothy Cookson Bradley, Assistant U. S. Engineer, St. Joseph, Mo.; Henry J. Gielow, Assistant U. S. Engineer, Town Creek, Ala.; Edward S. Safford, Division Engineer, New York, West Shore & Buffalo, and New York, Ontario & Western Railroads, Newburgh, N. Y.; James Dix Schuyler, Chief Engineer and General Superintendent, Sinaloa & Durango Railroad, Culiacen, Sinaloa, Mexico; Thomas Wellington Spencer, Civil Engineer, Utica, N. Y.; Russell Thayer, Chief Engineer and Superintendent Fairmount Park, Philadelphia, Pa ; Edmund Brownell Weston, Engineer in charge Water Department, Providence, R. I.

Mr. J. J. R. Croes, M. Am. Soc. C. E., described the progress that had been made in the procuring of supplies of water for cities and towns from subterranean sources, or "ground water," describing and illustrating the galleries, basins and wells which have been constructed in the United States within the last 18 years. The theory on which the first works of the kind were constructed was that a supply could be procured by filtration through the gravel of a bank of a river or pond. It was found, however, that more water came from the land side than the river, and that it was of a different character from the river water. It is now established that such supplies are furnished almost exclusively from the great underground streams or reservoirs, which are found in all the valleys of streams which are filled to a considerable depth with gravel. The failures and successes of many works were described, and the yield of filtering galleries discussed. The relative advantages of open and closed galleries were discussed, and it was shown that the experience of most places was decidedly against the advisability of open galleries or canals, on account of the growth of vegetation in them, although the water came from the gravel perfectly pure and cold, like spring water. An interesting discussion followed, in which Gen. George S. Greene, Mr. Joseph P. Davis, Mr. F. H. Leating, City Engineer of Halifax, and others participated.

ENGINEERING NOTES.

A DANUBE-ELBE CANAL.—The idea of joining the Elbe to the Danube by a waterway is a very old one, the construction of such a canal having been proposed as early as the reigns of Charles IV., Ferdinand II., and Joseph I. of Austria. In our own time the investigations connected with the subject of a canal from Linz, on the Danube, to Budweis, in Bohemia, have assumed practical shape in the form of a railway connecting the two towns. In a pamphlet published by the Vienna Danube Society, the idea has been revived. It is therein proposed to construct a canal which leaves the Danube at Kornneuburg, ascends in three main series of sluices, which together would overcome a rise of 1239 feet by means of 129 locks, to a plateau 45 miles long, whence it descends 538 feet by 56 locks to Budweis, where it joins the Moldau, a tributary of the Elbe. The whole length of the artificial canal would be 138 miles. The canalization of the Moldau from Budweis to Melnik, for a distance of 151 miles, would be effected by 62 locks, which together overcome a fall of 736 feet. The average distance of the locks from each other in the river would be 2¼ miles; in the artificial canal, exclusive of the horizontal portion, ¾ mile. The water supply of the canal on the plateau is to be provided for by the construction of reservoirs on the Upper Moldau and the Leinsitz, with corresponding feeders. The depth of water in the canal is to be 6½ feet; the width of the canal bed, 48 feet; the sluices have a width of 25 feet and a length of 208 feet. The sluices of the canalized river are about double as wide and long, and are consequently capacious enough to hold four vessels 192 feet long and 22½ feet beam at the same time. The water is to be dammed back by movable Poirée weirs combined with fixed overflowing weirs parallel to the direction of the current. The total cost of the canal and the canalization of the Moldau is estimated at £5,800,000, or about £20,000 per mile of waterway.

ANTWERP WATERWORKS.—The water for the supply of the town of Antwerp is drawn from the river Nèthe, at the Waelhem bridge, 10½ miles south of the town. The water, after having been purified, is sent in pipes, under a pressure of five atmospheres, from Waelhem to Antwerp.

The process of purification consists: 1st, in allowing the water as it comes from the river to settle the solid matter, and then decanting the clean water; 2d. Filtration through spongy iron and sand; 3d. A second filtration through

sand; 4th. The discharge of the purified water into the conduit.

The deposit from the water takes place in two basins alternately. These are filled three hours after each high-tide, when the water is in the best condition, by a 4-feet pipe from the river, passing through the embankment or dyke. Each basin holds 6,000 cubic meters (1, 320,000 gallons), and is filled in three-quarters of an hour. The water remains in each basin for twelve hours, and is then run off from the surface into pits, whence it is lifted by means of screw-pumps to the level required for delivering it into the filters.

There are two elevating screws, 40 feet long, and 3 feet in diameter, placed at an angle of 30°, driven each by a 12-H.P-engine. Each screw delivers 9 cubic feet of water per revolution, and they have eighty per cent. of efficiency. There are three filter-beds of spongy iron, placed side by side. The spongy-iron appears to be a bluish-gray scoria, of which the pores are much subdivided; it is in pieces of the size of hazelnuts, prepared from a rich iron ore. It holds in its pores a large quantity of air minutely divided, in presence of the oxygen of which the organic matters in the water are burned or oxidized, when the water passes away fresh. Each filter-bed consists of three layers. At the bottom, the first layer, 3 feet deep, is a mixture of three parts of spongy iron and one part of pea gravel; then a layer of fine gravel about 4 inches deep; and, at the top, a layer of sand from Dordrecht, 2 feet thick. The foundation consists of beds of brick, open laid, through which the filtered water passes into a collecting channel, by which it runs off to the sand filters.

The sand filters are three in number, one to each filter of spongy iron, formed of three layers of sand and gravel. They detain pieces of iron oxide that escape from the iron filter, with the last impurities in suspension. The water, now fresh and pure, flows off into the reservoirs, which are circular and covered, and are constructed of cast-iron plates.

The pumping engines for forcing the water through the mains were constructed by Messrs. Eastons and Anderson. They are in two pairs, compound, of 100 H.P. each. The cylinders are 18 inches and 29½ inches in diameter, with strokes of 44 inches and 66 inches respectively, making, at maximum speed, twenty-two turns per minute. The pump delivers 48 gallons per revolution. of the engine.—*From Abstracts of Inst. Civil Engineers.*

RAILWAY NOTES.

New Projects for a Simplon Railway.—Even during the progress of the works connected with the now completed St. Gothard Railway, people in France began to think of means for meeting the competition that would be caused to French through traffic. Amongst the various proposals, the establishment of new railway communication with Italy by way either of the Simplon, Mont Blanc, or Little St. Bernhard, probably found most favor. The engineer's report concludes: "Two lines have principally engaged the attention of the committee, that over the Simplon and that over Mont Blanc. The one reaches the Italian frontier only after using Swiss railways for a length of 127 miles; the other leads direct from France to Italy. For the first line the tunnelling of the Alps would take place outside of France; the tunnel of the second line would have its northern mouth on French territory. The route over Mont Blanc would preserve to France her own traffic and to the port of Marseilles the through traffic of Switzerland; the Simplon line would, on the contrary, take our travelers and our goods over Swiss railways, and at the same time withdraw from Marseilles the through traffic above mentioned, handing it over to the port of Genoa and the Italian railways. Under these conditions, the majority of the committee have pronounced in favor of the Mont Blanc route, and recommend the adoption of the following resolution: The chamber requests the government to cause the project for a new international railway communication through the Alps, by way of Mont Blanc, to be submitted to an early and thorough examination."

We are unaware how far the investigations urged upon the French government with regard to the Mont Blanc line have been carried; but the surveys for the various routes of the Simplon Railway, which do not present the same engineering difficulties, have been actively pushed forward, as we learn from an article in the *Eisenbahn*. Both lines extend like feelers into the high Alps of the Swiss-Italian frontier. One coming from Geneva and Lausanne, ascends the valley of the Upper Rhone, and ends at Brieg; the other branches off at Novara, and extends as far as Gozzano and Arona respectively, according as they join on to the one or the other of the branches. The connecting line between the two railway systems requires a railway of a length of rather more than 60 miles, of which, however, about 12½ miles, or over 8 miles more than the St. Gothard tunnel, would form the great Simplon tunnel.

The direction of the projected Simplon tunnel is from northwest to southeast. The northern opening is about 1½ mile up the Rhone from the terminus at Brieg, at an altitude of 2205 feet, the southern tunnel end is placed below the village of Isella, at an elevation of 2008 feet, and 26 feet above the high-water level of the Diveria. Compared with the highest elevation of the St. Gothard tunnel (3693 feet), the Mont Cenis tunnel (4048 feet), and the Arlberg tunnel (4102 feet), that of the Simplon tunnel would be comparatively low, which would facilitate the construction of the approach railways, but, on the other hand, greatly increase the difficulties of constructing the tunnel on account of its great depth under the superincumbent mountain mass. The experiences gained in building the Mont Cenis tunnel, and still more the St. Gothard tunnel, show that one of the greatest difficulties in Alpine tunnel construction of great length and depth is the high temperature met with, which requires a perfect system of ventilation in order to make work at all possible. In the St. Gothard tunnel the temperature is about 31 deg. C.; but it is well known that the means for ventilating it are defective, and

open to improvement. For a length of from 1300 to 1600 feet, the overlying mountain mass of the projected Simplon tunnel has a thickness of nearly 7000 (6944) feet, and, compared with what we find in the case of the St. Gothard, the maximum temperature in the interior of the Simplon tunnel would be between 32 deg. and 38 deg.

Professor Heim, of Zurich, has also made calculations with regard to the temperature in the interior of the projected Mont Blanc tunnel. The latter would lead from Chamounix to Pré-Saint Didier; its elevation would be 3434 feet above the sea, and its length about 12 miles. The result of his investigations was that, with an overlying strata here present of 11,200 feet, the temperature would be 40 deg. C. for a length of the tunnel of 3½ miles, and for a length of 2.2 miles even 55 deg. C., without the possibility of lowering the temperature by a change of the level from the straight, as in the case of the Simplon tunnel. But the formation of the soil permits of the portion of the tunnel under Saint-Didier to be ventilated for a length of about 8 miles by shafts of a depth of not more than 640 feet.

As regards the cost of the rival railways, the only available estimates are those furnished by the committee of the French chamber. According to them, the construction of the Simplon Railway (Brieg, Isella, Pré di Mulera, Arona) would involve an expenditure of about 145,000,000 francs (£5,800,000); that of the Mont Blanc Railway (Annemasse, Chamounix, Pré-Saint Didier, Aosta-Ivrea together about 120 miles) would cost 179,000,000 francs (£7,160,000). The expense for the tunnels alone is estimated at 83,700,000 francs (£3,348,000) and 76,000,000 francs (£3,040,000) respectively.

The number of railways to be worked by electricity is now considerable. Those which are working, authorized, or in course of construction, show a total length of about 100 miles. The lines actually at work are those of Lichterfelde, 1.56 miles, and that from the Spandauer Bock to Charlottenberg, near Berlin; the Port Rush to Bush Mills, in the North of Ireland, about 6.5 miles, and also in Holland, one from Zandvoort to Kostverloren about 1.3 miles long. Among lines authorized or in construction, the following are noted: In Austria, the Mœdling line, near Vienna, 1.5 miles, to be constructed by the Southern Railway Company there. In Germany, the line from Wiesbaden to Nürnberg, 1.3 miles, and that from the Royal mines of Saxony to Zankerode. The line under the Thames connecting Charing-cross and Waterloo stations will be about three-fourths of a mile; also a line in South Wales thirty-seven miles, for which the power will be derived from fall of water. In Italy, Turin and Milan will soon begin the construction of electric tramways. In America the Edison Company have arranged for the working of 80 kilometers on one of the great lines from New York. Another small line, 1.1 miles, is to be made at St. Louis, in Missouri, by Mr. Heisler.

Canadian Railway Progress.—The Canadian Pacific main line is constructed for a distance of 440 miles west of Winnipeg, of which about 300 miles have been built since May by Messrs. Langdon, Shepard & Co. The whole distance was built within fifteen months. The southwestern branch of the Canadian Pacific is now built two miles south of Morris, and the Selkirk branch is also under construction. Mr. Van Horne, general manager of the Canadian Pacific, states that the whole of the line north of Lake Superior will be under contract by January 1, 1883 and that next season work on the western division will be completed to the Rockies. Mr. Senecal is said to contemplate extending the North Shore Railway to Tadoussac, and establishing a new line of steamers from that point, thus making it the winter port of the western provinces of the Dominion. An era in the progress of Ottawa has been marked by the opening of the Canada Atlantic Railway extending from that city to Coteau Landing. The new line considerably shortens the route from Ottawa to Montreal.

New locomotives.—A locomotive on the compound principal has recently been perfected by Mr. E. W. Webb, Locomotive Superintendent of the London and Northwestern Railway. There are two sets of driving wheels, which are not connected, but each set is driven by independent engines. The steam is first received, under high pressure, in two cylinders, placed on each side of the boiler, and about half way back. The pistons of these work the rear drivers by direct motion. The steam from them is then exhausted into pipes, which are conducted in front of the boiler flues, to keep the steam hot, and then into one large cylinder located directly under the front of the boiler. This cylinder carries, by a shaft under the boiler, the forward drivers. A saving of over 25 per cent. upon ordinary requirements is said to be made in coal. Another fuel-saving locomotive is being tested on the St. Louis and San Francisco Railroad, which is known as the shut-down engine. In a trip from Springfield, Missouri, to the mouth of the tunnel through the Boston Mountains at Winslow, in Arkansas, and from there to Wichita, Kansas, returning to Springfield, a distance of 750 miles, it attained a speed of 50 miles an hour. To this engine is attached a smoke consumer and smoke arrester, and during the trip above referred to, no smoke was observable while the engine was working, though some could be seen to escape when the steam was shut off. It is stated that the consumption of fuel was at least one-third less. Still another novelty in America is Strong's express locomotive, which, says the *North American*, bids fair to spring into general use on every road where its merits are properly appreciated. The designer of the engine spent a long time in England and on the Continent, taking note of all the good points in locomotives used abroad, and, upon his return to the United States, constructed an engine embodying the results of his studies. In the boiler of Mr. Strong's locomotive the corner bars and side stays are done away with, the fire-box is designed to ensure complete combustion of fuel by burning the gases and sparks, the driver coupling is so arranged that the side rod is unnecessary, and there is also a better

distribution of wearing surface on brasses or crank pins than is the case in ordinarily constructed locomotives when the power for both wheels is transmitted through the forward pin. The valve motion is also improved, and the feedwater heated by a portion of the exhaust. In short, the locomotive is so constructed as to be economical, and as fast as is desirable, and always to have a reserve of power for a heavy train, while at the same time it is simple and not liable to derangement, and safe for those who run it as well as those who ride behind it, and one that shall burn its coal in so perfect a manner as to do away with cinders and smoke.

TUNNEL AND ELEVATED RAILWAY AT HAMBURG.—The Senate of Hamburg have had under their consideration a scheme, proposed by Herr Westendarp, an engineer, for constructing a tunnel under the Elbe, and an elevated railway in Hamburg. The construction of a bridge, instead of the tunnel s out of the question, owing to the width and crowded state of the harbor; and the tunnel must be laid deep enough not to interfere with the passage of deep draught vessels. The author of the project has designed the tunnel of such dimensions as would provide both for vehicular and foot passenger traffic, and a double-track railway for goods and passengers. This he proposes to effect by building the tunnel of two stories—the road for vehicles and pedestrians to be in the upper, the line of railway in the lower story. The crown of the tunnel for a length of 656 feet (the width of the navigable channel of the Elbe), is to be 20 feet below the new, or about 30 feet below the old low-water mark. Separate openings are provided for railway and roadway, the ascent to be very gradual, 1 in 35. The tunnel is to be an iron cylinder lined with brickwork, the upper story resting upon columns placed between the two lines of rails. The latter would be in continuation of the track of the elevated railway in the city, which is to be of a decorative ironwork, and similar to that of the New York Elevated Railroad. Three stations are provided—at the Exchange, the Fischmarkt, and the St. Annens-Platz, where elegant pavilions are to be erected in the level of the line for taking up and putting down passengers. The tunnel is to be lighted by electricity, and ventilated and drained by powerful machinery; provision is also made for taking through the tunnel gas and water pipes, telegraph and telephone wires, a pneumatic post, &c. The estimated cost of tunnel and railway is 26,000,000 marks (£1,300,000), and they are to be completed in 5¼ years. As it is of the greatest importance to the city of Hamburg to connect the territory of the projected free port on the left bank of the Elbe direct with the city, that expense will not stand in the way of carrying out the stupendous undertaking.

ORDNANCE AND NAVAL.

VICE-ADMIRAL VON HENK—for some years at the head of the constructive branch of the German Navy—has published an article in a military periodical on the subject of iron-clad ships. He considers that experience and observation up to date prove: First, that unarmored ships cannot maintain a fight of any duration against the heavy guns of ships and forts. Secondly, that iron-plating is still an effectual defense against the heaviest guns, and consequently indispensable for battle-ships. Thirdly, rams and torpedoes are, indeed, formidable weapons in sea fighting, but cannot supersede artillery as the chief means of defense.

The above conclusions are arrived at from a review of the history of naval warfare since the introduction of shell guns. Admiral Von Henk considers it desirable to draw as many lessons as possible from every event in this field, and thus increase our experience; because military history has as yet furnished but few materials for forming a final judgment as to the value of armor-plating for ships, the purposes aimed at in its use, and whether these purposes have really been attained. He commences at the year 1850, soon after which shell guns began to be employed as naval weapons. Their use soon diminished the value of the old line-of-battle ships carrying 80 and 100 guns each, while the introduction of steam made the construction of ships more complicated and increased their vulnerability. This was shown conclusively in the Crimean War, when many of the best Turkish line-of-battle ships were set on fire and burnt by the Russian shells. Admiral Von Henk goes on to the American war, in which the Merrimac attacked four wooden frigates and destroyed two of them, but the next day was obliged to retreat, severely damaged, after an engagement with the ironclad ship Monitor. At the battle of Lissa, when seven Austrian ironclads were opposed to eleven Italian, the armor of the ships was about equal in strength on both sides, but the Italians were stronger in artillery. The relative strength of armor and guns was then about what it is now. The loss on board each of the armor-clad ships was very small, being only thirty-three killed on the Austrian side out of 7,000, but in the unarmored ship Kaiser the casualties were twenty two killed and 124 wounded, or two-thirds of the whole crew. The Admiral says that the battle of Lissa was an eloquent advocate for the incipient system of armor-plating ships of war. In the engagement between the two British unarmored ships and the Huascar in 1877, the latter had to be allowed to escape on account of the danger of approaching her. As to the fish torpedo, he considers that we have as yet no experience of that new, malignant, and deceitful weapon, for the published reports of the Russo-Turkish war are so untrustworthy that one can found no opinion upon them.

Coming to the bombardment of Alexandria, Admiral Von Henk says that several of the ships were hit and damaged. The flagship Alexandra suffered most. She had fourteen shots in her hull. One shell went through her deck and burst in the Admiral's cabin, another in the captain's, while others went through the funnel and shattered the long boat. The Sultan and Superb were also severely punished by the enemy's shot. The Superb got twenty-three shots from Fort Ada, though most of the

shots from this fort fell either short of, or beyond the ships. The reasón why the Condor and other gunboats suffered so little is partly on account of their small size, but chiefly because of the unskilfulness of the Arab gunners. The position of the ships was unfavorable, as the sun interfered with the aim of the gunners, and the wind was also against them. After the first volley a cloud of smoke arose and made it difficult to see from the ships whether their shots took effect. Only from the top of the masts could it be judged approximately whether and how much, each shot fell short of, or went beyond, its mark. The same difficulty arose after each volley. The 1,700 lb. shells of the Inflexible are said to have had a most demoralizing effect upon the Egyptian soldiers, but special notice is due to the great resistance offered by the earthworks to the fire from the ships. The loss of the Egyptians is unknown. Although so many shots went through the ships, the only penetration was in the unarmored non-fighting portions. Not one pierced the armor of any ship. The total English loss was five dead and twenty eight wounded. Neither attack nor defence were, however, normal, for it admits of no doubt that if the English ironclads had been opposed to European forts, with modern guns, skilled gunners, and a number of swift torpedo boats and of sea mines, they would not only have had a hard time of it, but would have suffered much damage and loss of life.

Admiral Von Henk concludes that the lessons to be drawn from the bombardment of Alexandria are the following: 1. The great value of armor-plating for protecting the men, the engines, the gun platforms, and the most vital inner parts of the ships. 2. The necessity of using the heaviest guns first, at the beginning of the attack, in order to diminish the difficulties of aiming well, manœuvering, and guarding against torpedo boats, caused by the smoke.

These opinions agree with those expressed by us, as already referred to, and will doubtless form subjects of discussion for some time to come among naval men. The smoke difficulty is a serious one, and has been but little considered. It is certain that the dense smoke produced as soon as firing commences will embarrass very much the evolutions of the various ships of a fleet, and often make it very difficult for them to manœuvre and attack with precision. This will give the opportunity for fast torpedo boats and rams to rush in unperceived and give the *coup de grace* to large and heavy ironclads. The best position for a look-out in these circumstances has yet to be discovered: and also the best mode of making it somewhat secure. The tops furnished the most obvious and available means for the purpose at Alexandria; but their frailness and insecurity disqualify them from being anything more than a perilous and temporary refuge. The matter is deserving of the careful attention of naval men, and the conduct of future naval actions will depend largely upon it.

The high value of artillery as the primary offensive weapon in ships admits of no question, particularly after such an experience as we have had at Alexandria, where the gun was the only arm which could be employed. The ram and torpedo are effective and deadly weapons, but must be subordinate to the gun, and in this case the former would have been useless, however well our ships may have been supplied with them. It is suggestive to note that Admiral Von Henk calls the fish torpedo a deceitful weapon. The Germans, have had great experience in its manipulation, and have experimented with it on a large scale; but it would appear that they have not any great confidence in its practical value.

The remarks upon the great value of armor-plating in protecting the lives of the crew, and the machinery and fighting appliances of a ship, form an interesting commentary upon Sir W. Armstrong's presidential address to the Institution of Civil Engineers in January last, when he advocated that ships of war should be constructed without armor-plating, and made "freely penetrable." He argued that as it is possible to make a gun that will pierce a target composed of the thickest armor a ship can carry, armour-plating is therefore devoid of protective value; and that as unarmored ships can be so divided into compartments as to be practically unsinkable, armor-plating is not necessary to preserve them from destruction. These arguments, as we showed in our issue of 20th of January last, only show how narrow and erroneous the views of specialists often are, even though they may have risen to the eminence of Sir W. Armstrong. We then pointed out, and the recent action has proved it, that armor has a very high defensive value, even though it may not make a ship absolutely invulnerable against any imaginable attack—that it is a great protection to life, and against the disablement of the guns. It is of no use to make a ship unsinkable if she can be penetrated through and through by the enemy's shells, and the crew and fighting appliances destroyed. The bombardment of Alexandria will lead to a more correct appreciation of the value of armor than has formerly been general; as it is certain that but for what Sir W. Armstrong would call the "obsolete" armor-plating of our ships, there would have been very many more casualties and much damage done.

The German admiral takes an impartial, correct and practical view of the question; and is right in stating—what is after all an unfortunate necessity—that armor-plating is still an effectual defense against the heaviest guns, and consequently indispensable for battle ships.

IRON AND STEEL NOTES.

PROGRESS OF THE BASIC PROCESS.—At a meeting of the South Staffordshire Mill and Forge Manager's Association, held at Dudley on November 25, Mr. R. Edwards in the chair, a paper containing the latest information on the basic steel process, by Messrs. Thomas and Gilchrist, was read by Mr. P. C. Gilchrist. The authors remarked that the common Staffordshire pig-iron with which they experimented last June at Wednesbury had, before being melted, the following composition:—Strong forge pig—manganese, 1.12 per cent.; silicon,

1.17; sulphur, 0.08; phosphorus, 2.07; grey forge pig—manganese, 1.18; silicon, 1.67; sulphur, 0.5; phosphorus, 2.72. The strong and grey forge were mixed in equal proportions and melted in the air furnace, and the resulting metal contained—manganese, 0.75; silicon, 1.28; sulphur, 0.10; phosphorus, 2.94. The authors presented the analysis which they had specially obtained from Mr. Windsor Richards, and which had been made by Mr. E. W. Cook, the chemist to Bolckow, Vaughan & Co., of the pigs which that company used up in the basic process on the Thursday previous to the meeting November 23, and analyses of the rail made from it, and of the basic brick used for the lining of the converters on that day. The analyses of the brick was:—Lime, 49.91 per cent.; magnesia, 30.72; alumini, 4.50; oxide of iron, 3.46; and silica, 11.41. The analysis of the pig used was:—Iron, 92.85; combined carbon, 1.10; graphite, 2.25; manganese, 0.60; silicon, 1.80; sulphur, 0 15; phosphorus, 1.75. The rail analysis was:—Iron, 98.25; combined carbon, 0.46; manganese, 1.18; silicon trace, sulphur, 0.05; phosphorus, 0.060. The casket of basic steel which the authors exhibited as having been presented to them by the Central Director of the Kladno Steel Works, Austria, was made from pig containing carbon, 3.08; silicon, 1.06; phosphorus, 1.86; manganese, .48; and sulphur, 0.26. The ingot iron, of which the casket was wrought, contained carbon, 0.18; silicon, traces; phosphorus, 0.05; manganese, 0 34. The authors further stated that Messrs. Bolckow, Vaughan & Co. were now making 9,600 tons of basic steel per month, and that in January next they expected to make 16,000 tons. At present they had four 12-ton converters. But the Continental steel masters were still ahead of England in their appreciation of the process. The authors had obtained returns from the Continental Works, showing their individual output under this process as recently as during the last month (October). These showed a total production of 87,639 tons of steel. This was produced at one work in France, one in Belgium, eight in Germany, three in Austria, and one in Russia. The largest individual output during October at any one of these works was 7,000 tons, being at the establishment of the Dortmund Union, who were employing two converters of 9½ tons each. The next largest output was at the Hoerde Works, Germany, which possesses three 10-ton converters. Their output was 4,100 tons. The process was likewise extending more rapidly on the Continent than in England. In Europe 25 converters were now being built, with a minimum capacity of 36,000 tons per month; while in England nine converters were being erected, which would probably produce at least a further 16,000 tons per month.—*Iron.*

Expansion of Steel in Hardening.—It is well known to workers of steel that by the process of hardening it is sometimes expanded, so that in a job where two pieces are fitted accurately in their unhardened state they become so much enlarged that grinding is necessary to restore them to their former relative size. No rule has yet been found by which the amount of this expansion can be actually determined before hardening. It may depend upon the amount of carbon in the steel, which varies in different specimens; on the degree of heat employed in the process; and also largely on the size and shape of the pieces treated. This expansion, may, however, be reduced to the minimum by repeated annealings while the work is in progress at the lathe, vise, or planer. It is also true, that in some instances steel, having been hardened, will contract slightly when drawn to temper, the amount of contraction depending on the lowness of drawing.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

Abstracts of Papers in Foreign Transactions and Periodicals for the Institution of Civil Engineers.

Proceedings of the Society of Arts, Massachusetts Institute of Technology, for 1879–80; 1881–82.

Journal of the Association of Engineering Societies.

Report of Third Meeting of Michigan Association of Surveyors and Civil Engineers.

Annual Report of Chief Engineer of the Water Department of the City of Philadelphia.

Annual Report of the Chief of Engineers of the United States Army for 1882.

Engineers' Book of Tables for Railway Construction. Compiled for Engineering Department of New York, West Shore and Buffalo Railway, and the New York, Ontario and Western Railway.

Die Elektrische Belenchtung. Von A. Merling. Braunschweig: Vieweg & Sohn.

This is a 12mo book of 500 pages, abundantly illustrated and describing the leading types of Magneto and Dynamo machines, and the various systems of arc and incandescent lighting.

There is little if anything new in the volume, but to readers of the German language it offers a large amount of information in compact form.

Electro Metallurgy. By Alexander Watt. New York: D Van Nostrand.

To state that the present is the sixth edition is to say that the book has proved a valuable aid to those engaged in galvanoplastic operations.

Its chief value lies in the fact that all directions for working are remarkably specific. There is no better guide for amateurs.

The present edition presents considerable new matter, chiefly relating to the deposition of nickel.

Bessemer Steel. Ores and methods. Thomas W. Fitch. St. Louis: Morrison Renshaw.

The author prepared the papers here published at the request of the Miners' and Manufacturers' Association. They are statistical in character and in brief, compact form, suitable for newspaper articles in accord with the original intention of the author.

The Ores of the United States, Great Britain, Germany, France, Belgium and Sweden form the subject of the first chapter. Chapter 2d describes the American Bessemer processes, and chapters 3d and 4th are devoted to the methods in foreign countries.

The Basic Bessemer process forms the subject of of the 5th chapter, and the Harrison Steel Company of the 6th and last chapter.

No illustrations are given, and probably none are needed by the technical readers who alone can read it with profit.

The Theory of Equations. By William Snow Burnside, M. A., and Arthur William Panton, M. A. London: Longmans, Green & Co.

This book will be welcomed by students of Algebra who desire to supplement their college course of Mathematics. It is unlike modern treatises on the newer branches of research, inasmuch as it demands no familiarity with newly coined terms, and because the methods are familiar ones of old-fashioned Algebra.

The typography is exceedingly good.

The Railroad Spiral. By Wm. H. Searles, C. E. New York: John Wiley & Sons.

This neat little book is for the use of Railway Engineers only. It affords by its formulas and tables a ready way of easing circular curves at their tangent points.

The method involves the use of the transit and chain as in the ordinary way of location, and is a scientific substitute for "the rule of thumb" methods largely employed at present.

The Builder's Guide and Estimator's Price-Book. By Fred T. Hodgson. New York: Industrial Publication Co.

Estimates for all materials used in building are presented in tabular form, together with simple rules for calculating the dimensions of such parts as are subjected to strains, make up the most of this convenient little hand-book.

Some tables of weights of materials and for timber measure are properly added for convenience of the artisan.

Naval Hygiene. By John D. Macdonald, F. R. S. London: Smith, Elder & Co.

That ships are capable of hygienic improvement any one must be convinced who has ever been at sea. That the problems to be solved involve conditions not met with in dwellings is also apparent upon slight reflection.

The writer has certainly presented the subject in a way that indicates a laborious study of it, and seems to have treated it exhaustively. The topics are in order as follows: Conservative Hygiene treating of Structure of Ships; Ventilation of Ships; Water Supply; Cleanliness; Diet; Exercise and Clothing.

Trophylactic Hygiene and Remedial or Corrective Hygiene The last two sections are more especially for ship physicians

The diagrams are numerous and instructive.

House Drainage and Sanitary Plumbing. By Wm. Paul Gerhard. Science Series No. 63. New York: D. Van Nostrand. Price, 50 cents.

This is a carefully written essay designed for the household quite as much as for Sanitary Engineers.

The defects of the ordinary method and the most modern improvements are presented with satisfactory fullness.

Dwellers in houses everywhere can gain profitable knowledge from this little treatise.

The Theory of the Gas Engine. By Dugald Clerk. Science Series No. 62. New York: D. Van Nostrand. Price, 50 cents.

The interest in the Gas Engine in no way diminishes, as the prospect of cheap gas fuel seems more promising.

The economy of space as well as of fuel has long since been demonstrated, and in this little essay the theory upon which its future success is predicated is plainly set forth.

MISCELLANEOUS.

Tripolith.—Tripolith is the name given by its inventors to a new binding material for builders, a substitute for lime, cement and plaster under certain circumstances, and which is composed of sulphate of lime, coke, and oxide of iron in some form or other. That the material requires considerably less water to form a workable mortar than ordinary lime is no doubt an advantage, while the time for setting can be admirably regulated by adding more or less ordinary slaked lime. Thus while tripolith mixed with sand only, sets in 10 to 15 minutes, an addition of slaked lime may easily increase the time required for setting to 60 minutes. The specific gravity of tripolith is lower than that of plaster, the former is 1.678, the latter 1.696. Turning now to the tests we give in our Table the mean results in each case of five complete experiments. The extraordinary increase of tensile strength after a long exposure to the atmospheric air is remarkable; it amounts to 100 per cent from seven to ninety days in mortar B, and to 189 per cent in mortar C for the same time. Compared with the tensile strength of lime and cement, the results obtained with tripolith are highly satisfactory. The compression tests point out for tripolith a position between lime mortar and cement mortar, but since after being fairly set it acquires about the same crushing strength as ordinary bricks, no more would be needed for general use. In sifting, tripolith mortar loses in weight, and when placed in water does not absorb the latter so rapidly as ordinary mortar does. Its adhesion to brick, stone, and other materials is very considerable, and the tripolith mortar does not either reduce or increase noticeably in volume when setting. For facing and plastering, this material is excellently suited; it is easily handled and smoothed while soft, adheres well to brick or

stone surfaces, and attains far greater hardness than plaster-of-paris, and oil or other colors adhere to it well.—*Engineering.*

THE NEW EXPLOSIVE.—At the bi-monthly meeting of the North Staffordshire Mining Institute, Mr. Storey, of Kidsgrove, read a paper on Smith and Morris' process of getting coal by caustic lime. He had seen the new system in operation at Shipley, and he minutely described it. The new agent consisted of lime in cartridges in a special caustic state, and the experiments were made in the hard coal, where there was a good roof, and the seam was almost flat, the work being done on the long wall system. The circumstances under which lime was used at Shipley differed so much from those of North Staffordshire mining that he feared that it could not be successfully applied to many of the mines of the latter district, where the roofs were not so good as at Shipley. Neither had they the chance of undercutting to be found where there were hundreds of yards of long-walled face. There were other disadvantages in North Staffordshire, as compared with Shipley, but any method which would enable them to dispense with blasting would be heartily welcomed, and he should like to see the lime process fairly tried in the district. In the discussion which followed, a general wish was expressed that the new explosive should be fairly tested. A discussion also took place relative to the properties of fire-damp and coal dust, and several recent instances of coal dust firing were mentioned. Mr. Lawton said they were without the means of detecting such a small quantity of fire-damp as Professor Abel said was dangerous when mixed with coal dust. He urged that further experiments should be made to obtain the requisite knowledge with which to guard against the mischievous effects of fire-damp. Mr. Young said he used salt as a means of laying the dust in the most dusty seam in the district, and it answered well. Mr. Macdonald said, if salt was used for laying the dust near where the shots were fired, if any explosion occurred, its effects would be localized. Several other members joined in the debate.

EGYPTIAN AND CHINESE LOCKS.—The earliest lock of which the construction is known is the Egyptian, which was used 4,000 years ago. In this lock three pins drop into three holes in the bolt when it is pushed in, and so hold it fast: and they are raised again by putting in the key through the large hole in the bolt and raising it a little, so that the pins of the key push the locking pins up out of the way of the bolt. The security of this lock is very small, as it is easy to find the places of the pins by pushing in a bit of wood covered with clay or tallow; on which the holes will mark themselves; and the depth can easily be got by trial. Mr. Chubb, the English lock-maker, possessed a wooden Chinese lock, which is very superior to the Egyptian, and in fact, is founded on exactly the same principle as the Brahma lock, which long enjoyed the reputation of being the most secure one ever invented; for it had sliders or tumblers of different lengths, and could not be opened unless they were all raised to the proper heights, and no higher.

NICKEL VERSUS BRONZE.—A financial committee of inquiry, appointed to consider the question of substituting a nickel for the bronze coinage at present in use in France, has finally decided in favor of the project, which, it may be mentioned, has already been adopted by other countries, and notably by Germany, Belgium, and Switzerland. The work thus thrown upon the mints of Paris and Bordeaux will be gigantic, it being estimated that there are 500,000,000f. worth of bronze coins in circulation; but the necessary appliances are already in hand, and the work will be rapidly proceeded with. It should be known that coins of copper-nickel have been introduced in North America, Peru, Brazil, and Honduras; and in 1869 Professor Graham, the last Master of the Mint, issued a coinage of the same kind for the Island of Jamaica in penny and halfpenny pieces, and very handsome coins they are. It has been pointed out by Dr. Walter Flight, of the British Museum, South Kensington, in the *Journal* of the Chemical Society for April of this year, that he was informed by Professor Graham in 1869 that he should have advocated the issuing of a coinage of the same kind in the British Isles if only a sufficient supply of nickel could at all times be obtained. Our sources of nickel have been materially increased during the twelve years interval. It is a matter of no little interest, as pointed out by Dr. Flight, that more than 2100 years ago copper-nickel coins were used in Bactria by the kings Agathokles, Pantaleon, and Euthydemus, the composition of which were identical with those now coined, although nickel was only discovered in 1751 by Crondstadt.—*The Engineer.*

IN their sixth annual report, Colonel Majendie, and Major A. Ford, the Inspectors of Explosives, say: "Experiments conducted by us appear to establish very satisfactorily that the effect of small charges of dynamite, and similar explosives, upon masonry structures is essentially local. Where the charge is in contact with an external portion of the structure, any effect which may be produced is almost entirely confined to a complete or partial penetration of the structure at the spot where such contact occurs; while if the charge be not in contact with any part of the structure, the result in the case of an external explosion is either wholly or nearly negative, while if occurring in the interior of a building any effect which may be produced is limited to the more or less complete demolition of the chamber or portion of the structure in, or in the immediate neighborhood of which the explosion was effected. General or even partial destruction of a public building, or of a substantial dwelling house could not be accomplished except by the use of very much larger charges of dynamite and similar substances than could usually be brought to bear without attracting observation, and the effect of a single 'Infernal Machine,' containing a few pounds of explosive would be structurally insignificant."

FURNACE SLAG AND BAUXITE FOR CEMENT.—We learn from *Stahl and Eisen* that Herr Roth, mining engineer, of Wetzlar, uses bauxite in the manufacture of cement from blast

furnace cinder. Bauxite consists principally of alumina hydrates, besides small quantities of sesquioxide of manganese, titanic acid, lime, magnesia, alkali, &c.; but its chemical composition varies according to the localities where it is deposited. Its name is derived from the place where it was first discovered, Les Baux, in France; it also occurs in the Charente. In Italy it is found in Calabria; in Ireland, near Belfast; in the Austrian Empire, in Krain, Styria, and Lower Austria. In Germany bauxite occurs on the southern slope of the Westerwald near Mühlbach and Hadamar, also at the Vogelsberg, in Upper Hessen, and at Klein-Steinheim, near Hanau. If 100 parts of furnace cinder, which crumbles by itself, are mixed with 85 parts of limestone or chalk (containing 98 per cent. of carbonate of lime and 2 per cent. of silicic acid) and 15 parts of bauxite (containing 48.5 per cent. of alumina, 13.52 per cent. of sesquioxide of iron, and 9.40 per cent. of silicic acid; the composition of the bauxite found near Giessen), and burned, the product yielded—supposing that half of the sulphur escapes from the slag as sulphureted hydrogen—is 158.66 parts of cement of the following composition: Lime, 61.9 per cent.; silicic acid, 24.1 per cent.; alumina, 10.6 per cent.; sesquioxide of iron, 1.3 per cent.; protoxides of iron and manganese, 0.8 per cent.; magnesia, 1 per cent.; sulphur, 0.3 per cent. The cinder used was obtained in the production of foundry pig in a coke blast-furnace; if the cinder to be employed is of a different composition, the fluxing materials must be varied. Herr Roth demonstrates the economical advantages to be derived from the erection of special cement mills near blast furnaces.

Mr. A. J. Hadcock, A. Inst. Chem., recently related the following: A kettle filled with boiling water was hung in the hottest room of some Turkish baths with the lid on. The temperature of the surrounding air was 262 deg. Fah. After about an hour the temperature of the water was taken, and indicated, as was expected, 212 deg. The kettle was then re-hung with the lid off. The temperature of the room was now 252 deg. In twenty minutes the temperature of the water had fallen to 185 deg., in thirty minutes to 178 deg., in forty-five minutes to 170 deg., and was evidently still falling. The manager stated that it generally fell finally to about 140 deg., when a point of equilibrium seemed to be established, and the water neither got hotter nor cooler. Mr. Hadcock supposes the loss of heat was due to rapid vaporization, and conversion of the sensible heat of the water into the latent heat of steam, and as dry air is a very bad conductor of heat—one of the worst known—the heat required to convert a portion of the water into steam had to be abstracted from the remainder of the water, thus lowering its temperature. In substantiation of this explanation it is well known that if water is placed in a vessel over a large bulk of strong sulphuric acid, in the receiver of an air-pump, and the air is exhausted, the rapid evaporation of one portion of the water will actually cause the rest to freeze.

The *Nautical Gazette* says that during the year 1881 the vessels lost at sea averaged about one every four hours. A large proportion of these losses occurred from carelessness, and mostly in fogs and other darkness. There were 400 ocean steamer collisions in 1879 and 1880 in the North Atlantic Ocean alone. Each of these might have been avoided if the master of one colliding vessel had been informed in proper time of the course pursued by the approaching one. These losses gave an average of over one steamer a day in which human life was sacrificed and valuable property destroyed. The *Gazette* believes that if a system of fog signals had been in use, such as the Barker code, nearly all of these disasters would have been prevented or avoided.

For cleaning old and soiled engravings, Mr. W. Brooks, writing in the *Journal of Photography*, recommends the use of Holme's ozone bleach. The strength he prefers is one part of ozone bleach to ten of water, well shaken up before pouring into a dish. He immerses the engraving in the solution, face up ward, avoiding bubbles. The only caution to be observed is that when the engraving is sodden with water it is somewhat rotten; so the less it is handled the better. Sometimes, if the engraving be only slightly stained, half an hour is quite sufficient, but when quite brown he has left them in for as long as four hours. After all the stains are removed, and the paper has regained its pure whiteness, pour the solution out of the dish into a bottle, as this can be used over and over again, until it becomes discolored; then fill up the dish with water, changing frequently for about two hours, or, better still, place it in running water. When sufficiently washed it can be taken out and blotted off and then hung up to dry, and when perfectly dry, iron on the back with a warm flat-iron; but care must be taken not to have it too hot.

The annual iron produce of the world is calculated from the most recent statistics to yield some 19½ millions of tons. The yield from all the more important countries has been ascertained up to the year 1881. In regard to the others, it is assumed that the yield has not fallen off since the latest figures reported. For the year 1881 the yield of Great Britain was 8,377,364 gross tons; United States, 4,144,254; Germany, 2,863,400; France, 1,866,438; Belgium, 622,288; Austria-Hungary, for 1880, 448,685; Sweden, for the same year, 399,628; Luxembourg, for 1881, 289,212; Russia, 231,341; Italy, for 1876, 76,000; Spain, 73,000; Turkey, 40,000; Japan, 10,000; and all other countries, 46,000. Under "other countries" are included Canada, Switzerland, and Mexico, each producing about 7,500 tons per year, and Norway, with 4,000 tons per year. The grand total is 19,487,610. Great Britain, the United States, Germany, and France, produce no less than 88.4 per cent. of the world's iron supply; the first two 64.3 per cent., and Great Britain alone 43 per cent. The chief consumer is the United States, taking 29 per cent.; Great Britain comes next with 23.4 per cent.; and these two use more than half the whole supply.

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VAN NOSTRAND'S

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THE CONSTRUCTION OF ELECTRO-MAGNETS.

By TH. DU MONCEL.

Translated from the French for VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

If in the problem now under discussion the force of electro-magnetic attraction is to be expressed as a weight, it is plain that the formula

$$\frac{E^{\frac{2}{3}}}{R^{\frac{2}{4}}}\cdot\frac{Q}{f^2}$$

will represent this force (which we will call P), only by applying a coefficient K, which expresses the ratio $\frac{F'}{P'}$ deduced from the standard electro-magnet, whose attraction at a millimeter distance is

$$\frac{0.002297}{26.85}=0.00008555.$$

The quantity F' represents in the standard magnet the value $I^2t^2c^{\frac{3}{2}}$, and if we assume

$$\frac{ne^{\frac{2}{3}}}{(n\rho+r)^{\frac{2}{4}}}\,\frac{Q}{f^2}=PK$$

we can easily deduce the value of ne, which will be

$$ne=\sqrt{n\rho+r}\,\sqrt[2]{\sqrt[3]{\frac{f^4P^2K^2}{Q^2}}}$$

As Q and K are constants they may be removed from under the radical sign, and the expression easily reduces to

$$\frac{n^2e^2}{n\rho+r}=\left(0.0225\ \sqrt[3]{\sqrt[3]{f^4\ P^2}}\right)^2$$

The quantity in parenthesis which may readily be calculated, may be represented by A, and we obtain

$$n=\frac{A^2\rho}{2e^2}+\sqrt{\left(\frac{A^2\rho}{2e^2}\right)^2+\frac{A^2r}{e^2}}.$$

The values of ne and $n\rho$ being determined, it becomes easy to calculate the dimensions of an electro-magnet by means of the formulas already given.

In the preceding calculations the resistance of the circuit is supposed great enough to require that the battery employed should be arranged for *intensity;* but if the resistance is so small that the battery may be arranged for *quantity*, the calculations will require some modification. Two cases present themselves; the resistance R may be nothing; or it may be simply less than nr. In the first case we cannot obtain determinate values for a and b (the first representing the number of rows of cups in the battery, and b the number of cups in each

row), for the internal resistance of the battery which is then $\frac{a}{b}\rho$, and the resistance H of the helix, being two indeterminate quantities which should be equal, we can assign values at pleasure between the limits corresponding to the values arising from the condition of the battery being arranged entirely for intensity or entirely for quantity.* But we can obtain the value ab or the total number n of the elements by the preceding formulas, which give

$$ab=\frac{\rho}{e^2}\,0.000506\,\sqrt[3]{\sqrt[3]{P^4 f^8}}$$

or

$$ab=\frac{\rho}{e^2}\left(0.0225\,\sqrt[3]{\sqrt[3]{P^2 f^4}}\right)^2$$

Under these conditions the value of c is determinate, and the formula gives

$$c=\frac{e\sqrt{ab}}{\sqrt{\rho}}\,0.173. \tag{13}$$

In the second case, as the resistance ought to be equal to $\frac{a}{b}\rho+r$ in order to satisfy the conditions of maximum effect in the magnet, and as in order to give the resistance of the circuit a minimum value, corresponding to a maximum for I, a resistance based upon that of the electro-magnet, it is necessary that $\frac{a}{b}\rho=r$, we shall be able to determine a and b whenever we know ab or n. This will be determined by the preceding equation, which gives the value of ab, since the value $\frac{a}{b}\rho+r$ becomes then equal to $2\frac{a}{b}\rho$. We have then

$$a=\sqrt{\frac{nr}{\rho}},\text{ and } b=\sqrt{\frac{n\rho}{r}},\text{ or }\frac{n}{a}.$$

If, instead of a simple circuit we have a circuit with x branches starting from the poles of the battery, and upon each of these branches there be placed an electro-magnet, all having the same resistances and the same dimensions, the intensity of the current upon each circuit would be

$$\frac{aE}{x\frac{a}{b}\rho+H},\text{ or }\frac{bE}{2x\rho},$$

and the value of ab or n would be the same as in the case of the simple circuit multiplied by x.

In case the dimensions of the electro-magnet are all given, and it is desired to determine the size and arrangement of the battery to obtain the force P upon each of the x derived circuits:—

From the assumed conditions, the value of H in the equation

$$I=\frac{aE}{x\frac{a}{b}\rho+H}$$

is known; and we also know that for maximum effect $x\frac{a}{b}\rho$ should be equal to H. Furthermore, we know that we should have

$$I^2t^2c^{\frac{2}{3}}=PK.$$

Now, as the quantities t^2 and $c^{\frac{2}{3}}$ can be easily calculated, since c is given, the value of I is readily found.

$$I=\sqrt{\frac{PK}{t^2c^{\frac{2}{3}}}};$$

from which we deduce

$$a=\frac{2\,IH}{E}\text{ and } b=\frac{ax\rho}{H}.$$

V.—Numerical Examples, Applying the Foregoing Formulas.

To apply the preceding formulas to some practical cases, we will at first suppose that we wish to determine the best

* This appears clear from the following calculation: The attractive force of an electro-magnet may be represented by I^2H, and when the battery is arranged in series, this expression becomes

$$A=\frac{a^2E^2H}{\left(\frac{a}{b}\rho+H\right)^2}=\frac{a^2E^2H}{4\frac{a^2}{b^2}\rho^2}$$

Now, suppose the battery to consist of 24 elements, each having a resistance of 12, then the electromotive force being equal to 1, when the elements are united for intensity,

$$A=\frac{1\times 288}{4\times 12^2}=0.5,$$

when united for quantity,

$$A=\frac{24^2\times 1\times 0.5}{4\times 12^2}=0.5,$$

and when arranged so that $a=b$, then

$$A=\frac{24\times 1\times 12}{4\times 12^2}=0.5,$$

all the values being identical.

See also Chapter IX of this essay.

dimensions for an electro-magnet to be worked by a single Bunsen cup of medium size, upon a circuit having no other resistance than that of the helix. We will call the electromotive force 1.86, a Daniel element being the unit and the resistance that of 57 meters of telegraph wire, which is 0.57 ohm. These first given quantities exhibit the necessity of knowing the constants for the different batteries in use. They will be given further on.

From formula 10 we shall have for c the diameter of the iron core:

$$c = \frac{1.86}{\sqrt{57}} 0.172175 = 0.^{m}0424.$$

Each of the branches will have then a diameter of $0.^{m}0424$ and a length of $0.^{m}255$. The diameter g of the wire of the helix will be, by formula (7),

$$g = \sqrt{1.4\sqrt{\frac{0.0424^{3}}{57} 0.00020106}} = 0.^{m}004865$$

which includes the insulating cover. The wire alone would be $0.^{m}00336$ which is the value of g divided by f or 1.4.

The length of the wire would be by formula (5) $243.^{m}$, and the attractive force expressed as a weight at one millimeter, from the formula

$$A = \frac{I^{2}t^{2}c^{\frac{3}{2}}}{0.0000855} \text{ will be } 23.^{k}112.$$

If the quantity R is expressed in ohms instead of in meters of telegraphic wire, the constant of formula (7) should be 0.0000020106 instead of 0.00020106, so that the value of g would be

$$g = \sqrt{f\sqrt{\frac{c^{3}}{R} 0.0000020106}}.$$

The number of spiral turns is to be calculated by formula (6).

Suppose now that this electro-magnet be inserted upon a line of 100 kilometers, which with the resistance of the necessary battery composed of 20 Daniell elements will form a circuit of 118620 meters resistance (measured in telegraph wire); we shall have

$$c = \frac{20}{\sqrt{118620}} 0.172175 = 0.^{m}01,$$

or in electrical units

$$c = \frac{21.58 \text{ volts.}}{\sqrt{1186 \text{ ohms.}}} 0.015057 = 0.^{m}01.$$

Each of the branches will then have a diameter of $0.^{m}01$, and a length of $0.^{m}06$. The diameter of the wire will be $0.^{m}0002597$, including the covering, and $0.^{m}0001583$ without it, and the length will be 1116 meters. The attractive force equals 26.85 grams.

The different formulas show why electro-magnets that are introduced into long circuits should have small dimensions and have helices of fine wire; and why, on the contrary, for short circuits, the dimensions should be larger, and the battery be arranged for quantity.

The dimensions now to be proposed for an electro-magnet worked by a single Bunsen cell will appear to most people rather large; but they would be still greater if iron of a lower degree of magnetic saturation were to be employed. Thus a tubular electro-magnet having a diameter of $0.^{m}10$, a length on each arm of $0.^{m}30$, thickness of tube $0.^{m}01$, has been made to support a weight of 160 kilograms when charged by a single Bunsen cell. The helix being a copper wire 4 millimeters in diameter (not including the covering), 182 meters long and wound in 482 spiral turns. The poles of the magnet were not even furnished with an iron stopper.

With an ordinary electro-magnet $0.^{m}02$ in diameter, wound with copper wire one millimeter in diameter, the writer has never been able to obtain with a single Bunsen cell more than 12 kilograms of attractive force at contact with the poles.

We will assume now that we are to obtain a given force on a circuit of given resistance; that the attractive force is to be 273 grams at a millimeter distance; the circuit to have a resistance of 500 ohms, 50 kilometers of telegraph wire), to be charged with a single bichromate cell. In the proposed battery (potassium bichromate with sand) the electromotive force is 2 and the internal resistance about 10 ohms.

Applying the formulas in Chapter IV, we have

$$A = 0.0225 \sqrt[3]{\sqrt[3]{1.37^{4} \times 273^{2}}} = 0.09$$

and $A^{2} = 0.0081$.

$$n=\frac{8.1}{8}=\sqrt{\left(\frac{8.1}{8}\right)^2+\frac{0.0081\times50000}{4}}$$
$$=11.125$$

from whence

$$c=\frac{11.125\times2}{\sqrt{61125}}\times0.173=0.^{m}01553.$$

This gives for the length of each helix $0.^{m}0932$. Diameter of wire without covering, $0.^{m}0002842$. With its covering, $0.^{m}0003894$. Length of the wire, 1861 meters. Number of turns, 19078. Intensity of current, 0.0001859. The value $c^{\frac{3}{2}}=0.001935$.

Squaring the values of I and t and multiplying by $c^{\frac{3}{2}}$ we get for the electro-magnetic force 0.0243778517. Comparing this value with that of the standard electro-magnet, which is 0.002297, we find the ratio to be 10.6, which is very near the ratio demanded. The attractive force in grams of the standard being 26.85, and the force required above being 273 grams.

We will now suppose it is desired to obtain a force of 200 grams on each of six electro-magnets all connected with a battery of the same kind as the preceding.

The total number of cells will be given by the formula:

$$ab=\frac{6\times1000}{4}\times0.000506^{2}\sqrt[3]{\sqrt{200^4\times\overline{1.4}^3}}$$
$$=10{,}778,$$

or say 11 cells. But as this number is not readily divided into groups, we will take 12. With this number of cups we can arrange them in 3 rows of 4 cups, or in 2 rows of 6 cups each, but in either case we shall have

$$c=\frac{\sqrt{12}}{\sqrt{1000\times6}}\times2\times0.173=0.^{m}0153$$

and consequently the length of each branch of the electro-magnets will be $0.^{m}0918$, or about a tenth of a meter.

Now we will compare the values obtained under the supposition of the two different arrangements of the 12 cells.

1st, when $a=3$ and $b=4$.

$g^2=0.000000560075.$
$g=0.0007484.$
$\frac{g}{f}=0.0005346.$
$H=482^{m}$
$t=5015,\ t^2=25150225.$
$I=0.000666.$
$I^2=0.000000443556.$
$c^{\frac{3}{2}}=0.001892.$
$I^2t^2=11.155533.$
$I^2t^2c^{\frac{3}{2}}=0.021084.$
$A=247$ grams.

2d, when $a=2$ and $b=6$.

$g^2=0.00000083986.$
$g=0.000916.$
$\frac{g}{f}=0.0006543.$
$H=321^{m}.$
$t=3345,\ t^2=11189025.$
$I=0.001,\ I^2=0.000001.$
$c^{\frac{3}{2}}=0.001892.$
$I^2t^2=11.189025.$
$I^2t^2c^{\frac{3}{2}}=0.0211686353.$
$A=247$ grams.

It is not necessary, however, to conclude that in order to maintain a good condition of saturation in the magnets, that we ought to vary their dimensions according to the resistance of the exterior circuit. We can for some applications keep to the same dimensions for the magnet, by varying the force of the battery and the dimensions of the wire of the helix. Since the electromotive force of the battery employed, for the same diameter of electro-magnet, and a given electro magnetic force is proportional to the square root of the exterior resistance of the circuit, we may conclude that if we augment the electromotive force E in a proper ratio, we may as the resistance of the circuit increases, continue the use of an electro-magnet of the same diameter and having the same force; consequently, if instead of inserting our standard magnet of $0.^{m}01$ diameter in a circuit of 100 kilometers, we introduce in a circuit of 400 kilometers with double the number of cells, we shall have very nearly the same effect produced. But it would be necessary to change the dimensions of the wire, for the diameter of the magnet remaining the same, the diameter of the wire should vary in the inverse ratio of the fourth root of the resistance of the exterior circuit. It would be necssary, then, in

the example cited that this diameter should be to the diameter of the wire of the standard magnet 0.^{m}0001583.

as $\sqrt[4]{100000} : \sqrt[4]{400000}$,

or as 17.8 : 25.1,

which gives 0.000112. We see that for telegraph relays the assigned dimensions may be preserved.

It has been asserted above that in doubling the number of elements of a battery, we shall obtain *nearly* the same force upon a circuit of 400 kilometers as previously upon a circuit of 100 kilometers. The qualification *nearly* is necessary, for as the resistance of the circuit, the resistance of the battery is also counted, and as the latter varies with the number of elements employed, the real resistance of the circuit is 437240 meters instead of 400000 (400^{km}). Now this is a little less than 4 times 118620^m which represents the resistance of the first circuit, so that to obtain the same effect in the two cases it would be necessary that the number of elements (20) should be raised to 38.4 (say 39) instead of 40.

We will now consider the case of two small electro-magnets, each having 2200 meters resistance, of No. 15 wire, and connected with a Daniell battery of 8 cups arranged for intensity. Experiment proves that the force obtained from each would be 70 grams at a millimeter distance. Consequently the force developed by the pair would be 140 grams. Now if in place of the two magnets only one had been employed, the force would have been 200 grams. There is then a loss of 60 grams by the use of the second magnet, although both of them are connected directly with the poles of the battery. It would seem at first sight as though there were some defect in the electrical connections, but we will proceed to show that calculation verifies the above result.

Applying Ohm's formulas to the two cases, we find: 1st, that in the case of the single electro-magnet, if we represent the electro motive force of the Daniell cell by 5973, the intensity of the current will be 4.95; 2d, that, in the case of the two electro-magnets forming two branches the intensity is 2.79.

The forces of the electro-magnets under these two arrangements are as the squares of these numbers 4.95 and 2.79, and if we estimate the force of the electro-magnet in a simple circuit at 200 grams, the force of the two others would be given by the formula

$$\frac{\overline{2.79}^2 \times 200}{\overline{4.95}^2} = 64 \text{ grams.}$$

Consequently the force of the pair of magnets would be 128 grams against 200 grams for the single magnet.

The result arises from the arrangement of the battery which was not adjusted to accord with the exterior resistance, since its resistance is nearly four times as great as the latter, and is again less than the resistance of the circuit containing the two magnets. The formula

$$\frac{nE}{2n\rho + H},$$

which represents the intensity of the current upon each branch, and which, in case of a battery arranged in series, gives for condition of maximum effect $2\frac{a}{b}\rho = H$, or $\frac{a}{b}\rho = \frac{H}{2}$, shows that in this case the resistance of the battery ought to be half that of each circuit, consequently the number a of the elements in tension should be determined by the formula:

$$a = \sqrt{\frac{8 \times 2200}{2 \times 931}} = 3.074.$$

As we cannot divide a cell of a battery, and as the plan requires that the numbers expressing values of a should be divisors of n, the battery which best lends itself to the several conditions of maximum is the one which most readily admits of division into various groups, and we see that in the case before us the conditions require 3 cups in tension and 3 in quantity. Now under these conditions, the attractive force of the single electro-magnet would be 267 grams, and of the two magnets 316 grams. There is then a gain in the use of the two magnets.

The foregoing examples show the importance of making calculation precede the construction of electro-magnets and the arrangement of the battery.

VI.—Experimental Verification of the Laws of Electro-Magnetism.

The formulas given in the preceding chapters have all been verified by the writer's experiments, except such as are generally adopted and which present difficulties in verification as we shall see forthwith.

This experimental work seemed the more important, as formerly many sicentists would not accept the formulas without discussion, and furthermore mathematical calculations are not always convincing. The writer has made many experiments to prove the truth of his deductions and with results as follows:

1st. The experimental demonstration of the principle which establishes the rule that the resistance of the electro-magnet should be equal to that of the exterior circuit is quite difficult by reason of the difficulty of finding wire of different sizes whose conductivities are exactly proportioned to the squares of the diameters. The experiments tried gave contradictory results, and it is proper to say, some that were contrary to the theoretical conclusions, which led in the beginning of these researches to some hesitation in accepting them, although they had been already generally accepted. Meanwhile as the other conclusions have been confirmed by experiment, and as the ill success of those experiments relating to this subject may have been due to accidental causes, the principle above stated has been admitted in those cases in which it is really applicable: that is to say when the size and length of the spool or helix having been fixed, it is required to know the size of the wire which should be employed to correspond to a circuit of given resistance. It is a case in which the elements of the construction have been already determined, since the size of the iron core should in the first place be proportioned to the intensity of the current, and since the thickness of the helix should be determined by another important principle.

This is not the same problem as the determination of the resistance of the circuit upon which a given electro-magnet or galvanometer can be profitably employed, without having previously considered its dimensions. In this latter case, the conditions of maximum effect as established by the writer show that, *the resistance of the electro-magnet should be to the resistance of the exterior circuit, as the thickness of the helix plus the diameter of the core, is to the thickness of the helix.*

This rule has been established by the following experiments:

Upon a spool whose length between the ends was $0.^{m}061$, and with a tube $0.^{m}011$ in diameter there was wound with great care two lengths of 60 meters each of No. 15 wire, also another length of $57\frac{1}{4}$ meters which formed a third helix. These three helices presented their ends outward each distinct from the other so that they could be used separately or together. The first offers a resistance of 1800^{m} of telegraph wire; the second had nearly the same so that the two joined made a resistance of 2160 meters; and finally, the third helix added to the two first furnished a total resistance of 3200 meters.

In subjecting this electro-magnet to the test of the writer's electro-magnetic balance, and applying to that pole only which was enclosed by the helix, and exciting the magnet by the current from a Leclanché battery of 3 cups, each with a resistance of about 400^{m}, the following results were obtained:

Resistance of exterior circuit.	Helix A. 1080^{m}.	Helix B. 2160^{m}.	Helix C. 3200^{m}.
m. m.	gr.	gr.	gr.
1200+ 0 (1200)	F=112	F′=122	F″=112
1200+ 400 (1600)	F= 73	F′= 92	F″= 95
1200+1000 (2200)	F= 47	F′= 66	F″= 70
1200+2000 (3200)	F= 27	F′= 43	F″= 50
1200+3000 (4200)	F= 17	F′= 29	F″= 36
1200+4000 (5200)	F= 12	F′= 22	F″= 28

The attractive forces F, F′, F″ were measured at the distance of one millimeter from the pole.

Now this table shows that the helix B, whose conditions of resistance most nearly accord with the theoretical conditions,

$$\left\{ \frac{2160}{1+\frac{c}{a}} = 982 \right\}$$

affords the maximum effect, and it is only when the resistance of the exterior circuit becomes 1600^{m} or $\frac{3200}{2}$ that the

helix of highest resistance C exhibits a preponderance.

With 2 cups of the battery and an exterior resistance of 800^m (represented by the battery), the helix B gave a force of 60 grams, while A and C each gave 57 grams. With a single cup and its resistance of 400, the helix A had the advantage, and the forces were 21 grams for A, 19 for B and 17 grams for C.

These experiments repeated with the galvanometer were yet more decisive as the reader may readily verify by consulting the account published by the author in *Comptes rendus des Séances l'Académie des Sciences*, Aug. 13th, 1877.

2d. In order to demonstrate that upon a branched circuit, the resistance which is to serve as the basis of calculation of that of an electro-magnet is the total exterior resistance *taken inversely*, that is to say as though the generator had been substituted for the electro-magnet—a sensitive Ruhmkorff galvanometer was inserted in a circuit having a very feeble generator. The galvanometer had two multipliers, one of 733 kilometers resistance, and the other of 237^{km}. A rheostat was added to this circuit, and a second one to a branch joining the two extremities of the galvanometer wire.

Calling R the circuit of the electrometer, u the galvanometric derivation, l the derived circuit in which the galvanometer was inserted, the equation expressing the condition of maximum effort was

$$l+\frac{Ru}{R+u}=\frac{\pi ba^2}{g^2}.$$

It is true that l cannot well influence this calculation, since the points of branching were the two extremities of the galvanometer; but by means of the two rheostats, the resistances u and R could be so varied as to present a combined *total* resistance, which was equal to, or greater, or less than the less resistant of the multipliers of the galvanometer; so that it sufficed, R being given, to calculate u by the formula

$$u=\frac{RH}{R+H},$$

H representing the resistance of the multiplier.

The results obtained after taking all precautions to preserve proper conditions were as follows:

Derived circuits. km. km.	Total resistance. km.	Multiplier 237^{km}. °	Multiplier 733^{km}. °
R=512+272 u=86	78	I=27½	I′=24½
R=512+272 u=128	110	I=32½	I′=30
R=512+272 u=200	160	I=36½	I′=36½
R=512+272 u=256	193	I=40	I=41
R=512+272 u=512	309	I=46	I=51
R=512+272 u=200	145	I=36	I=33½
R=512+272 u=512	260	I=47	I=52

It will be seen by this table that, as was said above, the multiplier (resistance coil) of greatest resistance has the advantage even where the total resistance of the circuit outside of the galvanometer is nearly equal to the smaller resistance coil; and it is only when this total resistance falls below 160^{km} or 145^{km}, a point at which the two multipliers have the same sensitiveness that the multiplier of least resistance exhibits an advantage. As with the galvanometer experiment, the ratios of the two resistances (the exterior circuit and the resistance coil), ought to be according to the conditions of maximum deduced from calculation 1.9 and 2.425—the resistances of the exterior circuit where the multipliers could be applied to the best advantage, should be 98^{km} for the multiplier of 237^{km}, and 386^{km} for the multiplier of 733^{km}; and the point at which they become equally sensitive ought to correspond to a resistance which is a mean proportional between these two numbers. Now this mean proportional being 193, and as the number furnished by experiment is 160, we may conclude that the principle previously stated has been practically verified. It is capable of further proof by calculating the resistance of u, which would equal deflections for the two resistance coils. This resistance is given by the formula

$$u=\frac{R(t'H-tH')}{t(R+H')-t'(R+H)},$$

which gives under conditions established for the galvanometer experiment, 224^{km}, while the experiment indicates 200^{km}.

3d. The principle governing the thick-

ness of the magnetizing helix—that it should be equal to the diameter of the iron core—is readily verified. To demonstrate this experimentally, the writer took three magnets of the same length but of different diameters, as follows: 1st, 0.^{m}02; 2d, 0.^{m}01, and 3d, 0.^{m}0065. Only one pole was brought to act upon the electro-magnetic balance; each spool was wound with No. 16 wire; 23 layers of 111 turns each, or a total of 2553 spiral turns. The wire was closely wound in each case, and the helices had a uniform thickness of 0.^{m}01.

It follows, therefore, that the dimensions of the 2d magnet alone corresponded with the conditions of maximum effect as previously stated. This magnet had a resistance of 3200 meters, while the larger offered a resistance of 5200, and the smaller one 2800 meters.

The results of the experiments are exhibited in the following table. The battery consisted of from 1 to 3 Leclanché cells, each having a resistance of 400^m. The magnetic attraction was estimated at a millimeter distance.

	Resistances of circuit.			Attractive force of the magnets. No. 1. 0.^{m}02	No. 2. 0.^{m}01	No. 3. 0.^{m}0065
	m.	m.	m.	gr.	gr.	gr.
3 cells.	1200+	0	(1200)	76	112	86
	1200+	1600	(2800)	48	57	(44)
	1200+	2000	(3200)	43	(50)	39
	1200+	4000	(5200)	(28)	29	22
	1200+	10000	(11200)	13	10	8
2 cells.	800+	0	(800)	38	57	46
	800+	2000	(2800)	22	26	(20)
	800+	2400	(3200)	20	(22)	16
	800+	4400	(5200)	(14)	13	10
	800+	10000	(10800)	7	5	4
1 cell.	400+	0	(400)	12	18	15
	400+	2400	(2800)	7	7	(6)
	400+	2800	(3200)	6	(6)	5
	400+	4800	(5200)	(5)	4	4
	400+	10000	(10400)	3	2	2

The table clearly shows that for the same resistance of exterior circuit, and with sufficient intensity of current, the advantage is with the magnet whose diameter equals the thickness of the helix. This advantage is maintained so long as the forces produced through the circuits of different resistances are properly proportioned to the resistances of the electro-magnets; and such conditions only are considered in the formula. It is only when the electric force is so feeble that the increase of magnetic action with the increase of diameter is but slightly marked, that the superiority of magnet No. 2 is affected. This is in accoad with Müller's law, for the attractive forces of the magnets are proportional to their diameters only when the magnets are charged nearly to the point of saturation. By the term saturation is meant that magnetic state which the magnet would retain if it were hardened steel instead of iron. When the force developed is much below this, it is dependent chiefly upon the intensity of the current, and that is naturally the strongest on the circuit of least resistance.

The numbers in parenthesis in the preceding table indicate the forces corresponding to maximum conditions relative to the circuit, and these conditions have been established on the hypothesis that the resistance of the exterior circuit was equal to the resistance of the helix, and in the construction of these electro-magnets a given thickness of helix was the starting point. If it had been based upon maximum conditions of a given electro-magnet without consideration of dimensions the preceding law would assert itself still more, for the maximum resistances of the exterior circuit become 2600^m for the larger magnet, 1600^m for No. 2, and 1400^m for No. 3. The attractive forces of the three magnets then become:

	With 3 cells.	With 2 cells.	With 1 cell.
For magnet No. 1..	50 gr.	25 gr.	8 gr.
" " 2..	94	46	14
" " 3..	79	41	13

VII.—Effects upon Electro-magnets of Saturation more or less Complete.

As was said in a previous chapter, the laws governing electro-magnets can not all be stated with desirable precision by reason of the variable resultant effect of saturation of the soft iron core; but this cause of perturbation is far from being as prominent as some scientists would have us believe, and it interferes with the definite results which they have deduced, much less than the effects of polarization have upon calculated results of electric currents. During the last researches upon electro-magnets, the writer wished to assure himself of the importance of this cause of perturbation,

and a large number of experiments were undertaken which seem to be of sufficient interest to be related here, inasmuch as the rapid extension of the application of electro-magnetism renders it daily more important that the best working conditions of electro-magnets should become known.

In the second chapter of this essay some general principles were stated, deduced from the author's experiments, but not much stress was laid upon these experiments as they were not conducted with reference to any law outside of those formulated by Dub and Müller; but it is important that a more definite explanation should now be given.

First it will be shown that having determined that the number 11 is the proper multiplier by which to determine the length of the magnet after its diameter is fixed, it will follow that the attractive force will increase in proportion to the length. So that starting with a given length of wire to represent the resistance of the exterior circuit, and winding it upon electro-magnets of different diameters so as to furnish each with a thickness of helix equal to the diameter of the core, it would be necessary that the lengths should be different and that should satisfy the following conditions:

$$\frac{2\pi bc^2}{g^2}=\frac{2\pi b'c'^2}{g^2}, \quad \text{or } bc^2=b'c'^2,$$

so that the lengths would be inversely proportioned to the squares of the diameters. In this case the factor m (ratio of length in diameter) is no longer constant, and becomes proportional to the cubes of the diameters.

But then the law which directs that the electro-magnetic force should be proportional to the square of the number of turns of the helix wire, multiplied by the $\frac{3}{2}$ power of the diameter is not applicable, and it becomes necessary to resume the consideration of it so as to make a comparison of such forces among themselves, according to the law which states that these forces are proportional to the squares of the number of turns of the wire, multiplied by the diameters of the cores and the square roots of their length. We shall then have for the same intensity of current:

$$\frac{A}{A'}=\frac{c^2b^2c\sqrt{b}}{c'^2b'^2c'\sqrt{b'}}=\frac{c^3b^{\frac{5}{2}}}{c'^3b'^{\frac{5}{2}}}=$$

$$=\frac{c^3c'^5}{c'^3c^5}=\frac{c'^2}{c^2}=\frac{b}{b'},$$

which shows that *the forces are in the inverse ratio of the squares of the diameters, or proportional to the lengths;* always supposing that the iron cores are in the proper condition of saturation to render the laws of Dub and Müller applicable.

In order to make known the modifications required to the laws formulated connecting the state of saturation with the length and diameter, three electro-magnets having diameters of 0.^{m}008, 0.^{m}007 and 0.^{m}006 respectively, were wound with the same length of No. 12 wire, having a diameter of 0.^{m}00059 including its cover. This length was 71.^{m}47, and its resistance was equal to 722 meters of telegraph wire or 7.22 ohms; and the spools were so constructed that the thickness of the helix was in each case equal to the diameter of the core. The lengths of the spools were, consequently: the first, 0.^{m}059; the second, 0.^{m}077; and the third, 0.^{m}098; and the number of turns of wire were respectively, 1470, 1677 and 1842. The results given in the table on next page were obtained with a Leclanché battery of the later pattern, varying from 1 to 4 cells, each having a resistance of 113^{m} or 1.13 ohms. The attractive force was measured at the distance of a millimeter.

These results show that conformably to the theory, it is the longest electro-magnet which is generally the strongest, but is in *the same ratio as the length, only for electro-magnets of certain limits as to diameter, and for a certain intensity of current which corresponds probably to the saturation of the magnets.*

This intensity for electro magnets of 0.^{m}098 and 0.^{m}077 varies from 0.001082 0.001030, 0.00143 and 0.00113 in the four series of experiments; but in comparing the electro-magnets of 0.^{m}098 and 0.^{m}059, the proportion in question is no longer related to the intensity of the currents and the ratio of the forces is less than the ratio of the lengths. Further than this it may be remarked, that the

No. of cells.	Exterior resistances.	Force of magnet. $0^m,098$.	Ratios.	Force of magnet. $0^m,077$.	Ratios.	Force of magnet. $0^m,059$.
	m m m	gr.		gr.		gr.
4 Cells.	452+ 0 (452)	225	1,018	221	0,9210	240
	452+ 300 (752)	151	1,049	144	0,9660	149
	452+ 400 (852)	134	1,0635	126	0,9770	129
	452+1000(1452)	73	1,0900	67	1,0307	65
	452+2000(2452)	36	1,161	31	1,0333	30
	452+3000(3452)	21	1,235	17	1,0630	16
	452+4000(4452)	14	1,272	11	1,1000	10
3 Cells.	339+ 0 (339)	169	1,0432	162	0,953	170
	339+ 300 (639)	109	1,0792	101	1,010	100
	339+ 400 (739)	95	1,092	87	1,0117	86
	339+1000(1339)	50	1,163	43	1,0238	42
	339+2000(2339)	23	1,2105	19	1,0555	18
	339+3000(3339)	13	1,300	10	1.000	10
	339+4000(4339)	8	1,333	6	1,000	6
2 Cells.	226+ 0 (226)	100	1,111	90	1,000	90
	226+ 300 (526)	60	1,154	52	1,0196	51
	226+ 400 (626)	52	1,1818	44	1,0232	43
	226+1000(1226)	26	1,3000	20	1,000	20
	226+2000(2226)	11	1,375	8	1,000	8
	226+3000(3226)	6	1,500	4	1,000	4
	226+4000(4226)	4	2,000	2	1,000	2
1 Cell.	113+ 0 (113)	35	1,2070	29	1,036	28
	113+ 300 (413)	19	1,2666	15	1,071	14
	113+ 400 (513)	16	1,3333	12	1,0909	11
	113+1000(1113)	7	1,1666	6	1,200	5
	113+2000(2113)	2	1,0000	2	2,000	1
	113+3000(3113)	1	1,0000	1	"	0
	113+4000(4113)	0	"	0	"	0

forces of these electro-magnets is so governed by the state of saturation of the core that for the highest electric intensities, and upon circuits of lowest resistance, it is the shortest and largest electro-magnet that gives best results. Conversely the narrowest and longest magnet exhibits its advantage more and more in proportion as the electricity diminishes, whether this diminution is caused by lessening the number of cells of the battery, or by increasing the resistance of the circuit. The importance of such variations can be estimated by the relations of the forces exhibited in the second and fourth columns of the preceding table. It is readily understood why it should be so, as for very high intensities, the diameter of the longer electro-magnet is not in proper proportion to this intensity, and it has passed its point of saturation when the short and large magnet would have just attained it. On the other hand, the advantage of the magnet of small diameter, under the action of feeble currents, is explained by this consideration; that the magnetic mass of the electro-magnet being sufficiently great compared with the electric force, this force affects beneficially the greater number of the turns of the helix wire. We may conclude, then, that the dimensions to be given to an electro-magnet ought to depend essentially upon the electric force which excites it, and upon the resistance of the circuit in which it is to be placed. *When the circuit is long and the electric source is of feeble energy, the magnet should be long and of small diameter.* When, on the *contrary, the circuit is short and the electric current intense, the iron core should be of large diameter.*

This is expressed otherwise by the formula $c=\frac{E}{\sqrt{R}}0.173$, which is offered by the writer to aid in determining the diameters of electro-magnets when the other conditions are known.

In regard to the influence of the condition of the magnetic saturation of the iron, it is difficult to formulate its effects. Joule, Müller and Robinson all long ago recognized the fact, that at the commencement of the current and while

the magnetic state is yet far short of saturation, *the attractive force instead of increasing as the square of the intensity the current augments in a much higher ratio, exceeding the third or even the fourth power of the intensity.* But the fact is also verified, *that in proportion as the electro-magnetic force is developed this ratio diminishes rapidly to the point of saturation, remains stationary for a moment, and beyond this point diminishes till the forces are in simple ratio with the intensity of the current.*

If the magnetic force thus varies as it is being developed, similar results should be exhibited when it can be completely developed in magnets whose dimensions admit of different degrees of saturation for the same intensity of current. This hypothesis can be verified by the results of the experiments already cited.

Considering the case of the three magnets in the last example, in which the forces were developed by currents of different intensities; *we find that there is for each of them a degree of intensity for which the attractive force varies as the square of this intensity, and that above and below this limit it increases in a more or less rapid ratio. Also, that this limit varies with the dimensions of the electro-magnet.*

In preparing from these results the table which follows, we see: 1st, That it is the electro-magnet B which best conforms to the law of the squares of the electric intensities. 2d, That the larger magnet C affords the highest ratio of increase. 3d, That the smallest magnet A furnishes the lowest ratio. 4th, That the ratio of the forces for the three magnets varies rather more rapidly than it should to accord with the law of squares of the intensities, as the intensities become weaker, whether from reducing the number of cells or from increasing the resistance.

It is certain that these ratios, notwithstanding their larger value, correspond more nearly to the squares of the intensities than to their cubes. We have, for example, between the first and last experiment of the first series, 85600 instead of 19423; but we will remark that the three magnets have diameters not widely different; that they are nearly in the same condition as to saturation, and that the forces have been measured at the moment of maximum magnetization.

It is evident that we should not have obtained the same results with magnets differing more in their diameters, nor if the forces had been measured instantly at the moment of magnetization, and during a closing of the circuit. In such a case we should, perhaps, be able to get a higher ratio than 85600. Upon this principle is based the action of the sluggish electro-magnets employed in some of the printing telegraphs, and which are slow in action only because their mass being relatively large, considerable time is consumed in charging them.

The following table exhibits the ratios previously alluded to:

No. of cells.	Ratios of square of intensity of current.	Ratio of forces. A $0^m,098$.	Ratio of forces. B $0^m,077$.	Ratio of forces. C $0^m,059$.
4 Cells.	1,576	1,49	1,54	1,61
	1,797	1,68	1,75	1,86
	8.429	8,082	8,80	8,70
	7,309	6,25	7,18	8,00
	12,640	10,71	18,00	15,00
	19,428	16,07	20,09	24,00
3 Cells.	1,645	1,55	1,60	1,70
	1,896	1,78	1,86	1,97
	3,773	8,88	3,77	4,05
	8,328	7,85	8,52	9,44
	14,650	13,00	16,20	17,00
	22,753	21,12	27,00	28,33
2 Cells.	1,733	1,66	1,78	1,76
	2,022	1,92	2.04	2,09
	4,222	3,84	4.50	4,50
	9,670	9,09	11,25	11,25
	17,343	16,66	22,50	22,50
	27,242	25,00	45,00	45,00
1 Cell.	1,847	1,84	1,93	2,00
	2,187	2,19	2.42	2.55
	4,829	5,00	4,83	5,60
	11,527	17,50	14,50	28,00
	21,094	35,00	29,00	"
	33,529	"	"	"

From the numerical results given in this table, we are led to conclude that the law making the attractive forces proportional to the square of the intensities of the currents, holds good within certain limits, and under certain indications; and that the electro-magnets over which the currents are broken at short intervals, fail more or less. Now as this case is involved in the important question of

the magnetizing helix, it would seem opportune to consider it from this new point of view; not to establish the conditions of maximum effect which have been already determined, but to find out in what way they are to be modified to suit certain cases.

The question is this: When the electro-magnetic force increases in a more rapid ratio than that of the squares of the intensities of the current (say as the cubes for instance), should the resistance of the magnetizing helix be greater or less than that of the exterior circuit?

To answer this question it will suffice to change in the formula giving the value of electro-magnetic force the exponents of the values of the intensity, or in other words, change I^2 to I^3. We obtain then this expression:

$$8f^2\pi ba(a+c)=4qRg^4$$

which, if g varies, gives for a maximum

$$\frac{\pi ba(a+c)}{g^2}=\frac{qRg^2}{2f^2},$$

or

$$H=\frac{R}{2}.$$

If a is the variable quantity, the conditions of maximum correspond to the equation

$$\frac{qRg^2}{f^2}=\frac{\pi b\,a\left(2a+\frac{c}{2}\right)}{g^2}.$$

These two equations show that when the forces are proportional to the cubes of the intensity of the current, *the helices should offer less resistance than the exterior circuit; only half as much if g is the varible quantity, and in the ratio of $a+c$ to $2a+\frac{c}{2}$ when a is the variable.*

We may conclude, then, that upon circuits subject to frequent interruptions, the resistances offered by the magnets should be lessened in proportion as the intervals of closing the circuit are shortened. And it is for this reason as much as for that of defective insulation and of extra currents, that Hughes first, and subsequently telegraph engineers, reduced considerably the resistances of magnets upon long circuits.

It would be equally interesting to know in what way the resistance of the electro-magnet should be modified in the case where the point of saturation being passed, the force increases less rapidly than the square of the intensity of the current. If we suppose that this increase is simply in the ratio of the intensity, then there is no maximum possible, and we can with advantage increase the resistance of the magnets beyond the limits which have been assigned.

In seeking to determine how the conditions of maximum effect which involve the thickness of the helix should be modified to suit the conditions of defective saturation of the magnetic core, we find that we can increase this thickness, and that while the attractive force increases as the cube of the intensity, the thickness of the helix may with advantage be double the diameter of the core. The expression

$$\frac{E^3t^2c}{\left(\frac{\lambda\pi ba(a+c)}{g^2}\right)^3}$$

is a maximum if c is variable when $a=2c$. On the other hand, there is no maximum when the forces are simply proportional to the intensities of the current.

TRANSMISSION OF POWER BY ELECTRICITY.—A novel application of the electrical transmission of power has lately been made at the Trafalgar Collieries, Forest of Dean. The electrical arrangements were carried out by the Pyramid Electric Company, under the supervision of their managing director, Mr. A. Le Neve Foster. In this case an electric motor is used to drive a pump in the underground workings. The pump is employed for pumping the drainage water from some of the deep workings to the bottom of the shaft, from whence the ordinary steam pumps raise it to the surface. The total vertical lift of the electric pump is 115 feet whilst the length of pipes through which the water is forced is some 500 yards. A dynamo-machine is placed on the surface for generating the current for working the motor, and is connected to it by wires led down the shaft and along the workings, a distance of some 500 yards. Messrs. Brain are satisfied with the result of the undertaking, and propose still further to extend the utilization of electricity as a motive power.

ELECTRIC LIGHTING AND THE TRANSMISSION OF FORCE BY ELECTRICITY.

By DR. C. W. SIEMENS, F.R.S.

Amongst the practical questions that now chiefly occupy public attention are those of Electric Lighting, and of the transmission of force by electricity. I need hardly remind you that electric lighting, viewed as a physical experiment, has been known to us since the early part of the present century, and that many attempts have, from time to time, been made to promote its application. Two principal difficulties have stood in the way of its practical introduction, viz., the great cost of producing an electric current so long as chemical means had to be resorted to, and the mechanical difficulty of constructing electric lamps capable of sustaining, with steadiness, prolonged effects. The dynamo-machine, which enables us to convert mechanical into electrical force, purely and simply, has very effectually disposed of the former difficulty, inasmuch as a properly conceived and well constructed machine of this character converts more than ninety per cent. of the mechanical force imparted to it into electricity, ninety per cent. again of which may be reconverted into mechanical force at a moderate distance. The margin loss, therefore, does not exceed twenty per cent., excluding purely mechanical losses, and this is quite capable of being further reduced to some extent by improved modes of construction; but it results from these figures that no great step in advance can be looked for in this direction. The dynamo-machine presents the great advantage of simplicity over steam or other power-transmitting engines; it has but one working part, namely, a shaft, which, revolving in a pair of bearings, carries a coil or coils of wire admitting of perfect balancing. Frictional resistance is thus reduced to an absolute minimum, and no allowance has to be made for loss by condensation, or badly fitting pistons, stuffing boxes, or valves, or for the jerking action due to oscillating weights. The materials composing the machine, namely, soft iron and copper wire, undergo no deterioration or change by continuous working, and the depreciation of value is therefore a minimum, except where currents of exceptionally high potential are used, which appear to render the copper wire brittle.

The essential points to be attended to in the conception of the dynamo-machine, are the prevention of induced currents in the iron, and the placing of the wire in such position as to make the whole of it effective for the production of outward current. These principles, which have been clearly established by the labors of comparative few workers in applied science, admit of being carried out in an almost infinite variety of constructive forms, for each of which may be claimed some real or imaginary merits regarding questions of convenience or cost of production.

For many years after the principles involved in the construction of dynamo-machines had been been made known, little general interest was manifested in their favor, and few were the forms of construction offered for public use. The essential features involved in the dynamo-machine, the Siemens armature (1856), the Pacinotti ring (1861), and the self-exciting principle (1867), were published by their authors for the pure scientific interest attached to them, without being made subject matter of letters patent, which circumstance appears to have had the contrary effect of what might have been expected, in that it has retarded the introduction of this class of electrical machine, because no person or firm had a sufficient commercial interest to undertake the large expenditure which must necessarily be incurred in reducing a first conception into a practical shape. Great credit is due to Monsieur Gramme for taking the initiative in the practical introduction of dynamo-machines embodying those principles, but when five years ago I ventured to predict for the dynamo-electric current a great practical future, as a means of transmitting power to a distance, those views were still looked upon as more or less chimerical. A few

striking examples of what could be practically effected by the dynamo-electric current such as the illumination of the Place de l'Opera, Paris, the occasional exhibition of powerful arc lights, and their adoption for military and lighthouse purposes, but especially the gradual accomplishment of the much desired lamp by incandescence in vaacuum, gave rise to a somewhat sudden reversion of public feeling; and you may remember the scare at the Stock Exchange affecting the value of gas shares, which ensued in 1878, when the accomplishment of the sub-division of the electric light by incandescent wire was first announced, somewhat prematurely, through the Atlantic cable.

From this time forward electric lighting has been attracting more and more public attention, until the brilliant displays at the exhibition of Paris, and at the Crystal Palace last year, served to excite public interest, to an extraordinary degree. New companies for the purpose of introducing electric light and power have been announced almost daily, whose claims to public attention as investments were based in some cases upon only very slight modifications of well known forms of dynamo-machines, of arc regulators, or of incandescent carbon lights, the merits of which rested rather upon anticipations than upon any scientific or practical proof. These arrangements were supposed to be of such superlative merit that gas and other illuminants must soon be matters simply of history, and hence arose great speculative excitement. It should be borne in mind, however, that any great technical advance is necessarily the work of time and serious labor, and that when accomplished, it is generally found that so far from injuring existing industries, it calls additional ones into existence, to supply new demands, and thus give rise to an increase in the sum total of our resources. It is, therefore, reasonable to expect that side by side with the introduction of the new illuminant, gas lighting will go on improving and extending, although the advantage of electric light for many applications, such as the lighting of public halls and warehouses, of our drawing rooms and dining rooms, our passenger steamers, our docks and harbors, are so evident, that its advent may be looked upon as a matter of certainty.

Our Legislature has not been slow in recognizing the importance of the new illuminant. In 1879, a Select Committee in the House of Commons instituted a careful inquiry into its nature and probable cost, with a view to legislation, and the conclusions at which they arrived were, I consider, the best that could have been laid down. They advised that applications should be encourged tentatively by the granting of permissive bills, and this policy has given rise to the Electric Lighting Bill, 1882, promoted by Mr. Chamberlain, the President of the Board of Trade, regarding which much controversy has arisen. It could, indeed, hardly be expected that any act of legislation upon this subject could give universal satisfaction, because while there are many believers in gas who would gladly oppose any measure likely to favor the progress of the rival illuminant, and others who wish to see it monopolized, either by local authorities, or by large financial corporation, there are others again who would throw the doors open so wide as to enable almost all comers to interfere withe public thoroughfares, for the establishment of conducting wires, without let or hindrance.

The law as now established takes, I consider, a medium course between these diverging opinions, and, if properly interpreted, will protect, I believe, all legitimate interests, without impeding the healthy growth of establishments for the distribution of electric energy for lighting and for the transmission of power. Any firm or lighting company may, by application to the local authorities, obtain leave to place electric conductors below public thoroughfares, subject to such conditions as may be mutually agreed upon, the terms of such license being limited to seven years; or an application may be made to the Board of Trade for a provisional order to the same effect, which, when sanctioned by Parliament, secures a right of occupation for twenty-one years. The license offers the advantages of cheapness, and may be regarded as a purely tentative measure, to enable the firm or company to prove the value of their plant. If this be fairly established, the license would in all probability be affirmed, either by an engagement for its prolongation from time to time, or by a provisional order which would, in that

case, be obtained by joint application of the contractor and the local authority. At the time of expiration of the provisional order, a pre-emption of purchase is accorded to the local authority, against which it has been objected with much force by so competent an authority as Sir Frederick Bramwell, that the conditions of purchase laid down are not such as fairly to remunerate the contracting companies for their expenditure and risk, and that the power of purchase would inevitably induce the parochial bodies to become mere trading associations. But while admitting the undesirability of such consummation, I cannot help thinking that it was necessary to put some term to contracts entered into with speculative bodies at a time when the true value of electric energy, and the best conditions under which it should be applied, are still very imperfectly understood. The supply of electric energy, particularly in its application to transmission of power, is a ma ter simply of commercial demand and supply, which need not partake of the character of a large monopoly similar to gas and water supply, and which may therefore be safely left in the hands of individuals, or of local associations, subject to a certain control for the protection of public interest. At the termination of the period of the provisional order, the contract may be renewed upon such terms and conditions as may at that time appear just and reasonable to Parliament, under whose authority the Board of Trade will be empowered to effect such renewal.

Complaints appear almost daily in the public papers to the effect that townships refuse their assent to applications by electric light companies for provisional orders; but it may be surmised that many of these applications are of a more or less speculative character, the object being to secure monopolies for eventual use or sale, under which circumstances the authorities are clearly justified in withholding their assent; and no licenses or provisional orders should, indeed, be granted, I consider, unless the applicants can give assurance of being able and willing to carry out the work within a reasonable time. But there are technical questions involved which are not yet sufficiently well understood to admit of immediate operations upon a large scale.

Attention has been very properly called to the great divergence in the opinions expressed by scientific men regarding the area that each lighting district should comprise, the capital required to light such an area, and the amount of electric tension that should be allowed in the conductors. In the case of gas supply, the works are necessarily situated in the outskirts of the town, on account of the nuisance this manufacture occasions to the immediate neighborhood; and, therefore, gas supply must range over a large area. It would be possible, no doubt, to deal with electricity on a similar basis, to establish electrical mains in the shape of copper rods of great thickness, with branches diverging from it in all directions; but the question to be considered is, whether such an imitative course is desirable on account either of relative expense or of facility of working. My own opinion, based upon considerable practical experience and thought devoted to the subject, is decidedly adverse to such a plan. In my evidence before the Parliamentary Committee, I limited the desirable area of an electric district in densely populated towns to a quarter of a square mile, and estimated the cost of the necessary establishment of engines, dynamo-machines, and conductors, at 100,000*l*, while other witnesses held that areas from one to four square miles could be worked advantageously from one center, and at at a cost not exceeding materially the figure I had given. These discrepancies do not necessarily imply wide differences in the estimated cost of each machine or electric light, inasmuch as such estimates are necessarily based upon various assumptions regarding the number of houses and of public buildings comprised in such a district, and the amount of light to be apportioned to each, but I still maintain my preference for small districts.

By way of illustration, let us take the parish of St. James's, near at hand, a district not more densely populated than other equal areas within the metropolis, although comprising, perhaps, a greater number of public buildings. Its population, according to the preliminary report of the census taken on the 1st of April, 1881, was 29,865, it contains 3,018 inhabited houses, and its area is 784,000 square yards, or slightly above a quarter of a square mile.

To light a comfortable house of moderate dimensions in all its parts, to the exclusion of gas, oil, or candles, would require about 100 incandescent lights of from 15 to 18-candle power each, that being, for instance, the number of Swan lights employed by Sir William Thomson in lighting his house at Glasgow University. Eleven-horse power would be required to excite this number of incandescent lights, and at this rate the parish of St. James's would require $3,018 \times 11 =$ 33,200-horse power to work it. It may be fairly objected, however, that there are many houses in the parish much below the standard here referred to, but on the other hand, there are 600 of them with shops on the ground floor, involving larger requirements. Nor does this estimate provide for the large consumption of electric energy that would take place in lighting the eleven churches, eighteen club-houses, nine concert halls, three theatres, besides numerous hotels, restaurants, and lecture halls. A theatre of moderate dimensions, such as the Savoy Theatre, has been proved by experience to require 1,200 incandescent lights, representing an expenditure of 133 horse power; and about one-half that power would have to be set aside for each of the other public buildings here mentioned, constituting an aggregate of 2,926-horse power; nor does this general estimate comprise street lighting, and to light the six and a half miles of principal streets of the parish with electric light, would require per mile, thirty-five arc lights of 350 candle power each, or a total of 227 lights. This, taken at the rate of 0.8-horse power per light, represents a further requirement of 182-horse power, making a total of 3,108 horse power, for purposes independent of house lighting, being equivalent to one-horse power per inhabited house, and bringing the total requirements up to 109 lights = 12-horse power per house.

I do not, however, agree with those who expect that gas lighting will be entirely superseded, but have, on the contrary, always maintained that the electric light, while possessing great and peculiar advantages for lighting our principal rooms, halls, warehouses, &c., owing to its brilliancy, and more particularly to its non-interference with the healthful condition of the atmosphere, will leave ample room for the development of the former, which is susceptible of great improvement, and is likely to hold its own for the ordinary lighting up of our streets and dwellings.

Assuming, therefore, that the bulk of domestic lighting remains to the gas companies, and that the electric light is introduced into private houses, only, at the rate of, say twelve incandescent lights per house, the parish of St James's would have to be provided with electric energy sufficient to work $(9 + 12)\ 3,018 = 63,$-378 lights = 7,042-horse power effective; this is equal to about one-fourth the total lighting power required, taking into account that the total number of lights that have to be provided for a house are not all used at one and the same time. No allowance is made in this estimate for the transmission of power, which, in course of time, will form a very large application of electric energy; but considering that power will be required mostly in the day time, when light is not needed, a material increase in plant will not be necessary for that purpose.

In order to minimize the length and thickness of the electric conductor, it would be important to establish the source of power, as nearly as may be, in the centre of the parish, and the position that suggests itself to my mind is that of Golden-square. If the unoccupied area of this square, representing 2,500 square yards, was excavated to a depth of twenty-five feet, and then arched over so as to re-establish the present ground level, a suitable covered space would be provided for the boilers, engines, and dynamo machines, without causing obstruction or public annoyance; the only erection above the surface, would be the chimney, which, if made monumental in form, might be placed in the centre of the square, and be combined with shafts for ventilating the subterranean chamber, care being taken of course to avoid smoke by insuring perfect combustion of the fuel used. The cost of such a chamber, of engine-power, and of dynamo-machines, capable of converting that power into electric energy, I estimate at 140,000*l*. To this expense would have to be added that of providing and laying the conductors, together with the switches, current regulators, and arrangements for testing the insulation of the wire.

The cost and dimensions of the conductors would depend upon their length, and the electromotive force to be allowed. The latter would no doubt be limited, by the authorities, to the point at which contact of the two conductors with the human frame would not produce injurious effects, or say to 200 volts, except for street lighting, for which purpose a higher tension is admissible. In considering the proper size of conductor to be used in any given installation, two principal factors have to be taken into account; first, the charge for interest and depreciation on the original cost of a unit length of the conductor; and, secondly, the cost of the electrical energy lost through the resistance of a unit of length. The sum of these two, which may be regarded as the cost of electricity, is clearly least, as Sir William Thomson pointed out some time ago, when the two components are equal. This, then, is the principle on which the size of a conductor should be determined.

From the experience of large installations, I consider that electricity can, roughly speaking, be produced in London at a cost of about one shilling per 10,000 Ampère-Volts or Watts (746 Watts being equal to one horse-power) for an hour. Hence, assuming that each set of four incandescent lamps in series (such as Swan's, but for which may be substituted a smaller number of higher resistance and higher luminosity) requires 200 volts electromotive force, and 60 Watts for their efficient working, the total current requird for 64,000 such lights is 19,200 ampères, and the cost of the electric energy lost by this current in passing through 1-100th of an ohm resistance, is 16*l*. per hour.

The resistance of a copper bar one quarter of a mile in length, and one square inch in section, is very nearly 1-100th of an ohm, and the weight is about $2\frac{1}{8}$ tons. Assuming, then, the price of insulated copper conductor at 90*l*. per ton, and the rate of interest and depreciation at $7\frac{1}{2}$ per cent., the charge per hour of the above conductor, when used eight hours per day, is $1\frac{1}{4}d$. Hence, following the principle I have stated above, the proper size of conductor to use for an installation of the magnitude I have supposed, would be one of 48 29 inches section, or a round rod eight inches diameter.

If the mean distance of the lamps from the station be assumed as 350 yards, the weight of copper used in the complete system of conductors would be nearly 168 tons, and its cost 15,120*l*. To this must be added the cost of iron pipes, for carrying the conductors under ground, and of testing boxes, and labor in placing them. Four pipes of 10 inch diameter each, would have to proceed in different directions from the central station, each containing sixteen separate conductors of one inch diameter, and separately insulated, each of them supplying a subdistrict of 1000 lights. The total cost of establishing these conductors may be taken at 37,000*l*., which brings up the total expenditure for central station and leads to 177,000*l*. I assume the conductors to be placed under ground, as I consider it quite inadmissible, both as regards permanency and public safety and convenience, to place them above ground, within the precincts of towns. With this expenditure, the parish of St. James's would be supplied with the electric light to the extent of about 25 per cent. of the total illuminating power required. To provide a larger percentage of electric energy would increase the cost of establishment proportionately; and that of conductors, nearly in the square ratio of the increase of the district, unless the loss of energy by resistance is allowed to augment instead.

It may surprise uninitiated persons to be told that to supply a single parish with electric energy necessitates copper conductors of a collective area equal to a rod of eight inches in diameter; and how, it may be asked, will it be possible under such conditions to transmit the energy of waterfalls to distances of twenty or thirty miles, as has been suggested? It must indeed be admitted that the transmission of electric energy of such potential (200 volts) as is admissible in private dwellings would involve conductors of impracticable dimensions, and in order to transmit electrical energy to such distances, it is necessary to resort in the first place to an electric current of high tension. By increasing the tension from 200 to 1,200 volts the conductors may be reduced to one-sixth their area, and if we are content to lose a larger proportion of the energy obtained cheaply from a waterfall, we may effect a still greater reduc-

tion. A current of such high potential could not be introduced into houses for lighting purposes, but it could be passed through the coils of a secondary dynamo-machine, to give motion to another primary machine, producing currents of low potential to be distributed for general consumption. Or secondary batteries may be used to effect the conversion of currents of high into those of low potential, whichever means may be found the cheaper in first cost, in maintenance, and most economical of energy. It may be advisable to have several such relays of energy for great distances, the result of which would be a reduction of the size and cost of conductor at the expense of final effect, and the policy of the electrical engineer will, in such cases, have to be governed by the relative cost of the conductor, and of the power at its original source. If secondary batteries should become more permanent in their action than they are at the present time, they may be largely resorted to by consumers, to receive a charge of electrical energy during the daytime, or the small hours of the night, when the central engine would otherwise be unemployed, and the advantage of resorting to these means will depend upon the relative first cost, and cost of working the secondary battery and the engine respectively. These questions are, however, outside the range of our present consideration.

The large aggregate of dwellings comprising the metropolis of London covers about seventy square miles, thirty of which may be taken to consist of parks, squares, and sparsely inhabited areas, which are not to be considered for our present purpose. The remaining forty square miles could be divided into say 140 districts, slightly exceeding a quarter of a square mile on the avarage, but containing each fully 3,000 houses, and a population similar to that of St. James's.

Assuming twenty of these districts to rank with the parish of St. James's (after deducting the 600 shops which I did not include in my estimate) as central districts, sixty to be residential districts, and sixty to be comparatively poor neighborhoods, and estimating the illuminating power required for these three classes in the proportion of 1 to $\frac{2}{3}$ to $\frac{1}{3}$, we should find that the total capital expenditure for supplying the metropolis with electric energy to the extent of 25 per cent. of the total lighting requirements would be—

$$\begin{aligned} 20\times\ \ \ \ \ \ 177{,}000&=3{,}540{,}000l. \\ 60\times\tfrac{2}{3}\times177{,}000&=7{,}080{,}000l. \\ 60\times\tfrac{1}{3}\times177{,}000&=3{,}540{,}000l. \\ \hline &\ \ 14{,}160{,}000l. \end{aligned}$$

or say 14,000,000*l*., without including lamps and internal fittings, and making an average capital expenditure of 100,-000*l*. per district.

To extend the same system over the towns of Great Britain, Ireland and would absorb a capital exceeding certainly 64,000,000*l*., to which must be added 16,000,000*l*., for lamps and internal fittings, making a total capital expenditure of 80,000,000*l*. Some of us may live to see this capital realized, but to find such an amount of capital, and, what is more important, to find the manufacturing appliances to produce work representing this value of machinery and wire, must necessarily be the result of many years of technical developement. If, therefore, we see that electric companies apply for provisional orders to supply electric energy, not only for every town throughout the country, but also for the colonies, and for foreign parts, we are forced to the conclusion that their ambition is somewhat in excess of their power of performance; and that no provisional order should be granted except conditionally on the work being executed within a reasonable time, as without such a provision the powers granted may have the effect of retarding instead of advancing electric lighting, and of providing an undue encouragement to purely speculative operations.

The extension of a district beyond the quarter of a square mile limit, would necessitate an establishment of unwieldy dimensions, and the total cost of electric conductors per unit area would be materially increased; but independently of the consideration of cost, great public inconvenience would arise in consequence of the number and dimensions of the electric conductors, which could no longer be accommodated in narrow channels placed below the curb stones, but would necessitate the construction of costly subways —veritable *cava electrica*.

The amount of the working charges of

an establishment comprising the parish of St. James's would depend on the number of working hours in the day, and on the price of fuel per ton. Assuming the 64,000 lights to incandesce for six hours a day, the price of coal to be 20*s.* a ton, and the consumption 2lbs. per effective horse power per hour, the annual charge under this head, taking eight hours' firing, would amount to about 18,300*l.*, to which would have to be added for wages, repairs, and sundries, about 6,000*l.*, for interest with depreciation at seven-and-a-half per cent., 13,300*l.*, and for general management say, 3,400*l.*, making a total annual charge of 41,000*l.*, or at the rate of 12*s.* 9½*d.* per incandescent lamp per annum. To this has to be added the cost of renewal of lamps, which may be taken at 5*s.* per lamp of sixteen candles, lasting 1,200 hours, or to 9*s.* per annum, making a total of 21*s.* 9½*d.* per lamp for a year.

In comparing these results with the cost of gas-lighting, we shall find that it takes 5 cubic feet of gas, in a good argand burner, to produce the same luminous effect as one incandescent light of 16-candle power. In lighting such a burner every day for six hours on the average, we obtain an annual gas consumption of 10,950 cubic feet, the value of which, taken at the rate of 2*s.* 8*d.* per thousand, represents an annual charge of 29*s.*, showing that electric light by incandescenee, when carried out on a large scale, is decidedly cheaper than gas-lighting at present prices, and with the ordinary gas-burners.

On the other hand, the cost of establishing gas-works and mains of a capacity equal to 64,000 argand burners would involve an expenditure not exceeding 80,000*l.* as compared with 177,000*l.* in the case of electricity; and it is thus shown that although it is more costly to establish a given supply of illuminating power by electricity than gas, the former has the advantage as regards current cost of production.

It would not be safe, however, for the advocates of electric lighting to rely upon these figures as representing a permanent state of things. In calculating the cost of electric light, I have only allowed for depreciation and 5 per cent. interest upon capital expenditure, whereas gas companies are in the habit of dividing large dividends, and can afford to supply gas at a cheaper rate, by taking advantage of recent improvements in manufacturing operations, and of the ever-increasing value of their by-products, including tar, coke, and ammoniacal liquor. Burners have, moreover, been recently devised by which the luminous effect for a given expenditure of gas can be nearly doubled by purely mechanical arrangements, and the brilliancy of the light can be greatly improved.

On the other hand, electric lighting also may certainly be cheapened by resorting, to a greater extent than has been assumed, to arc lighting, which though less agreeable than the incandescent light for domestic purposes, can be produced at less than half the cost, and deserves on that account the preference for street lighting, and for large halls, in combination with incandescent lights. Lamps by incandescence may be produced hereafter at a lower cost, and of a more enduring character.

Considering the increasing public demand for improved illumination, it is not unreasonable to expect that the introduction of the electric light to the full extent here contemplated, would go hand in hand with an increasing consumption of gas for illuminating and for heating purposes, and the neck-to-neck competition between the representatives of the two systems of illumination, which is likely to ensue, cannot fail to improve the quality, and to cheapen the supply of both, a competition which the consuming public can afford to watch with complacent self-satisfaction. Electricity must win the day, as the light of luxury; but gas will, at the same time, find an ever-increasing application for the more humble purposes of diffusing light.

In my address to the British Association I dwelt upon the capabilities and prospects of gas, both as an illuminant and as a heating agent, and I do not think that I was over-sanguine in predicting for this combustible a future exceeding all present anticipations.

I also called attention to the advantages of gas as a heating agent, showing that if supplied specially for the purpose, it would become not only the most convenient, but by far the cheapest form of fuel that can be supplied to our towns. Such a general supply of heating separ-

ately from illuminating gas, by collecting the two gases into separate holders, during the process of distillation, would have the beneficial effects—

1. Of giving to lighting gas a higher illuminating power.

2. Of relieving our towns of their most objectionable traffic—that in coal and ashes.

3. Of effecting the perfect cure of that bugbear of our winter existence—the smoke nuisance.

4. Of largely increasing the production of those valuable by-products, tar. coke, and ammonia, the annual value of which already exceeds by nearly 3,000,000*l.* that of the coal consumed in the gas-works.

The late exhibitions have been beneficial in arousing public interest in favor of smoke abatement, and it is satisfactory to find that many persons, without being compelled to do so, are now introducing perfectly smokeless arrangements for their domestic and kitchen fires.

The Society of Arts, which for more than 100 years has given its attention to important questions regarding public health, comfort, and instruction, would, in my opinion, be the proper body to examine thoroughly into the question of the supply and economical application of gas and electricity for the purposes of lighting, of power of production, and of heating. They would thus pave the way to such legislative reform as may be necessary to facilitate the introduction of a national system.

If I can be instrumental in engaging the interest of the Society in these important questions, especially that of smoke prevention, I shall vacate this chair next year with the pleasing consciousness that my term of office has not been devoid of a practical result.

THE REGULATION OF RIVERS AND WATERWAYS, WITH A VIEW TO THE PREVENTION OF FLOODS.

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Translated from the German for the Institution of Civil Engineers by WM. ATKINSON BELL.

There is such an infinite variety in the characteristic features of each waterway, as regards the configuration and geological structure of the country it permeates—whether its course lie in the interior of some great continent, and it empties itself into some other river, or into an arm of the sea, more or less affected by the ebb and flow of the tide—and also as to the extent and nature of the floods and their effect, that an exhaustive description of the regulating works applicable to each case would fill a folio volume. This memoir will therefore be limited to a brief general summary of the first principles requisite to the successful regulation of intractable rivers.

I.—Regulation of the Non-Tidal Portion of Rivers.

In every case, first of all the upper course of the river must be dealt with separately, and then the lower portion of it, together with its mouth, whether it empties itself into an estuary or into the open sea.

From long experience it has been ascertained that every river or stream, following its natural course through wide tracts of level country, invariably, if the banks consist of deposits of earth or gravel, attacks them, the lighter particles being carried away, the heavier being deposited in the bed of the stream, so that in course of time its width increases while its depth decreases, and at the same time islands, sand-banks, bends, creeks, and by-channels are formed.

In rivers thus left to nature the fall, mean velocity, and force of the current are continually decreasing while the river-bed is rising; this naturally raises the general water-level relatively to the adjoining country, and exposes it to frequent inundation, the effects of which are disastrous floods and the formation of innumerable branches and by-channels which intersect the whole country, flooding and swamping it at every rise

of the river, and rendering it in time unfit for habitation by either man or beast. Instances of the kind are at the present time to be met with in many parts of the world, notably in Asia, Africa, and America.

In order to deal effectually with such cases, namely, to abate the floods, and to prevent the disasters accompanying them, as well as the ultimate formation of trackless swamps, the following procedure is recommended:

(1.) A new channel following the course of the valley should be carefully laid down by the superintending engineer. either in a direct line or with easy bends, and when excavated, the entire body of water should be admitted into this new channel, the old bed and all by-channels being filled up.

(2.) Having carefully determined—

(*a*) The discharge per second at low, mean, and high water-level of a cross section of the river, either immediately above or immediately below the portion to be regulated, and

(*b*) The increased fall which the new channel will afford; then the sectional area of the new bed must be fixed, according to approved hydraulic formulas, so as to allow of the passage of either an ordinary or an extraordinary volume of water.

(3.) The water having been admitted, the next thing is to protect the banks by random rubble or by stone pitching in order to prevent the action of the current injuring them, or forming bends or creeks.

(4.) After the completion of the above, the old river-bed and by-channels should be filled up, the land thus reclaimed should by degrees be brought under cultivation; in the same manner the marshy tracts exposed hitherto to innudation, and fertilized by the deposit therefrom, should be raised by a coating of rich soil.

(5.) If exceptionally high floods still overflow the banks and inflict loss and damage to the freshly cultivated valley, dikes at suitable distances apart will be necessary to confine such floods, and enable them to flow off gradually without causing damage.

From forty-eight years' observation and experience of extensive works undertaken for the improvement of rivers, the author can confidently affirm that by careful attention to the points above recommended, even the most tortuous rivers and the swampiest valleys have, generally within a few years, but in some cases only after many years, yielded the most satisfactory results, as for instance—

(*a*) The increase of fall due to the more uniform section and more direct course, and the concentration and confinement of the stream within a single channel provided with firm banks, considerably increase the force and velocity of the current, which tend to deepen the channel, and to carry away the material thus scoured out as well as that brought down from above.

(*b*) By lowering the bed of the river, in some cases to the extent of from 3 to 6 feet, the general water-level, both of the river, and of the ground springs in the neighborhood, is proportionately lowered, so that the adjoining country is less liable to inundation, and the swamps are more easily drained and brought under cultivation.

(*c*) The velocity being accelerated in the new channel, as shown above (*a*), floods pass off more rapidly and do not rise so high, consequently the low country is seldom or ever under water, or at any rate not to the same extent as before. If, however, these lesser and lower floods are to be entirely prevented, dikes parallel to the course of the river must be added.

(*d*) In rivers exposed to the action of frost, floating ice is apt to accumulate in the unregulated portions of its course, especially at sharp bends, and on shallows and sand-banks, occasionally to such an extent as entirely to obstruct the flow of the stream, and to raise the water in the river to such a height that it overflows the banks, inundates the neighboring country, and spreads ruin far and wide.

When once a river has been regulated this cannot take place, as there would then be nothing to hinder the free passage of floating ice, and should a temporary stoppage occur, the concentrated force of the current would soon overcome every obstacle, by raising the blocks, and bearing them away without causing any flood.

(*e*) It is a matter of general experi-

ence that even in a deep river following a winding course and dividing into numerous branches, navigation is often obstructed to such an extent that the river becomes all but impassable, yet when the same river has been regulated, a direct channel is provided, facilitating traffic and commerce, and increasing the prosperity of the country already improved by drainage and cultivation.

(*f*) On the banks of many rivers left to Nature but a scanty population exists, invariably affected and often decimated by epidemics, and generally exhibiting an imperfect physical and mental development. After regulation, and by draining and cultivating the adjoining country, these evils disappear, the inhabitants improve in health, strength and intelligence, the population increases, new villages spring up, and prosperity reigns where before disease and poverty were rife. The Government earns the hearty thanks of all thus benefited, and is at the same time fully recouped the capital laid out on the works by the increased revenues derived from the improved condition of the country.

Where such an improvement of the waterway has been rationally executed, in accordance with the particular nature and requirements of the locality, most, if not all, of the above advantages have been secured; as may be proved by numerous instances of works of the kind executed years back in France, Germany, Austria, Switzerland, and Italy. Moreover, the fact that the Chambers of Deputies of these States have, during the last few years, repeatedly devoted hundreds of millions of florins to the completion of works already begun, and to new undertakings of the same kind, is a proof that the importance and advantage of such improvements are fully recognized.

As a complete description of even the most important works of this kind would far exceed the limits of a short paper, the author must confine himself to a brief review of those successfully accomplished on the Rhine and the Danube.

The Rhine, between Basle and Mannheim, has for centuries followed a tortuous course, abounding in sharp bends and dividing into many branches, through a valley between 5,000 and 6,000 meters broad. Having further repeatedly shifted its course, the whole valley became cut up by old channels; to a considerable extent, too, its natural fall was lost, owing to its sinuous course, and consequently the rate and force of the currents were so much diminished, that deposit accumulated everywhere, raising the bed of the river and mean water-level to such a degree that the adjoining country was little better than a swamp. The bed of the Rhine being thus elevated, and its course so irregular, the flood-water could not flow off rapidly enough, but spread abroad, inundating the neighborhood, and destroying whole villages and townships.

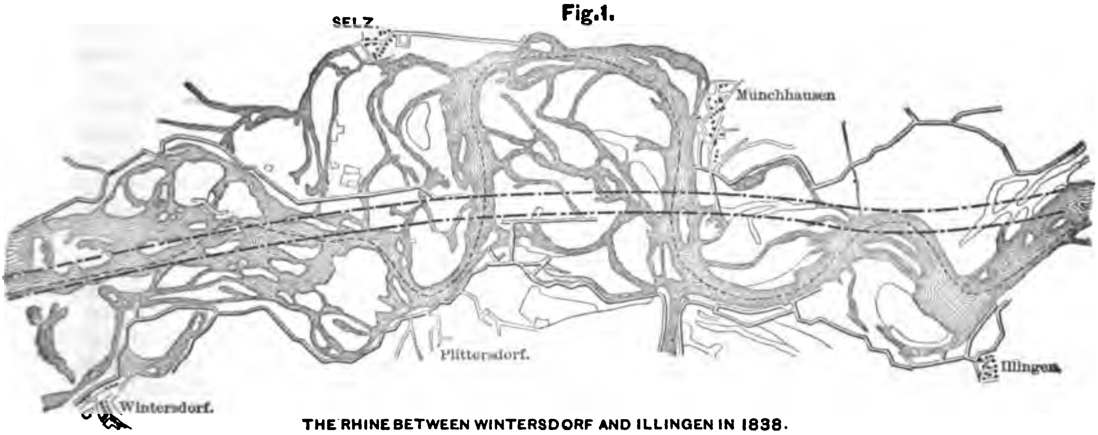
The riverside communities had in all ages attempted, by dams and other protective works, to abate these evils, but with little success, as, owing to the winding course of the river, the floods confined at one point escaped at another, and took their defences in reverse. This deplorable condition of the Rhine valley continued until the commencement of the present century, when the population, already greatly reduced by poverty and disease, was daily decreasing owing to emigration to America.

Colonel Tulla, of the Engineers, an eminent authority on hydraulics at that time, by repeated and unremitting exertions, induced the Government in 1817 to undertake a thorough survey of the entire Rhine valley. Upon that survey was based the project for the radical regulation of the Rhine bed, which was approved and ratified by treaty between the border States of France, Bavaria, and the Grand Duchies of Baden and Hesse, and according to which the regulation of the Rhine was carried out during the years 1819 to 1863.

The work consisted in regulating the course of the river and making it more direct. This necessitated the excavation of twenty-three considerable cuts, which reduced the distance by river between Mannheim and Basle from 252 to 169 kilometers, and increased the fall 30 per cent. Further, the stream was confined to a uniform channel of suitable section, both banks were substantially protected, the old river-bed and all branches were filled in, and the land thus reclaimed was for the most part brought under cultivation.

The above-mentioned regulation of the Rhine may be considered one of the most extensive and interesting undertakings of the kind ever attempted in Europe, the scope of which can scarcely be fully appreciated without reference to the exhaustive description and numerous plans issued by the Public Works Department of the Grand Duchy of Baden at Carlsruhe. By these it is proved that the following advantages have been secured, viz.:

Fig. 1.



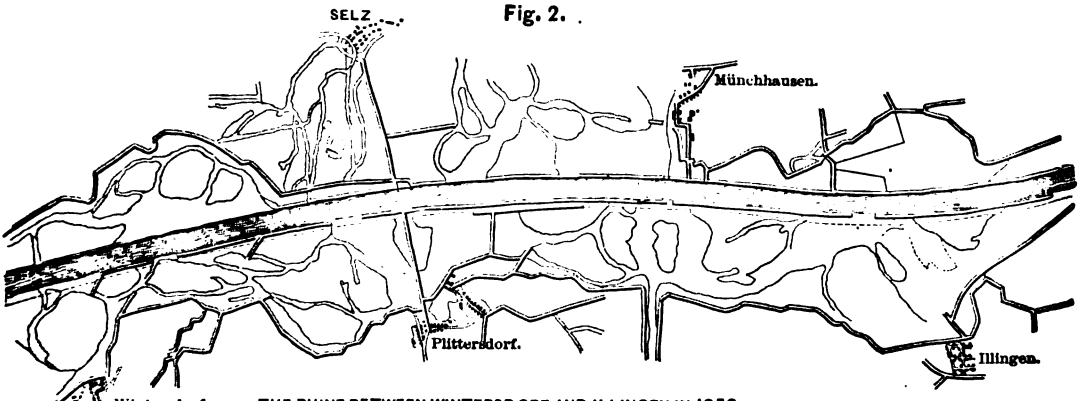
THE RHINE BETWEEN WINTERSDORF AND ILLINGEN IN 1838.

(*a*) The river has undeviatingly followed the new course provided for it, has deepened its bed to the extent of two meters in some places, and lowered the mean water-level proportionately. Flood-water also has been passed more quickly.

(*b*) The general water-level being thus reduced in height, extensive tracts of swampy ground have been laid dry and converted into fertile arable land. Further, more than 20,000 hectares of old river bed water-holes and sand-banks have been reclaimed, and brought in a great measure under cultivation; and, lastly, the low-lying tracts are now no longer exposed to inundations.

(*c*) The sanitary condition of the Rhine valley has visibly improved, and the general prosperity of the inhabitants materially increased.

Fig. 2.



THE RHINE BETWEEN WINTERSDORF AND ILLINGEN IN 1852.

(*d*) According to the concurrent reports of experts, Government officials, and local authorities, the benefits derived from the regulation of the Rhine are so considerable that the capital laid out has been amply repaid. Wherefore in grateful recognition of the emin-

ent services rendered by Colonel Tulla, the original promoter of the scheme, a statue has been erected in his honor at Maxau on the banks of the river.

In one respect only has the regulation of the river not fulfilled the expectations of its promoters, viz., the extensive sandbanks formed at the confluence of its tributaries have rendered intercommunication with them both difficult and dangerous, because these feeders to the main stream enter it across bars little more than 0.60 or 1.50 meter below low-water level.

Had the hydraulic engineers in 1817 correctly determined the minium discharge of the Rhine, and at the same time anticipated a probable decrease of the same, they would have diminished the waterway, and thereby considerably reduced the deposit and formation of such sandbanks, so that the channels of communication between the Rhine and its tributaries would have remained more open to navigation.

As a second example of the satisfactory results of the regulation of an important river, the works executed on the Danube, near Vienna, during the years 1869-1881, may be cited. These were described in a paper communicated to the Austrian Society of Engineers and Architects, and published in their journal for 1880.

The course of the Danube at Vienna was by no means so irregular as that of the Rhine. Owing, however, to three sharp bends in the immediate vicinity of the city, and to the width of the river and numerous shoals, vast masses of ice accumulated, almost every severe winter, causing the suburbs to be flooded, and leading to great damage. Moreover, the navigation was much impeded by shoals and shallows, and was exceedingly dangerous between the narrow bays of the wooden bridges. It was impossible to build masonry bridges, and there were no commodious landing-places, as the only place adapted to the purpose was the long sweep of the river at Florisdorff, some distance from Vienna and on the opposite side.

This state of matters was very injurious both to the health and to the commercial prosperity of the city, yet it was not until many years had been consumed in negotiations between the Government, the Landtag, and the Vienna Municipality, that at length a well-considered project for regulating 23 kilometers of the course of the river, from Nussdorf to Fishamend, was sanctioned and commenced with a capital of 32 million florins. The results of this work, already apparent, are as follows:

1. Although the cut (6,440 meters in length) between Nussdorf and Fishamend was only opened in 1876, and the other works were still incomplete, yet the extraordinary quantity of ice that came down the river during the severe winters of 1876 and 1880, as well as the high floods in August, 1880, passed Vienna without causing the slightest damage. It may, therefore, be taken for certain that these dangers to the city have been effectually removed. It is beyond doubt that had the regulation works not been constructed, the huge banks of ice piled up on both sides of the river a short distance below Vienna, would have spread over the low-lying suburbs, as was the case in 1830, and have caused incalculable damage and loss of life and property.

2. Since the low-lying suburbs have been protected against the floods and ice, and the general water levels of the country and of the river have been lowered about 0.50 meter, the epidemics formerly so prevalent have all but disappeared, the sanitary condition of the population has improved, and the death rate has been considerably lowered.

3. The river is now 1,890 meters nearer Vienna, and in order to extend the city in that direction a site for a new suburb has been supplied, by filling up the old river-bed, &c., equal to 267 hectares, available for building purposes, after deducting the space to be occupied by projected streets, squares, and gardens.

By diverting the river it has also been possible to remove the old pile-bridges and to replace them by five handsome stone structures, two being for ordinary and three for railway traffic, the details of which are given in the report issued on the occasion of the public opening of the new channel for navigation on the 30th May, 1876.

The course of the river at present affords a navigable channel of a uniform section and easy bends, with a depth of 3 meters at the lowest water level, so that the navigation is never interrupted.

Three extensive sandbanks have been

formed on each side of the river, but offer no obstacle to navigation, as they are everywhere at least 0.60 to 1 meter below the lowest water level, and they do not extend across the river; this is attributable to the fact that, profiting by experience gained on the Rhine, the width originally determined on, viz., 1,000 feet, was reduced to 900 feet.

5. Along the right bank of the new channel, besides public landing stages, ten quays 1,067 meters long have been constructed, and 13,276 meters of frontage secured, on which numerous warehouses have been built. These have been put into direct communication with all the railway stations in Vienna by the new line along the banks of the Danube, so that all goods arriving at or leaving Vienna may be transferred from the shipping to the railway, or *vice versa*, with the greatest facility, expedition, and economy. A roomy winter harbor is also provided for unloading the river craft during floods and during the breaking up of the ice.

In consequence of the above improvements the traffic and commerce on the Danube has quadrupled since the river has been regulated. It is also evident that all the results anticipated have been fully realized, and so many substantial benefits, both to the city and to the country, generally secured, that the immense capital of 32 million florins has been well expended and richly repaid. Mr. James Abernethy, Past-President Inst. C. E., greatly contributed to its success by strongly recommending the plan since executed, and assisting in the elaboration of the entire scheme, when summoned to Vienna in 1867.

The manifest advantages secured by the regulation of this portion of the Danube have induced the Government, Landtag, and Municipality to authorize forthwith the regulation of further portions of the river, namely, from the confluence of the Isper to Nussdorf, and from Fishamend to Theben, on the Hungarian frontier, a length of 157 kilometers, at an estimated cost of 24 million florins.

The next section, from Pressburg to Gönyö, 80 kilometers in length, is quite as irregular as the course of the Rhine between Basle and Mannheim was before 1817. Yet the Hungarians object to its regulation because they are jealous of any increase of traffic between the lower Danube and Vienna, and are anxious to attract it to Budapest. But lately the Hungarian Government has discovered, that the devastation caused to the adjoining country, by floods in this unregulated portion of the river, is on the increase, and that the present difficult navigation has not so much affected Vienna as their own export trade to the westward. It has therefore determined to proceed with the regulation of that portion of the Danube between Pressburg and Gönyö within their jurisdiction, at an estimated cost of about 20 million florins.

Advantage may be taken of this opportunity to refer briefly to the circumstances which prevented the regulation of the Theiss in Hungary being productive of any permanent improvement, and in fact rendered it a complete failure. It is beyond a doubt that the depressed Hungarian basin was at one time occupied by a vast inland sea, which gradually drained away after the Danube had forced a passage through the mountains between Moldova and Orsova.

The river Theiss receives the entire drainage from the Carpathian mountains, and after a most tortuous course through the level plains of the above basin, delivers it into the Danube. Throughout its whole course below Szolnok it falls at the rate of only 1 in 46,000, and latterly of 1 in 60,000; whereas in its upper course, between Tokay and Csáp, the fall is at the rate of 1 in 19,000 to 1 in 5,200. The wealthy landowners on this upper portion of the river have protected their own property from the effects of floods by providing a direct course for the Theiss, and confining it to that course by a continuous system of dikes. The natural fall has therefore been increased, and consequently it now bursts with redoubled force into the lower reaches, where the fall is very slight. Besides, in the lower portion of the river, little has been done to provide a more direct channel, and the dikes thrown up on either side have been constructed in such a haphazard manner that in some places they are actually at right angles to the course of the river, besides being at very unequal distances apart, ranging from 400 to 1,400 meters.

Lastly, no increase to the cross-section

of the channel has been attempted, though repeatedly recommended by the most eminent hydraulic engineers; nothing has been done to improve the course of the river Maros, which flows into the Theiss at right angles to the latter, nor has its junction with the Danube been regulated. It is but natural that the works undertaken on the Theiss, and executed in direct opposition to the first principles of hydraulics, should have proved most unsatisfactory; that floods should have been intensified, the dikes breached, and wide fertile tracts inundated and laid waste—a calamity that, on the 12th March, 1879, befel the populous commercial city of Szegedin.

These unsatisfactory results should in no way shake confidence in the undeniable and proved success of many rationally executed works of the kind in Europe and elsewhere, but rather serve as an example and a warning that improvements conducted regardless of true scientific principles can only end in that way.

The depth of water in almost every river having sensibly decreased within the last ten years, to the serious obstruction of navigation, whereas at some seasons the floods have risen even higher than before, some German engineers have given it as their opinion that this is owing to the regulation of rivers, as they affirm that the ordinary flow passes off more rapidly when confined to a channel of a regular section and direct course; for the same reason the floods also descend more quickly from the high ground and rise to a greater height. Arguing from the above hypothesis, these engineers maintain that the execution of regulating works is disadvantageous, and propose instead the construction of capacious reservoirs to impound the surplus rainfall, so as to prevent floods and at the same time to regulate and maintain an equable flow of water at all seasons.

The author cannot assent to this theory. In all the papers published by him from 1873 until 1879, he has clearly shown that during the last ten years the supply of water derived from natural springs has perceptibly decreased, and consequently the level in rivers, &c., has been lowered. This would have been still further the case had the rivers remained unregulated, because the same quantity of water spread over a wide irregular bed, and distributed in many branches, would expose a far larger surface to the atmosphere, and consequently more water would be lost by evaporation.

Floods at the present day occur in the regulated channels, rising even higher than formerly, in spite of the rapid overflow, and for this reason, that in consequence of the clearance of forests, especially on mountain tracts, rain-storms are more frequent; further, because the reduced total rainfall penetrates nowadays less into the ground, and flows off from the treeless slopes of the mountains and country, now intersected by numerous ditches and drains, into the rivers much more rapidly and in greater quantities, which accounts for these heavy floods.

The extraordinary advantages of constructing reservoirs in the drainage area of rivers was recognized by the Chinese four thousand three hundred years ago, and even at that time many reservoirs several square miles in extent were constructed in connection with the regulation of their large rivers.

In the present day, when every square yard of ground, even in mountainous districts, is private property, and obtainable only at almost prohibitive prices, the construction of huge reservoirs in the drainage area of rivers would be both difficult and expensive. Besides, such reservoirs could be of little or no use in connection with an unregulated river, consequently its regulation would in the end be obligatory.

Latterly, numerous ideas and projects for regulating and making navigable natural river courses, and at the same time preventing floods, have been propounded. But regarding them as impracticable, and as unlikely to lead to any favorable results, it is needless to enter into a discussion of their merits.

II.—Regulation of Rivers emptying into the Sea, or into an Arm of the Sea.

The irregular course of rivers flowing into the sea is due principally to the facts, that in the lower part of their course the inclination of the bed is inconsiderable, and that frequently bars are formed at the mouth by the joint action of the river and the sea. The bars also prevent the egress of the land water, driving it back and flooding the

adjoining country, until the ebb of the tide permits the river to flow again and find its way into the sea, by the numerous channels it has forced through the delta at its mouth.

The regulation of rivers flowing into the sea is a much more difficult and a more costly undertaking than that hitherto treated of; for the volume of water to be dealt with is greater, and the loose and yielding nature of the deposit forming the banks and bed of the river, as well as the violence of the storms and force of the waves, render it necessary that the protection of the banks and all structures should have solid foundations and be executed in the most substantial manner.

The ordinary works necessary to the perfect regulation of such a river consist of—

(1) Rendering the course as nearly straight as possible in order to increase the fall.

(2) Enclosing the river near its outfall by means of dikes, and continuing the same beyond the bar and as far out to sea as possible, that the force of the current may be sufficient to carry the sand and mud to such a depth that the action of the sea may carry it away and prevent the formation of a bar at the mouth.

If the adjoining country is to be effectually protected from inundation produced by the action of the tide, it will be necessary on either bank, beyond the limit reached by the tide, to construct dikes at such distances and in such a manner that at ebb tide the force of the river may be sufficient to carry out to sea the silt, &c., deposited during the flow of the tide; when the river is thus confined, then the low country beyond the dikes may be drained and brought under cultivation.

It is not proposed to discuss the works executed for regulating rivers falling into the sea. It may suffice to mention the celebrated works at the Sulina mouth of the Danube, those at present in course of completion at the mouth of the Mississippi; and, lastly the successful drainage and cultivation of the deadly swamps of Tuscany and the lowlands of Holland, lying 5 feet below mean sea-level.

The vast experience gained in Europe in works of this description justifies the opinion that the science of hydraulics has, thanks to the observations carefully recorded for many years, arrived at such perfection that every practical hydraulic engineer is in a position to project and execute works calculated to rectify a bad waterway, to drain a swampy tract, and to prevent the recurrence of floods and their dreadful consequences.

Lastly, a few remarks may be permitted upon the papers by Mr. Wheeler and Mr. Jacob on the Conservancy of Rivers, dealing respectfully with the Eastern Midland District of England, and with the Valley of the Irwell. From these communications it would appear that the irregularities in the course of the four rivers draining the Eastern Midland counties, and of the Irwell, were not due so much to the configuration of the country or to natural causes, but

(1) Chiefly to the independent way in which, on their own section, each proprietor carried out the improvements he judged best, without any regard to the very first principles of hydraulics, and utterly regardless of their effect on the course of the river above or below him.

(2) To the fact that the weirs, sluices, and other works, were built too high and too narrow, thus facilitating the formation of sandbanks and the silting up of the river-bed, and so hemming in the ordinary freshets as to force the water over the banks and flood the adjoining country.

(3) The scouring and deepening the bed of the river by the action of the tide was rendered impossible by the numerous weirs, sluices, and other obstructions connected with the supply of water of the mills, as well as by the bad management of the same, so that not unfrequently the floods were permitted to lie on the land for weeks together, rendering it little better than a swamp.

According to Mr. Jacob, all the slag, refuse, and even the earth excavated for the foundations of smelting furnaces, factories, and dwelling-houses, in the immediate vicinity of the Irwell, were shot into the river or on to its banks, to be swept away by floods into its bed; so that it is scarcely to be wondered at, that rivers so treated should at length become so obstructed as to cause frequent and destructive floods.

The evils thus produced by ill-designed

structures and by the bad management of those in charge of them were further augmented by two circumstances:

(1) That the bar at the mouth of the river raised the ordinary water-level, and dammed up and forced back the flood waters, so that the country around was continually exposed to inundation.

(2) In consequence of improved cultivation and better drainage the rainfall in the upper basin of the river flowed off in greater quantities and more rapidly than formerly, while the section of waterway, and the weirs and sluices on it had not been proportionately increased; this raised the height of the floods and increased the area subject to inundation.

From the minute particulars given by Messrs. Wheeler and Jacob, the author feels himself in a position to express a general agreement with the suggestions made by them for regulating the rivers and remedying the evils complained of, because they evidently coincide with the principles applied with success to many works of a similar kind in Europe and elsewhere. On the strength of long practical experience in this branch of the profession, he would beg to be allowed to make the following remarks:

(*a*) In the first place, the discharge per second at important points on each of these five rivers during high floods should be carefully ascertained, so that a suitable cross-section for the channel and for the sluices might be fixed.

(*b*) The masonry weirs across the bed of the rivers should undoubtedly be removed, as at present they tend to augment the floods and to facilitate the slitting up of the bed, and if allowed to remain, would render a complete regulation of the river either impossible or nearly useless.

(*c*) The sills and floors of all existing sluices and locks, as well as of those to be constructed in place of the present weirs across the bed of the river, should be laid several feet below the present river-bed, in order that, after the regulation, the anticipated lowering of it may meet with no obstacles.

If a thorough regulation and removal of the present ill-designed regulating works be carried out, it is certain that the serious floods complained of will cease, the inundation of the adjoining land disappear, the greater portion of these rivers be rendered navigable, agriculture flourish, and the health and well-being of the population improve, the mortality decrease, and the capital expended thereon give an abundant return.

From the concluding remarks of the above gentlemen the author gathers that none of the improvements advocated by them can be put in hand, until Parliament has passed an Act for the better regulation and control of all works erected by private enterprise, and for a due inspection of the same. The institution of Civil Engineers might, therefore, render immense service to the inhabitants of many river-basins were it to exert its influence in promoting such an Act of Parliament, and aiding to the best of its ability river improvements of this description.

STEEL MARINE BOILERS.—The progress which has been made during the past few years in the employment of steel for marine boiler construction is strikingly exemplified by some interesting data with which we have been favored by the Wallsend Slipway and Engineering Company of Newcastle-on-Tyne. In the three years —1878, 1879, 1880—that firm constructed seventy-five marine boilers of the aggregate weight of 1808 tons and of these sixteen, or say 21.33 per cent., were of steel. In 1881 they constructed forty boilers, weighing collectively 1089 tons, and of twenty five, or say 62½ per cent., were of steel, while during the current year the number of boilers made by the firm has been fifty-six, of the gross weight of 1360 tons, and of these no less than forty-seven, or 84 per cent. of the whole, were of steel. It will thus be seen that as far as the Wallsend Slipway and Engineering Company is concerned the relative positions of steel and iron, as materials for boiler construction, have been entirely reversed during the past two years. It will be remembered that in April, 1878, Mr. William Boyd, of the above-named firm, read before the Institution of Mechanical Engineers a valuable paper on "Experiments Relative to Steel Boilers," in which the value of steel, as a material for boilers, was most thoroughly discussed.

INFLUENCE OF THE EARTH'S ROTATION ON DERAILMENTS.

By RD. RANDOLPH, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

It has often been stated in scientific publications that upon railways lying north and south there was a tendency on the part of the cars to quit the track and to pass to the eastern side when moving northward, and to the western side when moving southward. It is also stated that this tendency is so strong that a large majority of the derailments on tracks so located have been towards the side, indicated by this theory. It may be interesting to analyze this influence of the earth's rotation upon bodies moving upon its surface, and to reduce the force at different latitudes to its equivalent in pounds. Such an examination will show that the pretended observations of derailments could not have been made, and that the force in question is too slight to have any appreciable effect upon them.

When a locomotive runs upon a track in the form of a circular curve, it is constantly and uniformly forced from the direction of its last impulse by the projecting rim of the wheel coming in contact with the rail, each part of which is constantly deflecting from the direction of its adjoining part in the same horizontal plane; the parts being considered infinitely short in theory and indefinitely short in practice. There is thus a constant tendency to depart from the curved line and to follow the direction of the last infinitely short piece of track; which is that of the tangent at the point of departure. But the curvature of the rail, in order to constantly deflect the moving body from its former course, is only necessary when it is fixed to a stationary plane. If, however, the horizontal plane is itself rotating about a perpendicular axis the rail fixed to the plain may be straight and yet have exactly the same effect upon the moving body as if the plane were stationary and the rail curved; it being only necessary that the moving body be forced to deflect from its former course at every point at the same rate on either the curved or straight line. In one case only that part of the line which forces the moving body makes the required deflection. In the other the whole line makes the required deflection. which does not affect the lateral force communicated to the body.

If a straight line in a plane deflects about a fixed point at one end until it returns to its first position, it will have described an angle of 360 degrees, and every small part of that line will have also deflected the same amount about its own terminal point. The same radius of the moon is always pointed towards the earth and may be considered a part of the radius which describes the moon's orbit about the earth; yet the moon makes one revolution about its own axis in the same time it makes one rotation in its orbit. An observer outside of the orbit would see all the points of its surface in succession. And if there were a succession of moons located along the radius of the orbit, their radii constituting parts of this radius and moving in the same periods, each lunar radius would make the same deflection as the orbital radius in the same time. Therefore if a body should move along such a deflecting line, it would constantly meet a portion of the line having a different direction from that of the motion which had just been imparted to the body, just as if it were running over a curve deflecting the same amount in the same time and distance.

The amount of deflection of a line about a fixed point is measured by the sum of the infinitely small angles described by the deflecting line; or, when the length of the line is constant, by the sum of the infinitely small arcs described by the end of the line; and in general, by the sum of these arcs divided by the length of the line. If the deflecting line describes a plane, when it returns to its first position, it has described 360 degrees. If the line of the same

length describes a cone, the number of degrees deflected will have the same proportion to 360 as the circumference of its base has to the circumference of the circular plane. If the line describing the cone is of a different length from that describing the plane, the amount of deflection in each case will be in proportion to the circumferences divided by their respective describing lines. When a cylinder is described the divisor becomes infinite, and consequently the amount of deflection reduces to zero.

Now a tangent to the earth's circumference intersecting its axis will, at the pole, describe a plane; approaching the equator, it will describe a series of cones increasing in acuteness until at the equator, where such tangent becomes infinite, the cylinder and the zero of deflection is reached. The cotangent of the degree of latitude of a point on the earth's surface is this describing line, and the cosine of the latitude is the radius of the base of the cone. The circumference of the base of the cone described by the cotangent at any point of latitude is cos. $\times 2\pi$. If the same cotangent had described a plane instead of a cone the circumference of the circle would be cot.$\times 2\pi$. Therefore if cot.$\times 2\pi$ subtends 360 degrees, cos.$\times 2\pi$ will subtend

$$\frac{\cos.\times 2\pi\times 360^\circ}{\cot.\times 2\pi}=360^\circ\times\frac{\cos.}{\cot.}$$

which is the amount of deflection of a tangent to the earth's surface in twenty-four hours, or one rotation.

If D is the distance on a straight track over which the velocity of the locomotive would take it in twenty-four hours, and this distance should be laid out on an immovable plain in the form of a circular arc containing the same amount of deflection which a tangent to the sphere at a certain degree of latitude would perform by the earth's rotation in twenty-four hours, it will subtend an angle of $360^\circ\times\frac{\cos.}{\cot.}$; and this arc will have the same proportion to the whole circle as cos. is to cot. Let R be the radius of the arc D; then the circle will be R$\times 2\pi$; which having the same proportion as cos. to cot.

$$\frac{D\times\cot.}{\cos.}=R\times 2\pi,\ \text{or}\ \frac{D\times\cot.}{\cos.\times 2\pi}=R.$$

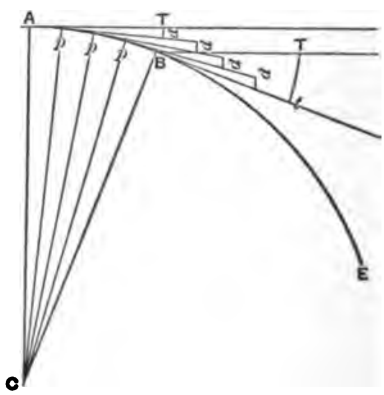
If D is divided by 86400 it will be the distance travelled in one second. Let this be denoted by V, which may be substituted in the equation for D, if 2π is also divided by 86400 thus,

$$\frac{V\times\cot.}{\cos.\times\frac{2\pi}{86400}}=R,\ \text{or}\ \frac{V\times\cot.\times 86400}{\cos.\times 6.2832}=R,$$

$$\text{or}\qquad 13750.955V.\times\frac{\cot.}{\cos.}=R.$$

So that the tendency of a locomotive, running with the velocity of V on a straight line on the earth's surface at any point of latitude, is the same as that of one running at the same velocity upon a stationary curve whose radius is 13750.955V.$\times\frac{\cot.}{\cos.}$. To reduce this force to its equivalent in gravitation or pounds, it is only necessary to determine the centrifugal force on such a curve.

To establish a formula for the centrifugal force of a body moving in a circle let AT represent the tangent in which the body is moving when the resistance is applied at A to deflect it into the curve ABE, and let AB=AT be the distance in the curve moved in one second.



If the resistance were suddenly to cease at any point of the curve the deviation from the last tangent would continue with uniformity; but as the resistance is applied at every point *ppp* along the curve AB, and, the motion being uniform, at equal periods of time just as the impulse of gravitation is applied at equal periods of time along the path of a descending body, the deviation from the first tangent is constantly acceler-

ated. At each point an impulse is imparted at right angles with the last motion causing the increments of deviation *dddd*, whose sum at the end of the distance moved in a second is equal to Tt, the arc of the angle formed by the first and last tangent having BT, equal and parallel to AT, for radius. This arc is the deviation per second produced by the sum of the impulses during one second and which would remain uniform if they were to cease. If the force of gravitation would cause the same body to move towards the earth's center at a certain rate after it had been applied for this same length of time, with no other resistance than that of inertia, the impelling force, or weight, would be in the same proportion to this centrifugal force as the rate of falling is to the rate of departure from the first tangent at the end of the same period of time. As the velocity of a falling body at the end of the second is 32.22 feet per second the weight of the revolving body will be in the same proportion to the centrifugal force as 32.22 is to the arc Tt; which may be expressed by the equation $\frac{32.22C}{W}=Tt$, in which C is the centrifugal force and W the weight of the moving body. The angle formed by the first and last tangent is equal to that formed by the two radii at the same points: therefore, the radius AC has the same proportion to the arc AB as the tangent $AT=BT$ has to the arc Tt. And since the arc AB is equal to the tangent Bt we have the proportion $AC : AB :: Bt=AB : Tt$ or $AC\times Tt=AB^2$. AB being the distance moved in one second is the velocity, to be denoted by V: and AC being the radius of the circle is denoted by R. Then $R\times Tt=V^2$ or $Tt=\frac{V^2}{R}$. In the equation $\frac{32.22C}{W}=Tt$, substitute for Tt its value $\frac{V^2}{R}$; then $\frac{32.22c}{W}=\frac{V^2}{R}$ or $C=\frac{V^2\times W}{32.22R}$. Now let the radius which has been determined as that of a stationary curve causing the same lateral resistance as that of the straight line on the rotating earth be substituted for R in this equation. It will then be

$$c=\frac{W\times V^2}{32.22\times 13750.955V\times\frac{\text{cot.}}{\text{cos.}}}$$

$$\text{or } \frac{W\times V}{443055.75}\times\frac{\text{cos.}}{\text{cot.}}=C.$$

which is the force with which a locomotive tends to leave the straight track at any degree of latitude.

Let V, the velocity of the locomotive, be assumed at 60 feet per second=41 miles per hour, and W, the weight at 80,000 lbs.

In latitude $20°\frac{80000}{60\times 356.605}=3.74$ lbs.= lateral impulse.

In latitude $40°\frac{80000}{60\times 190.340}=7.00$ lbs. = lateral impulse.

In latitude $60°\frac{80000}{60\times 141.165}=8.04$ lbs. = lateral impulse.

In latitude $90°\frac{80000}{60\times 123.070}=10.83$ lbs.= lateral impulse.

As every part of a line deflecting about a fixed point is itself deflecting about any of its own moving points at the same rate and in the same direction, the ends towards the fixed point deflecting in a contrary direction from the motion while the opposite ends deflect in the same direction, it follows that all the deflections north of the equator must be towards the left and all those south of the equator towards the right; which would cause the locomotive to tend to the right in northern latitudes and to the left in southern latitudes.

So far the track has been supposed to be located upon a north and south line, according to the pretended observations. But it is apparent that the effect is the same upon any horizontal line at the same latitude, for all such lines that can be drawn upon the surface of the sphere radiating from a common point must all deflect at the same rate like the spokes of a wheel, confined to the same horizontal plane. So that a locomotive upon a track located east and west will be subject to the same lateral impulse as that on the one north and south, the only difference being that of quantity according to latitude and direction according to position with regard

to the equator. The motion that the lateral impulse was confined to a north and south track, arises from the fact that it is easy to perceive a difference in rate of motion on two circles of different latitude, and that a body endowed with the velocity of one being transferred to the other would encounter the resistance arising from the difference of velocities. But this variation of the circles of the same conical zone corresponds exactly with variation in lateral velocity of equally distant points on the deflecting tangent which describes the cone, or any other lines belonging to the same small horizontal material plane which follows the describing tangent, and which rotates to the same degree which the tangent deflects. The rotation of the small horizontal planes which compose the surface of the sphere is the sole cause of the lateral impulse to moving bodies upon it, and the difference of the conical circles is the measure of the deflection of all these lines alike. Therefore as the observations of derailments have been credited solely to north and south tracks, this proves them to be purely imaginary. And as in every case of derailments the cause must be one involving a vastly greater force than this lateral impulse, it could have no appreciable effect in determining the direction.

SOME PRACTICAL NOTES ON THE SEASONING OF BUILDING WOODS.

From "The Builder."

There have, perhaps, been as many papers written relative to the seasoning of wood as have been contributed to the discussion of any other purely technical question.

The subject is one to which theorists in particular have devoted a large share of their attention. The matter, indeed, has, for the greater part, been theoretically rather than practically discussed, and whilst an extreme degree of attention has been directed to issues of comparative unimportance, others of material importance have been overlooked. It is hoped by this paper to supply some of the deficiencies which exist.

Commencing upon our inquiries, it is perhaps as well, in the first instance, to inquire what the seasoning of wood really amounts to. It appears to be a prevalent notion that some extraordinary chemical effect upon the juices and saps of wood is brought about by the process of seasoning. Such theories as are, however, held by some scientists possess very little interest to those who make use of wood only as a constructive material.

For practical purposes a given effect is required. Wood is required to be seasoned, and what is mainly wanted to be known upon the subject is, what is the best and most economical method of obtaining the desired result? By what is commonly known as seasoning wood is meant nothing more than drying wood.

Wood is to be dried only by the moisture being evaporated from it. It is an elastic material, and in degrees as the moisture exudes so it will shrink. It is also by nature somewhat absorbent, and it will therefore, under certain conditions, attract and retain moisture. For instance, if highly dried wood-work be fixed in a thoroughly damp house, the wood will absorb some moisture, and being of an elastic nature, it will re-expand just in proportion to the amount of moisture it has absorbed. When the house has become dry, the wood will also have dried, and the result of the expansion and afterwards of the contraction will be a number of open joints. For these open joints the joiner may receive much censure, although it is highly possible that he may not have been at all in fault. Indeed, if the wood he has used has been highly dried it will, in all probability, have absorbed more moisture than would have been the case if it had only been partially dried, and so when it shrinks again the openings left will in such a case be wider.

Woodwork to be fixed in a newly-

erected and, consequently, damp building, should have received a coating of paint before being fixed, as, in such a case, it will not absorb moisture.

These remarks may suggest the inquiry why unpainted woodwork does not continually undergo a process of expansion and contraction. The question is not a difficult one to reply to, although it has not usually been answered in our way.

It is well enough known that wood, in a living state, forms a skin over any exposed portion. Thus, if a branch be lopped off a tree, the injured limb for a time will bleed its sap. Nature, however, supplies its own protection against injury, and a thin skin is soon formed over the injured part, which stays the bleeding. It is the same, to some extent, with dead as with living wood. Flooring-boards, for example, which have been laid down for ten or almost any other number of years, will, if planed over, enter upon a second process of shrinking or expansion, according as they are situated in a dry or humid atmosphere. To season wood, then, is simply to dry it, and that means nothing more than evaporating the moisture from it. For all practical purposes, therefore, we have only to inquire,

First, what amount of seasoning is required?

Secondly, what is the best way to accomplish the seasoning process?

The issues are, consequently, narrowed very considerably.

When we ask ourselves what amount of seasoning is necessary, we must hold steadily in view the requirements of the different purposes for which wood is used.

And, first of all, it should be clearly explained that foreign-sawn building woods undergo a limited amount of seasoning before they leave the other side of the water. It would not at all do to store green wood into the unventilated holds of ships, for if such a thing were done, the wood would at once commence to "sweat." In a short time it would become covered with a mildew, and the sap-wood would become permanently discolored; it would then have depreciated very much in commercial value. To avoid such an occurrence the wood is for a time seasoned abroad under large wooden sheds before being shipped. It is in consequence of being seasoned under sheds that it usually reaches us of such an excellent color and clean appearance.

The bulk of the foreign building wood imported is used for bearing or joisting purposes. For such purposes it is not necessary that it should be at all seasoned beyond the preliminary seasoning it undergoes previously to shipment. It is a common error to suppose that old wood is in any way better for bearing purposes. A kind of vague idea has generally existed that old wood is necessarily seasoned, and that it is better that it should be seasoned.

In a vague way this view is held to, and yet no one can say in what way *bearing wood* is benefited by being seasoned. As a matter of fact, it is usually more or less *injured* by being old.

At the timber-yards, deals are, as a rule, most indifferently piled. They are not generally in any way protected from the weather, nor are they stored in such a manner as will insure any water which falls upon them running quickly off again. If not piled so that air can circulate through them, and if moisture be present, decay will rapidly supervene.

If the sun's rays be allowed to approach the ends and the sides of deals, a splitting process occurs. Should wet weather then follow, these cracks will become filled with moisture. When frost follows upon wet the splitting process continues, and the deals often become in consequence much shaken. Old deals will have endured these contingencies, and it may be taken for granted that they will not have escaped scatheless. Consequently it is better on the whole to employ perfectly new wood for all kinds of bearing work.

Wood, however, that is intended to be made up into joinery work must be seasoned. There are two matters to be considered in relation thereto. First, the wood has to be dried. Sec ondly, it is not to be injured in the process of drying. The injuries that can be done to the material are those to be caused by the sun, rain and frost, and by the dirt if it be stored in the immediate neighborhood of a large town. The only sufficient protection against these influences is a suitable shed for the wood to be placed under. The sides and the ends of the shed must, of course, be open to admit of the wind circulating freely

through the deals or boards required to be dried. Thus all that is required is a suitable roof. Such a roof should be exceedingly flat, so as to admit of the convenient piling away of the deals or boards under it. It should be made to well overhang so as to serve in the double capacity of a sunshade and an umbrella to keep off the sun and the rain. It should also be made capable of being raised or lowered at will, so that it can be placed just over any pile. It should be built in the most exposed part of the builder's premises, for no factor is so important in relation to the seasoning of wood as is the wind. If it blows a gale, so long as it does not upset the piles, it is all the better for the purpose of seasoning.

Deals and boards, when placed for seasoning, should alike be separated at regular intervals by evenly sawn strips of wood. It is better that boards should be seasoned when lying in a horizontal position, because they are then less likely to twist or warp whilst being dried. The long and the short pieces should be piled separately, and in no case should a long deal or board be allowed to overhang without any support, or it will be certain to twist. It is worth while taking care in this respect of those intended to be used for joinery purposes, that are required to be seasoned at all.

Flooring-boards will probably have to be seasoned in the open, because room will not, probably, be found for them under the drying-sheds. If such a thing is possible, then it is all the better, but it is a matter of less importance that flooring-boards should be protected from the weather than any other kind of wood that is required to be dried. When flooring-boards are to be dried in the open, it is better that they should be "perched" on their ends rather than laid out in a horizonal or triangular form. When piled in a triangular form the boards are exceedingly liable to sway at their centers, and thus they will be likely to dry in a twisted form. They also hold at times a considerable amount of water in such a case. It is a good plan, when stacking flooring-boards on a perch to pile the boards in pairs with their face sides together, as in such case the faces of the boards are kept clean, and thus the necessity of replaning the faces of the boards when laid is renpered unnecessary. Seasoned boards will enter upon second shrinking or expansion process if their newly-formed skins be removed with a planing-iron. There are those persons who are in the habit of drying their flooring-boards before passing them through the planing-machines. This is done so as to have a perfectly clean board when laid, but, for the reasons given, the result is not satisfactory, and further than this, clean faces can be preserved by adopting the practice of stacking the boards in pairs to dry.

For certain processes, such as mould-making, for instance, wood must be highly dried, and in such cases "stoving" may be resorted to with advantage. The main reform, however, required to be effected in respect to the seasoning of building wood appears to be the more general erection of suitable and sufficient drying-sheds, having open sides and flat, movable roofs. Doubtless the erection of such sheds necessitates the expenditure of a considerable amount of money, but it is as well to bear in mind the admitted adage, that what is worth doing at all is worthy of being done well. There can be no doubt but that most consumers of wood suffer heavily through losses incurred in consequence of the depreciation of stock which has not been sufficiently well cared for. A well-designed and suitably situated drying-shed will permit of the wood which is stored under it being perfectly seasoned, and yet kept free from grit and dust, whilst the havoc which the sun, the rain, and the frost usually commit upon wood that is undergoing the process of being dried will be altogether avoided.

It should be well borne in mind that wood often suffers very much in consequence of its having been stored away in the sale timber-yards without any sort of protection, and it is to be hoped that ere long some protective provisions, in the shape of sheds, will be brought into general requisition by the timber merchants. Until such protective provisions do become generally established, we would strenuously advise builders to buy, when possible, newly-arrived stock, and to take care themselves of that portion which they intend to devote to joinery or other similar purposes.

THE KINETIC THEORY OF GASES.

By HENRY T. EDDY, C.E., PhD., University of Cincinnati.

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1. INTRODUCTORY.—The atomic theory of the constitution of matter in general is an hypothesis which originated so far as we know among the Greek philosophers* but was held by them merely as part of certain philosophical and metaphysical systems. The development of the atomic and molecular theory by comparison with the facts of nature is a part of modern science with which chemistry has been largely concerned. But the Kinetic theory of the constitution of gases is a branch of this theory which does not lie in the domain of chemistry but almost exclusively in that of mathematical physics. It is a working hypothesis as to the physical nature of gases, and attempts to conceive of and account for their properties by a definite mechanical arrangement of atoms and molecules.

It does not apply to solids and liquids since they are regarded as possessing a more intricate arrangement mechanically considered, and, indeed, gases can have the supposed simplicity of construction only when not too dense.

It may be that the hypothesis is not a presentation of facts at all true to nature, it may be that the ideas embodied in the supposed atomic and molecular constitution of matter are in fact untrue; nevertheless the multitudinous cases of agreement between the mathematical conclusions drawn from the theory with the facts of nature leave hardly the shadow of a doubt that the mathematical relations arrived at by this theory expressed substantially actual laws of nature. The reason of this may be that the mathematical expressions are of more general nature than the special physical hypothesis which they have been used to express, and thus are possibly true while the hypothesis is untrue.

This theory is not merely an endeavor to get rid of metaphysical and other difficulties respecting the constitution of gases by predicating phenomena of molecules and atoms because they are so small that it may be quite impossible to refute assertions made respecting them. On the contrary, something of the kind assumed by this theory has place in nature as is evident from spectroscopic and other evidence. At all events, the theory is in many points so nearly coincident with experimental phenomena that it is deservedly regarded as a part of modern science, occupying a place analogous to that of the undulatory theory of light, as a useful, perhaps indispensable instrument of investigation. The deviations of theory from experiment are in many cases fully explained as in other applications of mechanics to complicated material systems, by the impossibility of solving the problem in its most general and complicated form, for which reason we are obliged to content ourselves with a more simple result, which is only a close approximation to the case which we had wished to treat.

The first step of importance in the mathematical investigation of this theory was taken by Daniel Bernoulli, who showed in 1738 how the pressure exerted by gases may be accounted for on this theory.

But it is the investigations of Clausius from 1857 onward, followed by the brilliant discoveries of Maxwell in 1860, and more recently the extended researches of Boltzemann, published during the last ten or fifteen years in the *Wiener Sitzungsberichte*, which have principally awakened the attention of physicists to the importance of the theory, and led to its development and experimental confirmation. Others have made important contributions to the theory, among whom may be mentioned Krönig, whose first paper, in 1856, anticipated some of Clausius results; Joule, whose first paper appeared in 1851; Nilyer and Watson whose valuable treatises* have done much

* See article entitled "Atom," in the Encyclopedia Britannica, Vol. III, 9th edition.

* Die Kinetische Theorie der Gase, pp. 338. D. O. E. Meyer, Breslau. 1877. The Kinetic Theory of Gases, pp. 51. H. W. Watson, M.A., Oxford, 1876.

to render the theory accessible to the student.

2. General Statement of the Theory.—The Kinetic theory assumes that any given volume of any of the gases consists of a great number of separate particles which are almost or entirely independent of each other. These particles are usually identified with chemical molecules. In a state of equilibrium these molecules are supposed to be in rapid and independent motion in straight lines. The unoccupied space between the molecules is assumed to be very large compared with that actually occupied by the molecules themselves. This might be illustrated by comparing the molecules to a swarm of insects on the wing.

It must happen that frequent encounters will occur between molecules moving in this manner.

The length of the path between the successive encounters of the same molecule with any others is called its free path. The length of the free path is, therefore, very great compared with the dimensions of the molecule itself, and the time occupied by an encounter is very small compared with that consumed in describing a free path.

The theory further supposes that the sensible heat of a gas exists in it in that form of energy called *vis viva* and is the total energy of progressive motion of the molecules, so that when the temperature of a gas is augmented it is assumed that the average velocity of its molecules is so increased that their augmented energy of translation corresponds to the increase of temperature.

As to the velocity of the molecules, Clausius' first investigations were made on the supposition that in a simple gas, *i.e.*, one which is not a mixture, all the molecules have the same velocity, and the results obtained on this supposition are for many purposes correct, the velocity supposed being simply such a mean velocity as would make the progressive energy equal to the sensible heat.

But Maxwell was the first to point out that whatever be the original distribution of velocities there is but one final distribution which can be permanent, a distribution whose only law is the law of chance, but one which is susceptible of mathematical statement and one in the average of a very large number of molecules must in all cases give the same result.

The whole path of a molecule will then consist of a broken, zigzag line, the straight portions of which will be of unequal length and in every possible direction, the length, direction, and velocity being dependent upon the laws of chance. Hence, after the lapse of even a considerable interval of time, a molecule will in general be found at an inconsiderable distance from its initial position.

It has often been furthermore assumed that the molecules are perfectly elastic or perfectly hard, or both, and objections have been made to the theory on this score, but assumptions of this nature seem not to be a necessary part of the theory.

3. Clausius' Theorem Respecting the Virial of Stationary Motion.—A material system is said to be in stationary motion when its constituents do not continually depart from their initial position, and their velocities do not continually recede from their initial values. The molecules of a gas in equilibrium constitute such a system. For the sake of clearness let the system of molecules under consideration to those constituting a one unit of mass of gas contained within impervious, immovable walls, although the same laws will evidently hold in case the walls are merely imaginary boundaries conceived as separating the unit of gas under consideration from other surrounding units.

Let m be the mass of any molecule of this gas and xyz its rectangular co-ordinates referred to any origin; let XYZ be the components along the axes of xyz respectively of the resultant of all the external forces acting upon the molecule m, taken as positive when they tend to increase xyz respectively.

Now, by the principles of the differential calculus, we have the identical equation

$$\frac{1}{2}\frac{d^2}{dt^2}(x^2)=\frac{d}{dt}\left(x\frac{dx}{dt}\right)=\left(\frac{dx}{dt}\right)^2+x\frac{d^2x}{dt^2} \quad . \quad (1).$$

as may be seen by performing the differentiation expressed in the first two terms. But by the fundamental principles of dynamics we also have

$$m\frac{d^2x}{dt^2}=X \quad . \quad . \quad . \quad . \quad (2).$$

Substitute from (2) in (1) and transpose,

$$\therefore \quad \frac{m}{2}\left(\frac{dx}{dt}\right)^2=\frac{m}{4}\frac{d^2}{dt^2}(x^2)-\frac{x}{2}X \quad . \quad . \quad (3).$$

The first member of (3), being one-half of the product of the mass by the square of the velocity, expresses the energy of progressive motion of m parallel to x at any instant when its co-ordinate is x and the moving force along x is X.

To find the average value of this energy during any interval t, (3) must be multiplied by dt, integrated between the limits 0 and t, and then the result divided by t, this being the ordinary process for finding the average or mean value of any function for the time t.

$$\therefore \quad \frac{m}{2t}\int_0^t\left(\frac{dx}{dt}\right)^2dt=\frac{m}{4t}\left\{\left(\frac{d}{dt}(x)^2\right)_t\right.$$

$$\left.-\left(\frac{d}{dt}(x^2)\right)_0\right\}-\frac{1}{2t}\int_0^t x\times dt \quad . \quad (4).$$

The first and last terms of (4), as just stated, are expressions for the average or mean values of the corresponding terms in (3). But the quantity within the square brackets is of a different nature; it is the difference between the final and initial values (as expressed by the subscripts) of a function whose final and initial values may, if t be properly chosen, be equal. But it is unnecessary so to choose t. For, by reason of the divisor t, it appears that if t be taken sufficiently large, the term under consideration vanishes even though the final and initial values are not equal, since they cannot recede indefinitely from each other. That they cannot so recede appears from the identical equation, obtained as was (1)

$$\frac{d}{dt}(x^2)=2x\frac{dx}{dt} \quad . \quad . \quad . \quad . \quad . \quad (5).$$

From which it appears that the term is dependent upon the products of quantities (co-ordinates and velocities) which by the definition of stationary motion could not recede indefinitely from their initial values.

Let now xyz denote no longer the actual co-ordinates of m, but instead the average values of these quantities during the time t, and similarly for XYZ, and let $x'y'z'$ be the corresponding velocities along the axes. Hence, we may write (4), and the two similar equations with respect to the axes of y and z, as follows:

$$\left.\begin{array}{l} mx'^2=xX \\ my'^2=yY \\ mz'^2=zZ \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (6).$$

in which, as just stated, the variables express average values during the interval t, which may be taken so large as to give sensibly constant values.

Let the unit of gas under consideration consist of n molecules, which may have equal or unequal masses.

Like equations apply to all the members of the system of molecules which may be distinguished as $m_1, m_2 \ . \ . \ . \ m_n$. Suppose these equations formed and take their sum.

$$\therefore \quad \tfrac{1}{2}\Sigma_1^n m(x'^2+y'^2+z'^2)=$$

$$-\tfrac{1}{2}\Sigma_1^n(xX+yY+zZ) \quad . \quad . \quad . \quad (7).$$

The first member of (7) is the total Kinetic energy of the system, and the last member is called the virial of the system, and depends upon the average forces acting upon the particles and their position.

The theorem which has been now demonstrated may be thus stated: The mean kinetic energy of a system in stationary motion is equal to its virial.

4. External Virial and Gaseous Pressure.—In case the molecules neither attract nor repel each other, and no forces act upon them except the external pressure exerted by the walls of the vessel containing them, the virial of this pressure may be computed as follows: Let the closed surface containing the unit of mass of the gas under consideration be of any shape whatever. It is evident that the pressure of the enclosed gas in equilibrium will be normal to the surface. Let p be the intensity of this pressure per unit of area, and let dS be the element of area of the enclosing surface, and l the cosine of the angle which the normal to it makes with the axis of x.

$$\therefore \quad ldS=dydz \quad . \quad . \quad . \quad . \quad (8).$$

$$\therefore \quad -\frac{1}{2}\Sigma_1^n xX=\frac{1}{2}\int xpldS$$

$$=\frac{p}{2}\int\int xdydz \quad . \quad . \quad . \quad (9).$$

in which the integration in the second member is to be extended over the enclosing surface alone, for only those molecules are acted upon by the pressure which are at the surface. The last integral in (9) expresses the whole volume v enclosed within the surface and does not depend upon the kind of molecules under consideration. The sign of p is opposite to that of X, because p causes the molecules to approach each other, while X tends to make them recede from each other.

$$\therefore \text{ by (9) } -\tfrac{1}{2}\Sigma_1^n xX=\tfrac{1}{2}pv \quad . \quad . \quad (10).$$

with similar equations for y and z, which being added to (10) give the total external virial due to the pressure.

$$\therefore \text{ by (7)} \tfrac{1}{2}\Sigma_1^n m(x'^2+y'^2+z'^2)=\tfrac{3}{2}pv. (11).$$

in which v is the volume occupied by the unit of gas considered, and n is the number of molecules in it, and (10) expresses the relation existing between the total energy of the progressive motion of the molecules to the volume and pressure in case the molecules neither attract nor repel each other.

Now, suppose that the unit of gas under consideration consist of molecules which are all of the same kind, *i. e.*, it is not a mixture of two or more gases, then $m_1=m_2$, etc.$=m_n$, $x'^2_1=x_2'^2=x'^2_n$, etc., etc., since each molecule will then have the same mean component velocities. Let $v^2=x'^2+y'^2+z'^2$, then (11) may be written in the form

$$\tfrac{1}{2}nmv^2=\tfrac{3}{2}pv \quad . \quad . \quad . \quad . \quad (12).$$

in which v^2 is the mean square of the velocity of the molecules, and the first member is the mean energy of translation of all the molecules in the unit of mass of the gas. If q is the mass of a cubic unit of this gas, then

$$nm=qv \quad . \quad . \quad . \quad . \quad (13).$$

in which either side of the equation expresses the unit of mass of the gas under consideration. Eliminate nm from (12) and (13).

$$\therefore \quad qv^2=3p \quad . \quad . \quad . \quad . \quad (14).$$

an equation which enables us from the known values of p and q to compute the value of v.

Let us consider the atmospheric air for the moment to be sufficiently near by a homogeneous gas for our purpose, though in fact it is a mixture whose different molecules differ slightly in mass.

Take the mean atmospheric pressure at $p=2116.4$ lbs. per square foot, and the product of the weight of 1 cubic foot of air at 32° F., at this pressure ($=0.080728$ lbs.) by the acceleration of gravitation, as the mass $q=0.80728\times 32\tfrac{1}{6}$ from which we obtain

$$v=1591 \text{ ft. per sec. for air,}$$

a velocity which does not from that of a rifle ball, and is about one and a half times that of sound.

The corresponding velocities for the simple gases can be found in like manner. Besides the above value for air, Clausius gave, in 1857, the following values:

For oxygen, $v=1512$. ft.
For nitrogen, $v=1614$. ft.
For hydrogen, $v=6048$. ft.

5. Internal Virial of Molecular Attraction and Repulsion.—The magnitude and algebraic form of the internal virial affords pretty decisive evidence as to the existence or non existence of molecular attractions or repulsions of appreciable amount. In order to compute it let r be the distance between any two molecules m_1 and m_2, and let $f(r)$ be the function of r, which expresses the law of the force, the value of which is positive in case it be a repulsion, and negative for an attraction.

$$\therefore \quad x_1X_1+x_2X_2=x_1\frac{x_1-x_2}{r}f(r)$$
$$+x_2\frac{x_2-x_1}{r}f(r)=\frac{(x_2-x_1)^2}{r}f(r) \ . \ (15).$$

with two similar equations for the remaining axes of y and z for each pair of molecules, in which $\frac{x_1-x_2}{r}$ is the cosine of the angle which the repulsion which m_2 exerts on m_1 makes with the axis of x, etc., etc.

If we take the summation of the three equations for each pair of molecules in the system by taking with each of the n molecules all the others, it is evident that a pair of terms containing the quantities $(x_2-x_1)^2$ and $(x_1-x_2)^2$ will appear and other pairs of the same form, so that the summation taken in this manner will be twice the result sought.

Furthermore, $(x_2-x_1)^2+(y_2-y_1)^2+(z_2-z_1)^2=r^2$, etc.

$$\therefore \quad -\tfrac{1}{2}\Sigma_1^n(xX+yY+zZ= -\tfrac{1}{4}\Sigma_1^n\Sigma_1^n rf(r) \quad . \quad . \quad . \quad (16).$$

in the last member of which the value of $\Sigma_1^n rf(r)$ is to be found for each of the n molecules by taking with each one all the others, and then these separate summations are to be added to form the sum total. The last member of (16) is often written with the coefficient $\frac{1}{2}$, in which case $\Sigma rf(r)$ is to be taken for all possible different pairs of molecules.

It is evident that the expression for the internal virial in (16), as also that for the external in (11) depends in no way upon the axes chosen for making the summation, for the results obtained are independent of the axes. But in general the value of the internal virial in (16) depends upon the form of the surface enclosing the gas under consideration, unless a suitable special form be assigned to the friction $f(r)$ which expresses the law of the force. Now within the limits of ordinary experiment the form of the vessel exerts no appreciable influence upon the behavior of the gas, for let the values of the external and internal virial from (11) and (16) be substituted in (7).

$$\therefore \quad \tfrac{1}{2}\Sigma_1^n m(x'^2+y'^2+z'^2)=\tfrac{3}{2}pv -\tfrac{1}{4}\Sigma_1^n\Sigma_1^n rf(r) \quad . \quad . \quad . \quad (17).$$

First suppose the unit of mass of the gas enclosed within a sphere of volume v, and next within a long narrow tube of the same volume. The first member, which is the total energy of translation of the molecules is not changed if the temperature is not; hence unless the last term be constant p must vary. But p being invariable within the limits of ordinary experiment the last term is constant.

$$\therefore \quad rf(r)=a, \quad \therefore \quad f(r)=\frac{a}{r} \quad . \quad . \quad . \quad (18).$$

in which a is some constant. But the law of force expressed by (18), which is a repulsion inversely as the distance would cause the pressure to be greater in the more distant parts of the vessel. Hence within the limits of experiment $a=0$ and the internal virial vanishes.

In the computation of the internal virial the virial due to encounters between the molecules has been neglected, for on account of the assumed minute size of the molecules its amount may be seen to be small as follows: In an encounter between m_1 and m_2, $X_2=-X_1$

$$\therefore \quad x_1X_1+x_2X_2=X_2(x_2-x_1) \quad . \quad . \quad . \quad (19).$$

in which (x_2-x_1) is the projection upon x of the distance between the centers of the molecules at the instant of encounter, which has been assumed to be small, an assumption which will be justified later. We shall also endeavor hereafter to take into further consideration the effect of very feeble forces acting between the molecules as well as that of the encounters.

Several demonstrations have been given of (11) besides that by means of the virial, all of them assuming, of course, that no molecular attractions or repulsion exist. The special advantage of this demonstration is that certain obscurities are avoided, and no assumption is made as to the elasticity or want of elasticity of the molecules; and further, in (17) is a term dependent upon the internal forces which can be employed when it shall seem best to consider them.

6.—Extension of Clausius' Theorem of the Virial to Rotations.—The author has ventured to propose the following investigation of the virial of rotations and rotary velocities of bodies in stationary motion analogous to that given by Clausius for translations.

Let a, b, c, be the moments of inertia of any molecule m with respect to its principal axes. Let

$$\frac{d\theta}{dt} \cdot \frac{d\varphi}{dt} \cdot \frac{d\psi}{dt}$$

be the angular velocities about these principal axes, then are θ, Φ, ψ angular co-ordinates with reference to same initial lines yet to be assumed.

Let L, M, N be respectively the components about these axes of the resultant of all the couples acting upon the molecule.

Then by Enler's equations of rotary motion

$$a\frac{d^2\theta}{dt^2}=(b-c)\frac{d\varphi}{dt}\cdot\frac{d\varphi}{dt}+L \quad . \quad . \quad . \quad (20).$$

We also have the identical equation,

$$\frac{1}{2}\frac{d^2}{dt^2}(\theta^2)=\frac{d}{dt}\left(\theta\frac{d\theta}{dt}\right)=\left(\frac{d\theta}{dt}\right)^2+\theta\frac{d^2\theta}{d^2t}. \quad (21).$$

as appears by performing the differentiation expressek in the first two terms.

Substitute from (20) in (21) and transpose

$$\therefore \quad \frac{a}{2}\left(\frac{d\theta}{dt}\right)^2=\frac{1}{4}\frac{d^2}{dt^2}(\theta^2)-\frac{1}{2}(b-c)\theta$$

$$\frac{d\varphi}{dt}\frac{d\psi}{dt}-\frac{1}{2}\theta \mathrm{L} \quad . \quad . \quad . \quad . \quad (22).$$

The first member of (22) being one-half of the product of the moment of inertia about one of the principal axes by the square of the angular velocity about that axis expresses the energy of rotation about that axis.

Now find the average value of this energy during the interval t as was done in (3), and let θ, θ', L, &c., be now used to express mean values of θ. $\frac{d\theta}{dt}$.L, &c., for the molecule m.

We then obtain from (22) an equation which with the two similar ones with respect to the remaining principal axes may be written

$$\left.\begin{array}{l} a\theta'^2=-\theta\mathrm{L}-(b-c)\theta\varphi'\psi' \\ b\varphi'^2=-\varphi\mathrm{M}-(c-a)\varphi\psi'\theta' \\ c\psi'^2=-\psi\mathrm{N}-(a-b)\psi\theta'\varphi' \end{array}\right\} \quad . \quad . \quad . \quad (23).$$

in which it is to be noticed that the quantities θ'^2, φ'^2, ψ'^2, in the first members are not the squares of the quantities represented by θ', φ', ψ' in the second members, for in the first members the quantities are the mean squares of all the numerical values of the angular velocities and no account is, therefore, taken of their signs. But the mean values of θ', φ', ψ' in the second members of (23) are each zero, because on the average there must apparently be as many and as large positive as negative angular velocities about any one of the axes. The last terms of (23), therefore, vanish, whatever may be the mean values of θ, φ, ψ.

Now let

$$\mathrm{L}=\rho_1\mathrm{X}, \quad \mathrm{M}=\rho_2\mathrm{Y}, \quad \mathrm{N}=\rho_3\mathrm{Z} \quad . \quad . \quad (24).$$

in which Y, X, Z have same significations as in (6) and (7), there is ρ_1 the mean value of the arm of the couple L, etc.

Also let

$$\theta\rho_1=x, \quad \varphi\rho_2=y, \quad \psi\rho_3=z \quad . \quad . \quad . \quad (25).$$

which equations will fix the initial lines from which θ, φ, ψ are measured.

Hence by (24) and (25) we may write (23) thus—

$$\left.\begin{array}{l} a\theta'^2=-\theta\mathrm{L}=-x\mathrm{X} \\ b\varphi'^2=-\varphi\mathrm{M}=-y\mathrm{Y} \\ c\psi'^2=-\psi\mathrm{N}=-z\mathrm{Z} \end{array}\right\} \quad . \quad . \quad (26).$$

Now take the sum of equations (26) for n molecules,

$$\therefore \quad \tfrac{1}{2}\Sigma_1^n(a\theta'^2+b\varphi'^2+c\psi'^2)=$$

$$-\tfrac{1}{2}\Sigma_1^n(x\mathrm{X}+y\mathrm{Y}+z\mathrm{Z}). \quad . \quad (27).$$

the first member of which is the total energy of rotation of the molecules of the gas, and the second member is the virial which is to be computed as in (7).

$$\therefore \quad \text{by (10)} \quad \tfrac{1}{2}\Sigma_1^n a\theta'^2=\tfrac{1}{2}pv \quad . \quad . \quad (28).$$

$$\therefore \quad \tfrac{1}{2}\Sigma_1^n(a\theta'^2+b\varphi'^2+c\psi'^2)=\tfrac{3}{2}pv \quad . \quad (29).$$

From (11) and (29) it appears that in general the mean energy of translation is equal to the mean energy of rotation, and that the total kinetic energy of a gas is one-half due to progressive motion and one-half to rotation.

We have said that this is true in general, we must, however, except the cases in which one or more of the couples L, M, N vanish identically L, M, N may evidently all three so vanish when the molecules are in effect smooth spheres, and a single one of them may vanish when the molecules are in effect smooth solids of revolution.

In case L=M=N=0 we must have in (24) $\rho_1=\rho_2=\rho_3=0$, and Eqs. (25) are then impossible, but by recurring to (23) we see that the energy of rotation about each axis vanishes.

In case only L=0, the total energy of rotation is

$$\tfrac{1}{2}\Sigma_1^n(b'\varphi+c\psi'^2)=pv \quad . \quad . \quad . \quad (30).$$

From (11) and (30) it appears that in this case the energy of rotation is two-thirds that of translation, and the total kinetic energy is two-fifths of it rotary and three-fifths progressive.

It does not seem necessary to suppose that the preceding investigation applies only to molecules in which abc remain constant, although it appears not unreasonable hypothesis to suppose that the molecules of any substance are unchanged by change of state of aggregation, and that whether a substance be solid, liquid, or gaseous, the molecules of which it is

composed may be regarded as nearly rigid, *i. e.*, incapable of deformation to any considerable extent by finite forces, which is equivalent to supposing abc constant.

It should be further noticed that in case $b=c$ it is not necessary to suppose as we have done that the mean values of θ', φ', ψ' vanish, for we shall evidently then have $\varphi'=\psi'$; and the two equations $b=c$, and $\varphi'=\psi'$ are sufficient to enable us to derive (26) and (27) from (23). The hypothesis that $b=c$ must in many cases be correct, and it may perhaps be shown in all cases not to be far from true, when (27) will be approximately true from this reason alone.

7. Temperature, Volume and Pressure.—The first member of (11) is the mean energy of the progressive molecular motion of a unit of mass of gas: and since heat is known to be energy, this energy is ordinarily assumed to be the quantity of sensible heat in the unit of gas, as stated in Art. 2.

It would, however, be apparently more correct to assume that the sensible heat is the total kinetic energy, rotary and progressive. But since it has been shown that in any given gas these quantities have a fixed ratio to each other, this assumption makes no change in the equations employed. The mean progressive energy in either supposition is proportional to and measured by the absolute temperature τ of the gas, *i. e.*, τ is to be reckoned from a state devoid of energy.

Let k be the specific heat of the gas at constant volume, measured in ft. lbs., then the statement just made is expressed by the equation

$$\tfrac{1}{2}\Sigma_1^n m(x'^2+y'^2+z'^2)=ak\tau \quad . \quad . \quad (31).$$

in which a denotes what fraction of the total heat $k\tau$ contained in the gas exists in it in the form of progressive molecular motion.

If, in (17), we let $f(r)=-\mathrm{R}$, then $+\mathrm{R}$ expresses the mean attractive intermolecular force.

If also we let a single sign of summation denote the sum with respect to all possible pairs of molecules, we obtain on substituting (17) thus modified in (31),

$$ak\tau=\tfrac{3}{2}pv+\tfrac{1}{2}\Sigma r\mathrm{R} \quad . \quad . \quad . \quad . \quad (32).$$

in which it should be noticed that for a given value of pv the greater the attraction between the molecules the higher the temperature, so that a perfect gas, *i. e.*, one having no intermolecular attractions, is at a higher temperature than one whose molecules attract; a remarkable result, the bearing of which seems heretofore to have been overlooked, but which will appear later.

If the effect the mutual attractions and repulsions be neglected, we may write (32) in the form

$$pv=c\tau \quad . \quad . \quad . \quad . \quad . \quad . \quad (33).$$

where for brevity $c=\tfrac{2}{3}ak$.

It is seen that (33), which is here derived from theoretical considerations alone, is the equation expressing the empirical law of Gay Lussac for the so-called perfect gases, and it also includes Boyle's law. We may therefore consider (33) to express the laws obeyed by a system of molecules having no mutual attractions or repulsions, and moving in a space infinitely larger than that occupied by the molecules themselves; if, however, an appreciable fraction of the whole space in which they move is occupied by the molecules themselves, the effect of encounters between them will, on the whole, be that of a feeble repulsion; for owing to their assumed hardness their centers cannot approach within a certain distance of each other. All the minute interactions between the molecules, attractive or repulsive, which interfere with the rigorous exactness of (33) are included in the last term of (32) which seems to be of such importance to the exactness of the theory as to demand more close scrutiny than it has heretofore received.

Let the temperature of the gas be augmented by the small amount $d\tau$, the remaining variables in (32) will also suffer variations expressed by the equation

$$ak'd\tau=\tfrac{3}{2}d(pv)+\tfrac{1}{2}\Sigma d(r\mathrm{R}) \quad . \quad . \quad (34).$$

in which, considering for the moment the interactions of a single pair of molecules only,

$$d(r\mathrm{R})=\mathrm{R}dr+rd\mathrm{R} \quad . \quad . \quad . \quad . \quad (35).$$

R and r expressing mean values for the pair considered.

The term $\mathrm{R}dr$ in (35) is evidently the work performed in increasing the distance of the pair of molecules by the amount dr against their mutual attraction

R, and is an amount of work which will then exist in the gas in the form of potential energy.

The term rdR in (35) expresses the amount of variation of energy involved in the variation of the mean attraction. In case R is a function r, dR will depend upon r and dr. But R may be also a function of τ, as seems quite evident from the phenomena observed near the point of condensation of gas, where for example carbonic acid gas is undoubtedly brought into a state more nearly fulfilling Boyle's law by simply increasing the temperature at constant volume, and constant volume implicitly involves a constant mean value for r.

If the sign of rdR is positive, the amount of energy employed becomes potential; if, however, it is negative, it is an amount of potential energy previously stored which is now set free. The latter is undoubtedly the fact, which perhaps may appear more clearly when we say that when the gas is raised to such a temperature that it is completely dissociated, and all attractions between its molecules and atoms are completely destroyed, then all the potential energy, intermolecular and interatomic, which has been stored in the gas, will be set free and become kinetic. The term rdR expresses then the amount set free during the variation $d\tau$.

To obtain a conception of the relative magnitude of the terms in (35) let us take the hypothesis that R is a function of r, expressed by the equation $R=fr^{-i}$,

$$\therefore\quad Rdr=fr^{-i}dr,\quad rdR=-ifr^{-i}dr,$$

$$\therefore\qquad rdR=-iRdr \quad . \quad . \quad . \quad . \quad (36).$$

from which it appears that when the attraction varies as the inverse i the power of the distance the energy set free is i times that which becomes potential. Different values of i, all greater than unity, have been proposed by various physicists to represent approximately the actual attraction.

On the other hypothesis suggested, namely, that R is a function of τ, such that R decrease as τ increases, it is evident that the term rdR is negative; and, as has been stated, that signifies that potential energy becomes kinetic by increasing temperature, although the relative amount cannot be found while the function, which R is of τ, is unknown.

The experiments of Thomson and Joule*, in making air and other gases expand freely in passing through a porous plug enable us to compare (34) and (35) with the results of experiment.

In the experiments now considered the variation of temperature is very small, and the gases obey the law of Boyle so closely that we are at liberty to assume $d(pv)=o$,

$$\therefore \text{ by (34)}\quad akd\tau=\tfrac{1}{2}\Sigma(Rdr+rdR)\ .\ (37).$$

in which the second member depends upon mean intermolecular forces and distances. If we take the hypothesis of (36),

$$akd\tau=\tfrac{1}{2}\Sigma(1-i)r^{-i}dr)\ .\ .\ .\ (38).$$

in which, owing to the expansion of the gas, dr is positive. The experiments showed $d\tau$ to be invariably negative, thus agreeing with the previous statement that $(1-i)$ is probably negative.

The explanations of this experiment ordinarily given appear to be erroneous, although they likewise make $d\tau$ negative. The explanation assumes in effect that the total intermolecular work depends upon terms of the form Rdr, and further states that this work must be done at the expense of the sensible heat of the gas, thereby attempting to apply the principle of the conservation of energy to the case in hand. This is stated mathematically by the equation

$$\lambda kd\tau+\Sigma(Rdr)=0\quad .\ .\ .\quad (39)$$

in which λ is a numerical coefficient. By comparing this with (37), when terms of the form rdR are neglected, it appears that the assumed equation (39) has its terms in Rdr of the wrong sign. According to the theorem of the virial as expressed in (37), the experiment should have given $d\tau$ positive when terms in rdR vanish; but they do not so vanish, and (37) is the correct equation of the conservation of energy. The mistake in (39) is the assumption without proof that the internal work depends only upon terms of the form Rdr. The introduction of the principle of the conservation of energy gave the correct sign to $d\tau$, notwithstanding this assumption, for the

* Phil. Trans. R. Soc. Lond., 1853, 1854, 1862; or Wüllner's Experimental Physik, 3te Aufl. 1875, Bd. 3, S. 461.

form of the expression for the internal work is here of no consequence; but in other cases, where the principle of the conservation of energy could not be employed, the assumption that the internal work is expressed by terms of the form Rdr has led to erroneous results, contradicting experiment. This is notably true respecting the ratio of the specific heats discussed in Art. 8.

The well-known behavior of carbonic acid and other gases, as they approach the point of condensation, also shows that the terms in Rdr and rdR are of opposite sign, the latter being numerically the larger.

Let a unit of gas be compressed at constant temperature until its pressure is increased by an amount dp, then $d\tau=0$ and (34) becomes

$$-pdv=vdp=\tfrac{1}{3}\Sigma(\mathrm{R}dr+rd\mathrm{R}) \quad . \quad (40).$$

in which dv and dr are negative, because the gas is compressed; hence $-pdv$ and vdp are both positive, but Rdr is negative. Experiment shows that for a given increment dp, the decrement dv (and hence the term $-pdv$) is numerically larger than it should be by Boyle's law, *i.e.*, than when the intermolecular forces vanish. If the terms in Rdr were alone considered, they would, since they are negative, decrease rather than increase $-pdv$; hence it must be the terms in rdR which cause the observed numerical increase in dv.

In the case just treated the signs of both terms of $d(r\mathrm{R})$ are the opposite of those in the case of free expansion previously treated, because here compression takes place, and as is seen kinetic energy is set free by the terms in Rdr, but a relatively larger amount is transformed into potential energy by terms in rdR.

Berthelot's* principle of the maximum work (*i.e.*, maximum heat) of chemical decomposition states that every chemical change, accomplished without the intervention of energy from without, tends to the production of the body, or the system of bodies, which set free the most heat.

This is announced as a law resting for its proof upon a vast array of experimental evidence, and is evidently of fundamental importance to the theory of chemistry. It seems, however, to depend theoretically upon the mechanical principles involved in (34), for at the instant of chemical decomposition the atoms must be regarded as separate bodies, obeying the laws of stationary motion.

In order to introduce the condition of no exchange of energy with external bodies, let $dv=0$. $\therefore pdv=0$, *i.e.*, there is no work of expansion against external pressure, and if vdp be small enough to be neglected, then (37) may be applied to this case. It appears, however, that the ordinary conditions of experiment from which the law was deduced would be better represented by supposing the pressure constant, *i. e.*, $dp=0$, $\therefore vdp=0$, in which case pdv the external work is, as appears from experimental evidence in general so inconsiderable as not to affect the correctness of the law, so that (37) may be considered in this case also to express approximately the relations to which it is desired to give expression.

Considering now the case when $dv=0$, the terms in (37) in Rdr are inconsiderable, since the mean distance of the atoms cannot be greatly changed when the total volume is constant. But the terms in rdR depend principally upon the increment dR of the attractions of the atoms in the final chemical arrangement over those in the initial. If the atoms in the final arrangement obey the greater attractions, then rdR is positive, and by (37) heat is set free, or the temperature of the body raised. Thus it appears that Berthelot's principle—that of several possible chemical decompositions in an isolated body, that one will occur which sets free the largest quantity of heat—follows as a consequence of the axiomatic principle that the more powerful interatomic attractions will always determine the rearrangement of the atoms into molecules.

8. Ratio of the Specific Heat at Constant Pressure to that at Constant Volume.—Let κk be the specific heat at constant pressure, and k that of constant volume, in foot lbs., then is their ratio κ a quantity which has been determined experimentally for a number of gases with considerable accuracy. Its theoretical value can be found as follows:

Let the temperature of a unit of mass of gas be augmented by $d\tau$ at constant pressure; let dh be the total amount of heat imparted,

$$\therefore \qquad dh=\kappa k dt.$$

* Essai de Méchanique Chémique, 1879, t. 2, p. 421.

Let ds be the increment of the sensible heat,

$$\therefore \quad ds=kd\tau.$$

Let dw be the external work performed,

$$\therefore \quad dw=pdv;$$

$$\therefore \text{ by (34) } dw=\tfrac{2}{3}\alpha kd\tau-\tfrac{1}{3}\Sigma d(r\text{R}).$$

Let dw_1 be the internal energy employed against intermolecular forces, then, as has been previously shown.

$$dw_1=\Sigma d(r\text{R}),$$

in which let $\Sigma d(r\text{R})=\beta kd\tau$, then β expresses how large a fraction dw_1 is of ds; and as has been shown in Art. 7, β is almost certainly negative for gases.

We may then write

$$dw+dw_1=\tfrac{2}{3}(\alpha+\beta)kd\tau.$$

Let dw_2 be the internal work done within the molecule against interatomic forces; and if we let

$$dw_2=\gamma kd\tau,$$

then γ expresses what fraction dw_2 is ds. On the hypothesis that molecules are solids whose atoms stand at nearly invariable distances at all temperatures, but whose atomic attractions are decreased as the temperature rises, γ would consist almost entirely of terms in $rd\text{R}$, and so be always negative.

But the total heat imparted being equal to the sum of its parts,

$$dh=ds+dw+dw_1+dw_2 \quad . \quad . \quad . \quad (41).$$

Substitute the values of the various terms in (41), which have been just given, and divide by $kd\tau$,

$$\therefore \quad \kappa=1+\tfrac{2}{3}\alpha+\tfrac{2}{3}\beta+\gamma \quad . \quad . \quad (42),$$

in which, as previously seen, β and γ are most probably negative but quite small.

Suppose for the moment that

$$\tfrac{2}{3}\beta+\gamma=0 \quad . \quad . \quad . \quad . \quad (43),$$

i. e., the intermolecular and interatomic forces vanish, in which case, as shown previously, α has the values 1, $\frac{3}{5}$, $\frac{1}{2}$, according as the molecules are regarded as solid spheres, solids of revolution, or of other figure.

$$\therefore \text{ by (42), when } \alpha=1.0,\ 0.6,\ 0.5,$$
$$\text{then } \kappa=1.67,\ 1.4,\ 1.33.$$

The vapor of mercury is perhaps the only gas regarded by chemists as non-atomic, and for it Kundt and Warburg* found $\kappa=1.67$. In this case it would be difficult to conceive γ to have a value differing from zero; hence, also for this gas, $\beta=0$ within the limits of experimental accuracy, *i.e.*, the forces between the molecules are insensible.

Atmospheric air is almost entirely composed of gases whose molecules are considered to be diatomic, and hence are supposed to be figures of revolution. A considerable number of determinations of κ for air by the most eminent do not differ much from the mean value 1.405, while the derived from Regnault's most accurate determination of the velocity of sound is 1.3945, which numbers are taken from Wüllner,* who treats this subject at length.

As to other diatomic gases, as H_2, O_2, N_2, CO, NO, HCl, the most probable experimental values of κ lie between 1.41 and 1.39, as given by Meyer,† and so far as these numbers show anything they confirm the truth of (43) for this kind of gases also.

All experimental determinations of κ for gases whose molecules consist of more than two atoms, lie, so far as known, between 1.33 and 1.25, and in some cases it would appear that the value of $\frac{2}{3}\beta+\gamma$ must be decidedly negative. This is the case in which from the number of atoms in the molecules we should expect the interatomic action to appear in more marked degree. It should be noticed that α is not itself independent of β and γ, and the form of its dependence upon them could be expressed were it possible to state what function R is of r and τ. But this fact will not apparently reverse the nature of the result we have obtained by supposing α constant. Especially is this the case if γ if numerically much larger than β as has been shown to be extremely probable, knowing as we do that the interatomic attractions are strong and the intermolecular feeble. These considerations add to the probability of the correctness of the hypothesis that molecules are hard, smooth bodies.

Watson, in his treatise,‡ regards it as impossible that β and γ can have negative values, and so finds the experimental

* Pogg. Ann., 1876; Bd. 157, S. 353.

* Lehrbuch der Experimental Physik, 3te Rufl. 1875 Bd. 3. S. 461.

† Kinetische Theorie der Gase, 1876, S. 90.

‡ Kinetic Theory of Gases, 1876, p. 38.

results inexplicable, while Boltzmann,* in his paper *Ueber die Nature der Gasmolecüle*, with whose conclusions mine are in general accordant, finds here the same contradiction as Watson between theory and experiment, due, as has been shown, to the omission of terms in rdR.

* Sitzb. d. Wein. Akad. 1876, Bd. 74.

ON THE SPEED AND POWER OF RAILWAY TRAINS.

By DR. H. SCHOEFFLER.

Organ für die Fortschritte des Eisenbahnwesens. Selected Papers of the Institution of Civil Engineers.

This article gives the methods used on the Brunswick Railways for fixing the speed and size of trains, according to the gradients and curves on the line. The rules are approximate only, but sufficient for practice. The maximum speed in any given train may be determined from two points of view; (1) that of the power of the engine, (2) that of safety in running. For speeds below the lower of these two limits no rule can be given which does not include the fuel expended, and this embraces general considerations of economy, &c.

(1) *Speed at constant expenditure of Fuel.*—Let a be the train resistance, b the weight of the engine and tender, f the number of axles in the train (supposing an average load on each, say 5,000 kilogrammes), 1 in n the gradient, r the radius of the curve. The resistance of the train may be taken at $\frac{1}{300}$ of its weight; that of the engine as $\frac{1}{150}$. But to this must be added the resistances due to the air, &c., which are such that if b be expressed in the corresponding number of train axles, it will have a value varying from 4.5 b to 11 b. The resistance due to the curves may be expressed by adding to the gradient $\frac{3}{4r}$. From these figures the following formula is obtained, as representing the work done per hour by an engine at a speed of v kilometers per hour:

$$\left\{a+f+300\left(\frac{1}{1}+\frac{3}{4r}\right)(b+f)\right\}v.$$

From this expression the velocity v of a given load can be determined. Also the velocity v, or the time t, for a given length of track can be determined in terms of v_1 and t_1, the values for the same train on a straight horizontal road; or in terms of v_0 t_0, the values for the engine alone upon a straight horizontal road.

These formulæ show that the time per hour increases by a percentage of the time t_0, which percentage increases as the gradients are higher and the curves sharper. If it be determined to keep the time constant, a virtual length may be substituted for the real length of the line increased in the same proportion. This, throughout assumes that the consumption of fuel is constant, which is by no means a good assumption for practical working.

(2) *Maximum Speed with a given power of Engine.*—If the consumption of fuel is varied, the speed can evidently be increased up to the maximum power of the engine, or up to the limit of safety. The lower of these limits must be taken as the highest admissible speed. Now, for ordinary speeds the resistance of a train of x axles may be taken as proportional to

$$\left\{1+0.1\,v+1{,}000\left(\frac{1}{n}+\frac{3}{4r}\right)\right\}x,$$

where v is the velocity, $\frac{1}{n}$ the gradient, and r the radius of the curve. The resistance of the engine, taking its weight as equal to that on b axles, is given by

$$d+\left\{1+0.1\,v+1{,}000\left(\frac{1}{n}+\frac{3}{4r}\right)\right\}b,$$

where d is a number found from the resistance of the engine at the minimum speed. By adding these two formulæ together, an expression is obtained for the total resistance which the engine has to overcome at a given speed. The power of the engine being known, the maximum speed can then be calculated; or if the limit of speed is fixed, the number of axles it can draw at that limit can be

calculated. Examples of the calculation are given.

Taking v_1 as the velocity on a straight horizontal road, the following formula is obtained:

$$v=v_1-10,000\left(\frac{1}{n}+\frac{3}{4r}\right)$$

which shows how the speed is to be reduced to correspond with any given gradients and curves. If in the above formula $v=0$, the greatest number of axles are found which a train can move upon a given gradient.

(3) *Maximum Speed with reference to the conditions of the Road.*—The question of safety will often fix the maximum speed. In Germany, on gradients below 1 in 200, and curves of more than 1,000 meters radius, the maximum speed allowed is 90 kilometers per hour (55 miles) for express trains, 75 for ordinary trains (45 miles), and 45 for goods trains (27 miles); and this must be reduced on steeper gradients or sharper curves.

The distance x, in which a train will be brought to rest under the action of brakes, is given by the expression

$$\frac{v^2}{2g\left(\frac{1}{m}-\frac{1}{n}\right)},$$

where v is the speed, $\frac{1}{n}$ the falling gradient, and $\frac{1}{m}$ the retarding resistance, taken as a fraction of the weight of the train. Putting v equal to maximum speed, this gives the value for n on which this speed is allowable. The speed on any other gradient $\frac{1}{n}$ is given by

$$v\left(1+\frac{m}{2n}-\frac{m}{2n'}\right).$$

The quantity m depends, of course, upon the brake arrangements. In Germany, passenger trains on gradients of 1 in 200 must have one-fifth of the wheels braked. If it be assumed that half the weight of the engine and tender is available for braking, and that the coefficient of friction is $\frac{1}{10}$, then $m=20$ for express trains, $m=30$ for passenger trains, and $m=40$ for goods trains. On these data a table is constructed, giving the maximum speed for the three classes on various gradients. The distances in which the train will be brought to rest are as follow:

Express trains, 600 to 700 meters (say 700 yards).

Ordinary trains, 700 to 800 meters (say 800 yards).

Goods trains, 300 to 400 meters (say 400 yards).

With regard to curves, it may be assumed that the centrifugal force is to be always the same; and a table constructed on this basis is given. It may be objected that the raising of the outer rail should be considered; but the German State regulations take no account of this, and it appears, in fact, that the relation between the speed and the radius of the curve is not altered thereby. The practical results are as follow: On gradients of 1 in 200 to 1 in 80, and curves of 1,100 to 900 yards radius, the speed must be reduced by one-eighth; on gradients of 1 in 80 to 1 in 40, and curves of 900 to 650 yards radius, by one-quarter; on curves of 650 to 450 yards radius, by three-eighths; on curves of 450 to 275 yards radius by one-half.

(4) *Maximum Speed with reference to the construction of the Engine.*—This point cannot be treated theoretically. In fixing the highest speed at which a given engine will run easily, account must be taken of all points in construction which tend to produce rolling, jolting, and other irregularties. Such points are: (*a*) the number of coupled wheels, whether 0, 4, or 6; (*b*) the overhang beyond the leading axle, which varies between 1.4 and 2.16 meters; (*c*) the overhang beyond the trailing axle, which varies between nothing and 2.46 meters; (*d*) the stroke, which varies between 0.53 and 0.66 meters; (*e*) the height of the center of gravity of the empty boiler above the rails, which varies between 1.6 and 1.9 meter; (*f*) the distance of the cylinder center-line from the engine center-line, which varies between 0.37 and 1.01 meter; (*g*) the distance of the middle of the cylinder in front of the center of gravity.

In all the above the evil increases as the dimensions increase. In those which follow, it increases as the dimensions diminish. These are: (*h*) the diameter

of the driving wheels, which varies between 1.1 and 1.83 meter; (*i*) the distance of the center-line of the driving-axle brasses from the engine center-line, which varies between 0.57 and 0.96 meter; (*k*) the wheel-base, which varies between 2.0 and 4.57 meters, and which belongs to this class, except on very sharp curves, where it would belong to the former class.

The author discusses these various elements, and arrives at the following formula (for dimensions in meters):

$$v=h(63-2.5\ a-2.2\ c-16.8\ f).$$

The influence of the quantities (*b*), (*d*), (*e*), (*g*), (*i*), and (*h*) is here neglected

From this formula the author has constructed a table, part of which is subjoined.

Class of engine.	Diameter of driving wheel. n	Number of axles coupled. a	Over-hang behind. c	Distance of cylinder axis from loco-axis. f	Maximum speed. v	
	Meters.		Meters.	Meter.	Kilom.	Miles.
Prussian normal passenger engine.	1.730	2	1.585	0.940 (outside)	71.8	44.8
Prussian normal goods engine....	1.830	3	2.645	1.015 "	46.7	29.0
Lilleshall Co. express engine......	2.184	.	0.700	0.380 (inside)	117.5	72.7
Great Northern railway express engine....	2.465		0.500	1.000 (outside)	111.2	69.0
London, Chatham, and Dover Railway express engine......	1.980	2	0.219	0.356 (inside)	102.6	63.4

The general result is that no single formula can be given for the speed of trains, which must be regulated by the lowest of the three limits assigned by the power of the engines, by their construction, and by the conditions of the road. Certain general rules may be laid down. The maximum speed consistent with the curves and gradients should first be determined. If there is no limit in the choice of locomotives, this maximum speed can always be realized; but if there is no stringent necessity for it, it is better to reduce the speed; from considerations of economy in fuel, and in wear and tear. If, however, the class of engines to be used is fixed, their maximum speed, as determined both by their power and their construction, must be found; and the lower of these forms the limit of speed for that particular set of engines. This may, of course, be diminished as before from economical considerations.

THE GEOLOGICAL SURVEY OF ITALY.

From "Nature."

Amongst the numerous signs of renewed life which characterize United Italy, the rapid spread of scientific research must take important rank. In its old homes at the world-famous universities, science, not often entirely neglected, is now once more largely cultivated. New museums are springing up in many of the more important towns, and old ones are everywhere remodelled and enlarged.

Geology has its full share in this scientific revival, as a glance at the annual volumes of the *Geological Record* will show. The establishment of the Geological Society of Italy, to which we referred a fortnight back, will do much to encourage the study of this science.

The Geological Survey of Italy was established in the year 1868; since that date it has gradually developed, and has now accomplished some very important work. The Survey is at present a branch of the Corps of Mining Engineers, but we speak of the service in the phrase best known in England for similar organizations. Since 1870 twelve volumes of the *Bollettino del Ro. Comitato Geologico* have been published. These contain memoirs of various districts, often well illustrated, by members of the Survey or

by other workers whose essays are considered to be worth publishing at the public expense. Probably many memoirs of the latter class will in future find their way into the Geological Society's volume, and the *Bollettino* be more purely official.

The organization of the Survey is somewhat peculiar, and exhibits an amount of divided responsibility which can hardly conduce to its success. The service is partly under the control of a committee of eleven members and a secretary. Of this committee Prof. J. Meneghini of Pisa is president. Seven of the members are nominated by the King, chiefly from amongst the professors at the universities. These give their services gratuitously, only the actual traveling expenses being paid by the Government. The remainder are official members, and some of them are in other ways connected with the Survey.

Each member of the committee has a certain amount of influence in the control of the Survey within his own district; he is supposed to be consulted upon all questions relating to classification, naming of fossils, &c., but the surveyors are really responsible to the official chief of the Survey, M. G. Giordano. Three members of the Scientific Committee take the chief share in the direction. These are Prof. J. Meneghini for Tuscany and Rome; Prof. G. Cemmellaro of Palermo for Sicily; and Prof. J. Capellini of Bologna, whose advice and assistance is always freely at the disposal of the Survey. This dual government might have been desirable in the early stages of the Survey; but now that Italian geology has made such progress, the staff so well trained, and the work so far advanced, it will probably be desirable to reorganize the Survey upon its own basis, giving the sole responsibility to its own official chief.

The surveying staff is part of the Corps of Inspectors of Mines (Ingegneri delle Miniere), the Chief Inspector of which is also the Chief of the Survey. Italy is divided into eight mineral districts—Turin, Milan, Vicenza, Florence, Ancona, Naples, Caltanissetta, Iglesias (Sardinia). The Inspectors of Mines have duties very similar to those of officers holding a like position in England. They visit and report upon mines in cases of accidents, and when any important changes take place in the working of the mines they may be called on for advice. The engineers are chosen from students trained in one of the seven engineering schools of Italy (Turin, Milan, Padua, Bologna, Pisa, Naples, Palermo). They then go for two years to a foreign mining school (Berlin, Freiberg, London, or Paris). Those engineers who are to serve on the Geological Survey Staff receive additional instruction for this purpose. Till now this extra training has generally been obtained from the Geological Survey of England, so that we may regard the Italian Survey as in a certain sense related to our own. Of the officers thus trained in England we may mention M. Anselmo, L. Baldacci, L. Mazzetti, R. Travaglia, De Ferrari, and E. Cortese; to the last named of these we are indebted for much information here given.

The basis of every geological survey must be a good topographical map. The Austrians published a map of a great part of Italy on the scale of 1:75,000; this, however, is not satisfactory. An entirely new topographical survey is now in progress; commenced in Sicily in 1862, it is gradually advancing to the north. The general map of Italy is on the scale of 1:50,000, with contour lines at every 10 meters. The more important mineral districts are published on the scale of 1:25,000, with contour lines at every 5 meters; a very beautiful map of Rome and the surrounding country is now published on the larger scale, as also are Sicily and parts of the N. W. Apennines. There is a smaller map, on the scale of 1:100,000, with contours at every 50 meters.

The small sum voted annually by the Italian Parliament has hitherto been spent in surveying only, and none of the maps have yet been published. They were, however, all exhibited at the Geological Congress at Bologna. It is expected that a larger grant will now be made, and the Survey be placed on a more satisfactory footing. The Central Survey office at Rome, hitherto lodged in the Piazza S. Pietro in Vincoli, will shortly be transferred to the handsome buildings lately erected for the Ministry of Agriculture, Industry and Commerce.

The whole of the Island of Sicily has

been geologically surveyed on the largest scale. This district is of commercial importance from its great yield of sulphur, amounting to 250,000 tons per year. The entire map of Sicily on the scale of 1 : 50,000, was exhibited at Bologna; this is a very beautiful work, and will be of great service to all students of that most interesting region. The Apennines north of Pisa are also surveyed on the scale of 1 : 25.000. This district is of great importance from the marble quarries of Carrara, Massa, &c., the yield of which is 150,000 tons per year. Great uncertainty has long been felt as to the geological age of the Carrara marble; it contains no fossils, and its exact relation to adjacent formations has hitherto been doubtful. It has at various times been referred to many different geological horizons; but now the geological surveyors seem definitely to have fixed its position in the Trias. The mineral districts of Sardinia and the Campagna of Rome have also been surveyed on the scale of 1 : 25,000.

The complete geological survey of a country is a work of some time, and many years must elapse before that of Italy is finished and its maps published. In the meantime the Survey has done a most useful work in preparing a general geological map of Italy on the scale of 1 : 500,000. For this purpose all previously published information has been utilized; the geological notices scattered through various scientific journals, Italian and foreign, have been collected and arranged by M. Giordano and his colleagues. The numerous blanks have been filled up by special researches; and the result is a valuable and beautiful map, which will shortly be published. It was desired to issue a reduction of this on the scale of 1 : 1,000,000, but as no topographical map on this scale exists, a French map was adopted, engraved on the scale of 1 : 1,111,111 (or 1 decim. to a degree). This map was corrected where necessary, and was published in time for the meeting of the Congress at Bologna. The map is issued in two editions, one with hill-shading and one without. The only general map previously published was that of Collegno, in 1846, on the scale of 1 : 2,000,000. A glance at the two maps will show the immense advance which has been made in our knowledge of Italian geology since that date.

The map in question is colored in accordance with the scheme recommended by the Italian Map Committee of the International Geological Congress. The Italian Committee (like the English) prefer to retain some shades of red for the Trias. The Congress, however, chiefly influenced by the wishes of Germany, proposes to color this violet, as the natural base of the secondary series; the Jurassic beds being colored blue. The Italian Survey is desirous of adopting for its future maps the scheme of coloring upon which the Congress may decide. The Indian Survey also, being now about to publish a connected series of maps, wishes, if possible, to do the same. We have little doubt that the geological map of Europe, now being prepared by the Map Committee of the Congress, will be so drawn up and colored as to form a scheme of colors which can, with only small modifications, be adopted by all.

From the report of the United States Commissioner of Agriculture it appears that 2,500,000 packages of seeds have been distributed, and 260,000 copies of special reports printed by the department. The statistical division estimates the following as the yield of 1882:—Corn, 1,635,000,000 bushels; wheat, 400,000,000 bushels; oats, 470,000,000 bushels; barley, 45,000,000 bushels; rye, 20,000,000 bushels; and buckwheat, 12,000,000 bushels.

For waterproofing brick walls the following has been given. Dissolve soft parafine wax in benzoline spirit in the proportion of about 1 part of the former to 4 or 5 parts of the latter by weight. Into a tin or metallic keg place 1 gallon of benzoline spirit, then mix 1½ lb. or 2 lbs. wax, and when well hot pour into the spirit. Apply the solution to the walls whilst warm with a whitewash brush. To prevent the solution from chilling, it is best to place the tin in a pail of warm water, but on no account should the spirit be brought into the house, or near to a light, or a serious accident might occur.

HYDRAULIC TABLES BASED ON KUTTER'S FORMULA.

By P. J. FLYNN, C. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE tables given below are intended to facilitate the calculation of velocities, discharges, slopes and dimensions of sewers and other conduits, and their use will effect a great saving of time; as, for instance, instead of calculating the velocity and discharge by the use of a troublesome formula, the same result, practically, will be arrived at by taking the product of two factors given in the tables.

Kutter's formula is a complicated equation, and in its general form is:

$$v=c\sqrt{rs} \text{ in which}$$

$$c=\left\{\frac{41.6+\frac{1.811}{n}+\frac{.00281}{s}}{1+\left((41.6+\frac{.00281}{s})\times\frac{n}{\sqrt{r}}\right)}\right\}$$

In this and the following formulæ,

v=mean velocity in feet per second.

s=fall of water surface (h) in any distance (l) divided by that distance= $\frac{h}{l}$=sine of slope.

r=hydraulic mean depth=area of cross section of water divided by wetted perimeter=$\frac{a}{p}$.

d=diameter of circular channel.

a=area of cross section of water.

p=wetted perimeter.

Q=discharge in cubic feet per second.

n=the natural coefficient depending on the nature of the bed, that is, the lining of the channel over which the water flows, which throughout this article, and in the preparation of the tables, has been taken at .015.

Mr. J. C. Trautwine, in his *Engineer's Pocket Book*, states that, "In consideration of the rough character of sewer brickwork generally," he has taken n= .015 in Kutter's formula when he calculated the velocities in sewers.

Mr. R. Hering, in a paper read before the American Society of Civil Engineers in 1878 on the velocity and discharge of sewers, gave:

"n=.015" for "foul and slightly tuberculated iron; cement and terra cotta pipes with imperfect joints, and in bad order; well dressed stonework and second-class brickwork." The tables do not apply to channels with smooth or plastered surfaces. They are intended to apply only to sewers, conduits and other channels whose surfaces exposed to the flow of water are of second-class brickwork, or have surfaces of other material equally rough, such, for instance, as those given above from Mr. Hering's paper.

The general form of Kutter's formula is:

$$v=c\sqrt{rs}=c\sqrt{r}\times\sqrt{s} \quad . \quad . \quad . \quad . \quad (1).$$

from which

$$c\sqrt{r}=\frac{v}{\sqrt{s}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

$$\sqrt{s}=\frac{v}{c\sqrt{r}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

$$s=\left(\frac{v}{c\sqrt{r}}\right)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

$$Q=av=ac\sqrt{r}\times\sqrt{s} \quad . \quad . \quad . \quad . \quad (5).$$

from which

$$a=\frac{Q}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

$$ac\sqrt{r}=\frac{Q}{\sqrt{s}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

$$\sqrt{s}=\frac{Q}{ac\sqrt{r}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

$$s=\left(\frac{Q}{ac\sqrt{r}}\right)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

The values of $c\sqrt{r}$ and $ac\sqrt{r}$ for 124 diameters are given in table 1, and the values of $\sqrt{s}$ for 557 slopes are given in table 2. It will then be seen that a large range of channels numbering 69068 are included in these tables. The velocity is found by the product of two factors $c\sqrt{r}$ and $\sqrt{s}$, and in a similar way the discharge is found by the product of the two factors $ac\sqrt{r}\ \sqrt{s}$.

In Kutter's formula given above the value of c is found from an equation involving the value of r, n and s, so that any change in the value of s would cause a change in the value of c, but as the influence of s on the value of c is not very marked in such slopes as are usually adopted for sewers and conduits, the value of the coefficient has been calculated for one slope, that of 1 in 1000 or $s=.001$. This value of the coefficient is *practically constant* for all values of s with a steeper slope than 1 in 1000, and as sewers are generally designed with steeper slopes than 1 in 1000, the tables are well adapted to facilitate the calculations. For flatter slopes than 1 in 1000 up to even 2 feet per mile, or 1 in 2640, the tables give results showing a maximum error in the case of a sewer 2 feet in diameter of less than 2 per cent., and in the case of a sewer 8 feet in diameter less than ½ per cent.; therefore, for all practical purposes, the tables are sufficiently accurate.

The hydraulic mean depth of a cylindrical conduit flowing full is equal to one-fourth of the diameter.

The *mean velocity* in circular sewers and conduits is the same when running half full as when running full.

Application and Use of the Tables.

To find the mean velocity in feet per second and the discharge in cubic feet per second.

Example 1.—A circular brick sewer has a diameter of 3 feet and a fall of 1 in 500. What is its mean velocity in feet per second and also its discharge in cubic feet per second?

By formula (1) $v=c\sqrt{r}\times\sqrt{s}$.

In column 4 of table 1 and opposite 3 feet diameter the value $c\sqrt{r}$ is found equal to 80.77, and in table 2 opposite 1 in 500 the value of $\sqrt{s}$ is found equal to .044721; substituting these values in equation, we have:

$v=80.77\times.044721$

$=3.61$ feet per second the mean velocity.

By formula (5) $Q=av=a\times3.61$, but by table 2 the area of a sewer 3 feet in diameter $=7.068$; substitute this value in equation and

$Q=7.068\times3.61$

$=25.52$, the discharge in cubic feet per second.

Again, as a check,

By formula (5) $Q=ac\sqrt{r}\times\sqrt{s}$.

In column 4 of table, and opposite 3 feet diameter the value of $ac\sqrt{r}$ is given as 570.9, substituting this value and also the value of $\sqrt{s}$, as found above, in equation we have

$$Q=570.9\times.044721=25.53$$

cubic feet per second the discharge, which is the same as already found above.

Example 2.—*To find the diameter.*—(d). The grade (s) of a sewer is to be 1 in 480, and its mean velocity (v) 4 feet per second. What is the required diameter? By formula (2),

$$c\sqrt{r}=\frac{v}{\sqrt{s}}$$

In column 3 of table 2 we find for a slope of 1 in 480 that $\sqrt{s}$ is equal to .045644. Substitute this in equation, and also the value of v already given, and

$$c\sqrt{r}=\frac{4}{.045644}=87.63.$$

Now look in column 4 of table 1 for the nearest value of $c\sqrt{s}$ to this which we find to be 87.15, opposite 3 feet 4 inches in diameter, which is the diameter required.

Example 3.—*To find the grade of Sewer.*—A sewer 2 feet 6 inches diameter is to have a velocity when running full or half full of not more than 3½ feet a second. What should its grade be?

By formula (3) $\sqrt{s}=\frac{v}{c\sqrt{r}}$.

In column 4 of table 1 find opposite the diameter 2 feet 6 inches that $c\sqrt{r}$ is equal to 70.74. Substitute this value and also the value of v already given in equation, and $\sqrt{s}=\frac{3.5}{70.74}=.049477$. Now look out the nearest the value of $\sqrt{s}$ to this in column 3 of table 2, which we find to be .049507, opposite a slope of 1 in 408, which is the required grade.

To find the grade of sewer when the grade is not given in Table 2.

Example 4.—A sewer having a diameter of 1 foot 9 inches is to have a velocity of 2¼ feet per second. What is its required grade?

By formula (3) $\sqrt{s}=\frac{v}{c\sqrt{r}}$. Look out

the value of $c\sqrt{r}$ for 1 foot 9 inches diameter in Table 1, and it will be found to be 54.29. Substitute this value and also the value of v already given in equation, and

$$\sqrt{s}=\frac{2.25}{54.29}=.041444.$$

On looking for this value on Table 1, we find that table does not extend to a flatter slope than that whose $\sqrt{s}=042254$. Therefore square each side of the equation:

$$\sqrt{s}=.041444 \text{ and we get}$$
$$s=.001717$$

and $\frac{1}{.001717}=582$, therefore the slope is 1 in 582.

To find the diameter (d).

Example 5.—A sewer is to discharge 9 cubic per second and its grade is to be 1 in 200. What is its diameter to be?

By formula (7) $ac\sqrt{r}=\frac{Q}{\sqrt{s}}$. In the third column of table 2 and opposite 1 in 200 the value of $\sqrt{s}$ is found to be .070710. Substitute this value and the discharge already given in equation, and we have $ac\sqrt{r}=\frac{9}{.070710}=127.28$. In column 5 of Table 1, the value of $ac\sqrt{r}$ nearest to this we find to be 130.58, opposite to which is the diameter of 1 foot 9 inches, which is the diameter required.

To find the grade or slope of sewer. (s).

Example 6.—A sewer 6 feet in diameter is required to discharge 180 cubic feet of water per second. What should be its slope?

By formula (8) $\sqrt{s}=\frac{Q}{ac\sqrt{r}}$. In column 5 of Table 1 and opposite 6 feet in diameter the valve of $ac\sqrt{r}$ is found equal to 3702.3. Substitute this and also the value of Q in equation, and we have

$\sqrt{s}=\frac{180}{3702.3}=.048618$. Now in column 4 of Table 2 look out the number nearest to this, which will be found to be .048621 opposite a slope 1 in 423, therefore the required grade is 1 in 423.

To find diameters in a series of sewers with increasing discharge.

Example 7.—A circular sewer has for 500 feet in length to discharge 10 cubic feet per second, then for 600 feet more has to discharge 12 cubic feet feet per second, and again for 700 feet, farther on 15 cubic feet per second. The total fall available is 5 feet. What is the required diameter and fall of each section? The total length is 1800 feet and $\frac{5}{1800}=.002777=s$ and $\sqrt{.002777}=.052705=\sqrt{s}$.

By formula (7) $ac\sqrt{r}=\frac{Q}{\sqrt{s}}$.

In this equation substitute values of Q and s for each section and find the corresponding diameters, which will be the diameters required.

$$\left.\begin{aligned}ac\sqrt{r}&=\frac{10}{.052705}=189.7\\ ac\sqrt{r}&=\frac{12}{.052705}=227.7\\ ac\sqrt{r}&=\frac{15}{.052705}=284.6\end{aligned}\right\}\text{Opposite which in Table 1 is}\left\{\begin{aligned}&\text{diam.}2'-0''\\ &\text{diam.}2'-2''\\ &\text{diam.}2'-4''\end{aligned}\right.$$

Now $s=\frac{h}{l}$ $\therefore$ $h=sl$, therefore the

Fall of first section $=sl=.002777 \times 500$ .... $=1.39$ ft.
Fall of second section $=sl=.002777 \times 600$ .... $=1.67$ "
Fall of third section $=sl=.002777 \times 700$ .... $=1.95$ "

Total fall .... 5.00 ft.

We have, therefore,

1st section,	diameter	2′−0″,	fall 1.39 ft.
2d	"	2′−2″	" 1.67 "
3d	"	2′−4″	" 1.94 "

To find velocity and discharge of trapezoidal channel.

Example 8.—A trapezoidal channel lined with brickwork, 6 feet wide at bottom and with side slopes of 1 to 1, has 2 feet in depth of water and a grade of 1 in 160. What is its velocity and discharge per second?

Area $(a)=\frac{6+10}{2}\times 2=16$ square feet.

Wetted perimeter $(p)=2\times\sqrt{2^2\times 2^2}+6=11.66$ feet.

$\therefore$ Hydraulic mean depth

$$(r)=\frac{d}{p}=\frac{16}{11.66}=1.372.$$

In column 3 of Table 1 look out the

nearest value of r to this which we find to be 1.375, and corresponding to this we find $c\sqrt{r}$ equal to 123.5. In Table 2 for a slope of 1 in 160 the value of $\sqrt{s}$ is found to be $=.079057$.

Now by formula (1) $v=c\sqrt{r}\times\sqrt{s}$ and
" " " (5) $Q=av$ substituting the values above found of the factors, then $v=123.5\times079057=9.76$ feet per second and $Q=16\times9.76=156.2$ cubic feet per second. therefore the mean velocity is equal to 9.76 feet per second and the discharge equal to 156.2 cubic feet per second.

To find the dimensions of a circular sewer to replace a rectangular brick channel.

Example 9. An open brick channel 5 feet wide at bottom, with vertical sides, has a depth of water in floods of 3 feet and a slope of 1 in 520. It is intended to substitute for it a circular sewer whose mean velocity flowing full shall be about 5 ft. per second. What should be the diameter and grade of the new circular sewer flowing full?

In Table 2 the $\sqrt{s}$ for a grade of 1 in $520=.043853$.

Area of rectangular channel $(a)=5\times3=15$ sq. ft. Wetted perimeter $(p)=3+5+3=11$ feet. $\therefore$ Hydraulic mean depth $(r)=\frac{d}{p}=\frac{15}{11}=1.364$. In Table 1 find corresponding to this hydraulic mean depth the nearest $c\sqrt{r}$, which is 123.5.

By formula (5) $Q=a\times c\sqrt{r}\times\sqrt{s}$ substitute the values found above, of the factors in right hand side of equation, and $Q=15\times123.5\times.043853=81.24$ cubic feet per second, the discharge from the rectangular channel.

We have now to find the diameter and grade of a circular sewer to convey this quantity of water with a velocity not greater than 5 feet per second.

By formula (6) $a=\frac{Q}{v}$ substitute values

$a=\frac{81.26}{5}=16.252$ square feet=area of circular sewer. In column 2 of Table 1 we find the area nearest in value to this $=16.499$, and the corresponding diameter equal to 4 feet 7 inches, and at the same time find the value of the corresponding $ac\sqrt{r}$ which is 1796.5.

By formula (8) $\sqrt{s}=\frac{Q}{ac\sqrt{r}}$ substitute values of Q and $ac\sqrt{r}$ found above and

$$\sqrt{s}=\frac{81.26}{1796.5}=045232.$$

In Table 2 we find the grade corresonding to this equal to 1 in 489, therefore the diameter of circular sewer is 4 feet 7 inches, and the grade 1 in 489.

TABLE I.—GIVING VALUES OF a AND r AND ALSO THE FACTORS $c\sqrt{r}$ AND ALSO $ac\sqrt{r}$.

These factors are to be used only where the value of N, that is the coefficient of roughness of lining of channel=.015 as in second class or rough-faced brickwork.

$v=c\sqrt{r}\times\sqrt{s}.\quad Q=av=ac\sqrt{r}\times\sqrt{s}.$

d = diameter in ft. in.	a = area in square ft.	r=hydraulic mean depth.	$c\sqrt{r}$	$ac\sqrt{r}$	d = diameter in ft. in.	a = area in square ft.	r=hydraulic mean depth.	$c\sqrt{r}$	$ac\sqrt{r}$
0 5	0.136	0.104	17.36	2.3615	1 6	1.767	0.375	48.38	85.496
0 6	0.196	0.125	20.21	3.9604	1 7	1.969	0.396	50.40	99.242
0 7	0.267	0.146	22.95	6.1268	1 8	2.182	0.417	52.45	114.46
0 8	0.349	0.167	25.56	8.9194	1 9	2.405	0.437	54.29	130.58
0 9	0.442	0.187	28.10	12.421	1 10	2.640	0.458	56.29	148.61
0 10	0.545	0.208	30.52	16.633	1 11	2.885	0.479	58.20	167.90
0 11	0.660	0.229	33.03	21.798	2 0	3.142	0.500	60.08	188 77
1 0	0.785	0.250	35.40	27.803	2 1	3.409	0 521	61.95	211.20
1 1	0.922	0.271	37.60	34.664	2 2	3.687	0.542	63 72	234.94
1 2	1 069	0.292	39.85	42.602	2 3	3.976	0.562	65.51	260.47
1 3	1.227	0.312	42.05	51.600	2 4	4.276	0.583	67.32	287.87
1 4	1.396	0 333	44.19	61.685	2 5	4.587	0.604	69.02	316.59
1 5	1.576	0.354	46.36	73.066	2 6	4.909	0.625	70.74	347.28

TABLE I.—GIVING VALUES OF a AND r AND ALSO THE FACTORS $c\sqrt{r}$ AND $ac\sqrt{r}$ FOR CORRESPONDING DIAMETERS IN COLUMN I.

$$v=c\sqrt{r}\times\sqrt{s}. \quad Q=av=ac\sqrt{r}\times\sqrt{s}.$$

d = diameter in ft.	in.	a = area in square ft.	r=hydraulic mean depth.	$c\sqrt{r}$	$ac\sqrt{r}$	d = diameter in ft.	in.	a = area in square ft.	r=hydraulic mean depth.	$c\sqrt{r}$	$ac\sqrt{r}$
2	7	5.241	0.646	72.59	380.46	8	0	50.266	2.000	158.7	7978.3
2	8	5.585	0.667	74.27	414.81	8	3	53.456	2.062	162.0	8658.8
2	9	5.939	0.687	75.98	451.23	8	6	56.745	2.125	165.3	9377.9
2	10	6.305	0.708	77.56	488.99	8	9	60.132	2.187	168.4	10128
2	11	6.681	0.729	79.16	528.85	9	0	63.617	2.250	171.6	10917
3	0	7.068	0.750	80.77	570.90	9	3	67.201	2.312	174.7	11740
3	1	7.466	0.771	82.39	615.14	9	6	70.882	2.375	177.7	12594
3	2	7.875	0.792	84.03	661.77	9	9	74.662	2.437	180.7	13489
3	3	8.295	0.812	85.54	709.56	10	0	78.540	2.500	183.7	14426
3	4	8.726	0.833	87.15	760.44	10	3	82.516	2.562	186.7	15406
3	5	9.169	0.854	88.61	812.38	10	6	86.590	2.625	189.5	16412
3	6	9.621	0.875	90.11	866.91	10	9	90.763	2.687	192.4	17462
3	7	10.084	0.896	91.60	923.70	11	0	95.033	2.750	195.2	18555
3	8	10.559	0.917	93.11	983.11	11	3	99.402	2.812	198.1	19694
3	9	11.044	0.937	94.62	1045.0	11	6	103.87	2.875	201.0	20879
3	10	11.541	0.958	96.15	1109.6	11	9	108.43	2.937	203.7	22093
3	11	12.048	0.979	97.55	1175.2	12	0	113.10	3.000	206.5	23352
4	0	12.566	1.000	99.10	1245.3	12	3	117.86	3.062	209.2	24658
4	1	13.096	1.021	100.5	1315.8	12	6	122.72	3.125	212.0	26012
4	2	13.635	1.042	102.0	1390.8	12	9	127.68	3.187	214.6	27399
4	3	14.186	1.062	103.4	1466.7	13	0	132.73	3.250	217.4	28850
4	4	14.748	1.083	104.8	1545.7	13	3	137.88	3.312	220.0	30330
4	5	15.321	1.104	106.2	1627.0	13	6	143.14	3.375	222.6	31860
4	6	15.904	1.125	107.6	1711.4	13	9	148.49	3.437	225.2	33441
4	7	16.499	1.146	108.9	1796.5	14	0	153.94	3.500	227.8	35073
4	8	17.104	1.167	110.3	1886.8	14	3	159.48	3.562	230.0	36736
4	9	17.721	1.187	111.6	1977.7	14	6	165.13	3.625	232.9	38454
4	10	18.348	1.208	113.0	2074.1	14	9	170.87	3.687	235.4	40221
4	11	18.986	1.229	114.4	2172.9	15	0	176.72	3.750	237.9	42040
5	0	19.635	1.250	115.7	2272.7	15	3	182.65	3.812	240.5	43931
5	1	20.295	1.271	117.1	2376.7	15	6	188.69	3.875	242.8	45820
5	2	20.966	1.292	118.4	2482.0	15	9	194.83	3.937	245.3	47792
5	3	21.648	1.312	119.7	2590.5	16	0	201.06	4.000	247.8	49823
5	4	22.340	1.333	121.0	2702.1	16	3	207.40	4.062	250.3	51904
5	5	23.044	1.354	122.2	2816.7	16	6	213.83	4.125	252.7	54056
5	6	23.758	1.375	123.5	2934.8	16	9	220.35	4.187	254.9	56171
5	7	24.484	1.396	124.8	3056.4	17	0	226.98	4.250	257.2	58387
5	8	25.220	1.417	126.0	3177.3	17	3	233.71	4.312	259.7	60700
5	9	25.967	1.437	127.3	3305.6	17	6	240.53	4.375	261.9	62999
5	10	26.725	1.558	128.6	3436.3	17	9	247.45	4.437	264.4	65428
5	11	27.494	1.479	129.7	3566.6	18	0	254.47	4.500	266.6	67839
6	0	28.274	1.500	131.0	3702.3	18	3	261.59	4.562	268.9	70346
6	3	30.680	1.562	134.6	4130.3	18	6	268.80	4.625	271.3	72916
6	6	33.183	1.625	138.3	4588.3	18	9	276.12	4.687	273.5	75507
6	9	35.785	1.687	141.8	5074.7	19	0	283.53	4.750	275.8	78201
7	0	38.485	1.750	145.3	5591.6	19	3	291.04	4.812	278.0	80216
7	3	41.283	1.812	148.7	6136.8	19	6	298.65	4.875	280.2	83686
7	6	44.179	1.875	152.0	6717.0	19	9	306.36	4.937	282.4	86526
7	9	47.173	1.937	155.5	7333.5	20	0	314.16	5.000	284.6	89423

TABLE 2—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
560	.001785714	.042258	510	.001960784	.044281
559	.001788909	.042295	509	.001964637	.044324
558	.001792115	.042333	508	.001968504	.044368
557	.001795332	.042371	507	.001972387	.044412
556	.001798561	.042410	506	.001976285	.044455
555	.001801802	.042448	505	.001980198	.044499
554	.001805054	.042486	504	.001984127	.044544
553	.001808318	.042524	503	.001988072	.044588
552	.001811594	.042563	502	.001992032	.044632
551	.001814882	.042601	501	.001996008	.044677
550	.001818182	.042640	500	.002000000	.044721
549	.001821494	.042679	499	.002004008	.044766
548	.001824817	.042718	498	.002008032	.044811
547	.001828154	.042757	497	.002012072	.044856
546	.001831502	.042796	396	.002016128	.044901
545	.001834862	.042835	495	.002020202	.044947
544	.001838235	.042874	494	.002024291	.044992
543	.001841621	.042914	493	.002028398	.045037
542	.001845018	.042953	492	.002032520	.045085
541	.001848429	.042993	491	.002036660	.045129
540	.001851852	.043033	490	.002040816	.045175
539	.001855288	.043073	489	.002044990	.045222
538	.001858736	.043113	488	.002049180	.045268
537	.001862197	.043153	487	.002053388	.045314
536	.001865672	.043193	486	.092057613	.045361
535	.001869159	.043234	485	.002061856	.045407
534	.001872659	.043274	484	.002066116	.045454
533	.001876173	.044315	483	.002070393	.045502
532	.001879699	.043355	482	.002074689	.045549
531	.001883239	.043396	481	.002079002	.045596
530	.001886792	.043437	480	.002083333	.045644
529	.001890359	.043478	479	.002087683	.045691
528	.001893939	.043519	478	.002092050	.045739
527	.001897533	.043561	477	.002096436	.045787
626	.001901141	.043602	476	.002100840	045835
525	.001904762	.043644	475	.002105263	.045883
524	.001908397	.043685	474	.002109705	.045932
523	.001912046	.043727	473	.002114165	.045980
422	.001915709	.043769	472	.002118644	.046029
521	.001919386	.043811	471	.002123142	.046077
520	.001923077	.043853	470	.002127660	.046126
519	.001926782	.043895	469	.002132196	.046176
518	.008930502	.043937	468	.001136752	.046225
517	.001944246	.043979	467	.002141328	.046274
516	.001937984	.044022	466	.002145923	.046324
515	.001941748	.044065	465	.002150538	.046374
514	.001945525	.044108	464	.002155172	.046424
513	.001949318	.044151	463	.002159827	.046474
512	.001953125	.044194	462	.002164502	.046524
511	.001956947	.044237	461	.002169197	.046575

TABLE 2 (*Continued*).—GIVING VALUES OF s AND $\sqrt{s}$.

k :- sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
460	.002173913	.046625	410	.002439024	.049387
459	.002178649	.046676	409	.002444988	.049447
458	.002183406	.046726	408	.002450980	.049507
457	.002188184	.046778	407	.002457002	.049568
456	.002192982	.046829	406	.002463054	.049629
455	.002197802	.046880	405	.002469136	.049690
454	.002202643	.046932	404	.002475248	.049752
453	.002207506	.046984	403	002481390	.049814
452	.002212389	.047036	402	.002487562	.049876
451	.002217295	.047088	401	.002493766	.049938
450	.002222222	.047140	400	.002500000	.050000
449	.002227194	.047193	399	.002506266	.050062
448	.002232143	.047245	398	.002512563	.050125
447	.002237136	.047298	397	.002518892	.050188
446	.002242152	.047351	396	.002525253	.050252
445	.002247191	.047404	395	.002531646	.050315
444	.002252252	.047458	394	.002538071	.050379
443	.002257336	.047511	393	.002544529	.050443
442	.002262443	.047565	392	.002551020	.050507
441	.002267574	.047619	391	.002557545	.050572
440	.002272727	.047673	390	.002564103	.050637
439	.002277904	.047728	389	.002570694	.050702
438	.002283105	.047782	388	.002577320	.050767
437	.002288330	.047836	387	.002583979	.050833
436	.002293578	.047891	386	.002590674	.050899
435	.002298851	.047946	385	.002597403	.050965
434	.002304147	.048001	384	.002604167	.051031
433	.002309469	.048057	383	.002610966	.051097
432	.002314815	.048113	382	.002617801	.051164
431	.002320186	.048168	381	.002624672	.051231
430	.002325581	.048224	380	.002631579	.051299
429	.002331002	.048280	379	.002638522	.051366
428	.002336449	.048337	378	.002645503	.051434
427	.002341920	.048393	377	.002652520	.051502
426	.002347418	.048450	376	.002659574	.051571
425	.002352941	.048507	375	.002666667	.051640
424	.002358491	.048564	374	.002673797	.051709
423	.002364066	.048621	373	.002680965	.051778
422	.002369668	.048679	372	.002688172	.051847
421	.002375297	.048737	371	.002695418	.051917
420	.002380952	.048795	370	.002702703	.051988
419	.002386635	.048853	369	.002710027	.052060
418	.002392344	.048911	368	.002717391	.052129
417	.002398082	.048970	367	.002724796	.052199
416	.002403846	.049029	366	.002732240	.052270
415	.002409639	.049088	365	.002739726	.052342
414	.002415459	.049147	364	.002747253	.052414
413	.002421308	.049207	363	.002754821	.052486
412	.002427184	.049266	362	.002762431	.052559
411	.002433090	.049326	361	.002770083	.052632

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=full of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=side of slope.	$\sqrt{s}$.
360	.002777778	.052705	310	.003225806	.056796
359	.002785515	.052778	309	.003236246	.056888
358	.002793296	.052851	308	.003246753	.056980
357	.002801120	.052925	307	.003257329	.057073
356	.002808989	.052999	306	.003267974	.057166
355	.002816901	.053074	305	.003278689	.057260
354	.002824859	.053149	304	.003289474	.057354
353	.002832861	.053224	303	.003300330	.057449
352	.002840909	.053300	302	.003311258	.057544
351	.002849003	.053376	301	.003322259	.057639
350	.002857143	.053452	300	.003333333	.057735
349	.002865330	.053529	299	.003344482	.057831
348	.002873563	.053606	298	.003355705	.057929
347	.002881844	.053683	297	.003367003	.058026
346	.002890171	.053760	296	.003378378	.058124
345	.002898551	.053838	295	.003389831	.058222
344	.002906977	.053916	294	.003401361	.058321
343	.002915452	.053995	293	.003412969	.058420
342	.002923977	.054074	292	.003424658	.058520
341	.002932551	.054153	291	.003436426	.058621
340	.002941176	.054232	290	.003448276	.058722
339	.002949853	.054312	289	.003460208	.058824
338	.002958580	.054393	288	.003472222	.058926
337	.002967359	.054474	287	.003484321	.059028
336	.002976190	.054555	286	.003496503	.059131
335	.002985075	.054636	285	.003508772	.059235
334	.002994012	.054717	284	.003521127	.059339
333	.003003003	.054799	283	.003533569	.059444
332	.003012048	.054882	282	.008546099	.059549
331	.003021148	.054965	281	.003558719	.059655
330	.003030303	.055048	280	.003571429	.059761
329	.003039514	.055132	279	.003584229	.059868
328	.003048780	.055216	278	.003597122	.059976
327	.003058104	.055300	277	.003610108	.060084
326	.003067485	.055385	276	.003623188	.060193
325	.003076923	.055470	275	.003636364	.060302
324	.003086420	.055556	274	.003649635	.060412
323	.003095975	.055641	273	.003663004	.060523
322	.003105590	.055728	272	.003676471	.060634
321	.003115265	.055815	271	.003690037	.060746
320	.003125000	.055902	270	.003703704	.060858
319	.003134796	.055989	269	.003717472	.060971
318	.003144654	.056077	268	.003731343	.061085
317	.003154574	.056165	267	.003745319	.061199
316	.003164557	.056254	266	.003759398	.061314
315	.003174603	.056344	265	.003773585	.061430
314	.003184713	.056433	264	.003787879	.061546
313	.003194888	.056523	263	.003802281	.061662
312	.003205128	.056614	262	.003816794	.061780
311	.003215434	.056705	261	.003831418	.061899

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
260	.003846154	.062018	210	.004761905	.069007
259	.003861004	.062137	209	.004784689	.069172
258	.003875969	.062257	208	.004807692	.069338
257	.003891051	.062378	207	.004830918	.069505
256	.003906250	.062500	206	.004854369	.069673
255	.003921569	.062622	205	.004878049	.069843
254	.003937008	.062746	204	.004901961	.070014
253	.003952569	.062870	203	.004926108	.070186
252	.003968254	.062994	202	.004950495	.070359
251	.003984064	.063119	201	.004975124	.070534
250	.004000000	.063246	200	.005000000	.070710
249	.004016064	.063372	199	.005025126	.070888
248	.004032258	.063500	198	.005050505	.071067
247	.004048583	.063629	197	.005076142	.071247
246	.004065041	.063758	196	.005102041	.071429
245	.004081623	.063888	195	.005128205	.071612
244	.004098361	.064018	194	.005154639	.071796
243	.004115226	.064150	193	.005181347	.071982
242	.004132231	.064283	192	.005208333	.072169
241	.004149378	.064416	191	.005235602	.072357
240	.004166667	.064549	190	.005263158	.072548
239	.004184100	.064685	189	.005291005	.072739
238	.004201681	.064820	188	.005319149	.072932
237	.004219409	.064957	187	.005347594	.073127
236	.004237288	.065094	186	.005376344	.073324
235	.004255319	.065233	185	.005405405	.073521
234	.004273504	.065372	184	.005434783	.073721
233	.004291845	.065512	183	.005464481	.073922
232	.004310345	.065653	182	.005494505	.074125
231	.004329004	.065795	181	.005524862	.074329
230	.004347826	.065938	180	.005555556	.074536
229	.004366812	.066082	179	.005586592	.074744
228	.004385965	.066227	178	.005617978	.074953
227	.004405286	.066372	177	.005649718	.075164
226	.004424779	.066519	176	.005681818	.075378
225	.004444444	.066667	175	.005714286	.075593
224	.004464286	.066815	174	.005747126	.075810
223	.004484305	.066965	173	.005780347	.076029
222	.004504505	.067116	172	.005813953	.076249
221	.004524887	.067267	171	.005847953	.076472
220	.004545455	.067419	170	.005882353	.076697
219	.004566210	.067574	169	.005917160	.076923
218	.004587156	.067729	168	.005952381	.077152
217	.004608295	.067885	167	.005988024	.077382
216	.004629630	.068041	166	.006024096	.077615
215	.004651163	.068199	165	.006060606	.077850
214	.004672897	.068358	164	.006097561	.078087
213	.004694836	.068519	163	.006134969	.078326
212	.004716981	.068680	162	.006172840	.078568
211	.004739336	.068843	161	.006211180	.078811

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
160	.006250000	.079057	110	.009090909	.095346
159	.006289308	.079305	109	.009174312	.095783
158	.006329114	.079556	108	.009259259	.096225
157	.006369427	.079809	107	.009345794	.096674
156	.006410256	.080065	106	.009433962	.097129
155	.006451613	.080322	105	.009523810	.097590
154	.006493506	.080582	104	.009615385	.098058
153	.006535948	.080845	103	.009708738	.098533
152	.006578947	.081111	102	.009803922	.099015
151	.006622517	.081379	101	.009900990	.099504
150	.006666667	.081650	100	.010000000	.100000
149	.006711409	.081923	99	.010101010	.100504
148	.006756757	.082199	98	.010204082	.101015
147	.006802721	.082479	97	.010309278	.101535
146	.006849315	.082760	96	.010416667	.102062
145	.006896552	.083046	95	.010526316	.102598
144	.006944444	.083333	94	.010638298	.103142
143	.006993007	.083624	93	.010752688	.103695
142	.007042254	.083918	92	.010869565	.104257
141	.007092199	.084215	91	.010989011	.104828
140	.007142857	.084516	90	.011111111	.105409
139	.007194245	.084819	89	.011235955	.106000
138	.007246377	.085126	88	.011363636	.106600
137	.007299270	.085436	87	.011494253	.107211
136	.007352941	.085749	86	.011627907	.107833
135	.007407407	.086066	85	.011764706	.108465
134	.007462687	.086387	84	.011904762	.109109
133	.007518797	.086711	83	.012048193	.109764
132	.007575758	.087039	82	.012195122	.110431
131	.007633588	.087370	81	.012345679	.111111
130	.007692308	.087706	80	.012500000	.111803
129	.007751938	.088045	79	.012658228	.112509
128	.007812500	.088388	78	.012820513	.113228
127	.007874016	.088736	77	.012987013	.113961
126	.007836508	.089087	76	.013157895	.114708
125	.008000000	.089442	75	.013333333	.115470
124	.008064516	.089803	74	.013513514	.116248
123	.008130081	.090167	73	.013698630	.117041
122	.008196721	.090536	72	.013888889	.117851
121	.008264463	.090909	71	.014084507	.118678
120	.008333333	.091287	70	.014285714	.119524
119	.008403361	.091669	69	.014492754	.120386
118	.008474576	.092057	68	.014705882	.121286
117	.008547009	.092450	67	.014925353	.122169
116	.008620690	.092848	66	.015151515	.123091
115	.008695652	.093250	65	.015384615	.124035
114	.008771930	.093659	64	.015625000	.125000
113	.008849558	.094072	63	.015873016	.125988
112	.008928571	.094491	62	.016129032	.127000
111	.009009009	.094916	61	.016393443	.128037

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
60	.016666667	.129100	31	.032258065	.179605
59	.016949153	.130189	30	.033333333	.182574
58	.017241379	.131305	29	.034452759	.185695
57	.017543860	.132453	28	.035714286	.188982
56	.017850143	.133630	27	.037037037	.192450
55	.018181818	.134839	26	.038461538	.196116
54	.018518519	.136085	25	.040000000	.200000
53	.018867925	.137361	24	.041666667	.204124
52	.019230769	.138676	23	.043478261	.208514
51	.019607843	.140028	22	.045454545	.213200
50	.020000000	.141421	21	.047619048	.218218
49	.020408163	.142857	20	.050000000	.223607
48	.020833333	.144337	19	.052631579	.229416
47	.021276600	.145865	18	.055555555	.235702
46	.021739130	.147444	17	.058823529	.242536
45	.022222222	.149071	16	.062500000	.250000
44	.022727273	.150756	15	.066666667	.258199
43	.023255814	.152499	14	.071428571	.267261
42	.023809524	.154303	13	.076923077	.277350
41	.024390244	.156174	12	.083333333	.288675
40	.025000000	.158114	11	.090909090	.301511
39	.025641026	.160125	10	.100000000	.316228
38	.026315789	.162221	9	.111111111	.333333
37	.027027027	.164399	8	.125000000	.353553
36	.027777778	.166667	7	.142857143	.377978
35	.028571429	.169031	6	.166666660	.408248
34	.029411765	.171499	5	.200000000	.447214
33	.030303030	.174077	4	.250000000	.500000
32	.031250000	.176777			

ON TIDES AND TIDAL SCOUR.*

By Mr. JOSEPH BOULT, C. E.

From "The Engineer."

THAT the force producing tides is different from that observed in tidal currents is very obvious when the velocity of the tidal force in different places is compared with that of the currents. As in most places contiguous to tidal water there are, speaking broadly, tides twice in every twenty-four hours, the tidal force must run over the circumference of the globe in that period—that is, on the equator—approximately at the rate of 1,000 miles in the hour. From observations made by Mr. Rendel, in the Mersey, it appears the tidal force moves at varying velocities in that river, and that the velocity vary with the state of the tide between springs and neaps. Beween Formby Point and New Brighton the rate of the so-called head of the wave appears to be uniformly 24 miles in the hour; between New Brighton and Prince's Dock, 12 miles at springs, and 6.22 miles at neaps; from Prince's Dock to Ellesmere Port the rates are respec-

*A paper read before the Liverpool Engineering Society.

tively 27 miles and 15.43 miles; from Ellesmere Port to Runcorn 28 miles and 26.67 miles; from Runcorn to Fidler's Ferry, 12.5 miles and 6 miles; from Fidler's Ferry to Warrington, 7.5 miles and 6.76 miles; the average rates from Formby Point to Warrington, 16.7 miles and 11.4 miles. As is well known, the greatest velocity of tidal streams in the Mersey does not exceed 7 miles in the hour, the average being much less. As the number of observations taken by Mr. Rendel's directions were extremely limited, the results quoted are open to correction, but for the present purpose they suffice.

On referring to other rivers, it will be seen that the differences in the rate of tidal force and tidal current are very great. In the Thames they appear as follows:

	Head of wave S.	Head of wave N.
Between Deptford and London Bridge	15.6	15.6
Between London Bridge and Battersea	9.78	14.6
Between St. Katherine's Docks and Battersea Bridge	9.	8.28
Between Battersea Bridge and Putney Bridge	18.	6.
Between Putney Bridge and Kew Bridge	22.5	17 nearly
Between Kew Bridge and Teddington Lock	7.6	4.6
The average between Deptford and Battersea	11.7	15.
The average between Battersea Bridge and Teddington Lock	10.7	7.4

The rate of the tidal current between London Bridge and Putney Bridge at flood appears to be two miles per hour.

In the Tyne the velocities of the head of the wave are reported as follows:

	Head of wave S.	Head of wave N.
Between Tynemouth Haven and Prior's Stone	2	2
Between Prior's Stone and Ballast Office	2.95	2.95
Between Ballast Office and Howdon	10	5
Between Howdon and Bill Point	9+	7.4
Between Bill Point and Old Quay	9–	11.8
Between Old Quay and Elswick	9.74	—
Between Elswick and Stella	—	—
Between Stella and Newbern	5.6	5.6
Between Tynemouth and Newburn	9–	9–

In the Clyde.

Between Port Glasgow and Bowling	12.28
Between Bowling and Clyde Bank	6.63
Between Clyde Bank and Glasgow	16.37
Between Port Glasgow and Glasgow	10.67

The velocities of the flood and ebb streams respectively, as—

	F	
Dumbarton Castle to Newshot Isle	0 91	1.6
Newshot Isle and Glasgow	0.48	0.9

In the Severn.

The greatest velocity of the tidal force	74	miles per hour.
At the same place the tidal current	4.48	"
The least velocity of tidal force	6	"
The tidal current at the same place	not given.	
The greatest velocity of the tidal current	6	"
The tidal force at same place	10	"

These examples suffice to show that the force by which tides are produced is very different in degree from that which produces tidal currents, it being remembered both forces vary with the state of the tide. On examining the time at which there is high-water F and C at different places from which reliable observations are recorded, it will be found that, with some remarkable variations, the gradual progress of the tidal force is from east to west. Apparently from this circumstance, combined with the unbroken mass of water in the Southern Ocean, between the latitudes of 30 deg. and 70 deg., it has been assumed tides are produced by an immense wave originating in that part of the world, much as if the ocean were pulled up by the combined influence of the sun and moon over an enormous area, and then suddenly or rapidly dropped again. Obviously this illustration is very misleading, for, if the ocean were so raised, and suddenly dropped, the wave would move concentrically in ever widening rings of diminishing altitude, as is seen when a stone is dropped into a pond, and the general advance in a westerly direction only would be impossible. The equilibrium theory of the genesis of tides which has hitherto been accepted as correct has been pronounced by such au-

thorities as Professors Whewell and Sir William Thomson to be inadequate for explaining various phenomena, but I am not aware that any modification of that theory or any other hypothesis has been generally adopted. A globe insulated from all external influence is assumed to acquire its form through three balanced forces, namely, the cohesion of its particles amongst themselves, centripetal force or gravitation to the center of the globe, and centrifugal force, and it is further assumed if the equilibrium of these forces be disturbed, the shape of the globe will be modified.

Supposing a body like the moon to be so near as to exercise an influence analogous to that of gravitation to the earth's center, the particles of the globe on the side next to the moon will be attracted with a greater, those on the further side with a less force than those which are intermediate. Consequently, the gravitation to the center of the particles nearest to the moon is diminished, and, therefore, if at liberty to move amongst themselves, will rise above the general level, and the more distant particles will be heaped up on the side which is turned away from the moon. Hence if the globe were at rest, the free particles would take the form of an oblong spheroid with its longer axis passing through the moon; and it may be shown from theory that the spheroid would be an equilibrium if the longer semi-axis exceeded the shorter by about 58in. In applying this illustration to the tides it is assumed that, in consequence of the rapid rotation of the earth, its spheroid of equilibrium is never fully formed, for the vortex of the spheroid has shifted its position on the earth's surface, and an immensely broad and very flat wave is formed, and follows—or precedes—the motion of the moon at some interval of time. In the open sea that interval is usually from two to three hours after the moon's transit over the meridian, either above or below the horizon.

It appears to me this theory is capable of a very important amendment in its conception. The movement of the particles, consequent upon the moon's influence, appears to me due to the greater freedom given to centrifugal force, in consequence of the earth's gravitation being to some small extent neutralized by the moon's influence; and that such centrifugal force does not cause "an immensely broad and very flat wave" following "the motion of the moon," but imparts to all the free or fluid particles on the earth's surface a tendency to fly off in a direction opposite that of the earth's rotation, just as mud flies backward off a carriage wheel; this centrifugal tendency operating on every meridian in turn, its effects being visible at some interval after the moon's transit above or below the horizon.

Allowing for the fact that the sun's influence is much less than the moon's, the phenomena of solar tides are identical, but they are not perceptible apart from the lunar tides. When the two sets of tides coincide in time and place, which they do when the sun and moon are in opposition or conjunction, the result appears in what are known as spring tides; when the sun and moon occupy intermediate positions, and are neither in conjunction nor in opposition, then the solar high water coincides with the lunar low water, or the reverse, and the result appears in the neap tides. Other changes in the relation of sun and moon are indicated in the varying levels of the tides, the result or what is termed their priming and lagging, as the sun is westward or eastward of the moon. Obviously these variations of the tide depend upon the extent to which the earth's gravitation is modified by the influence of the moon and sun, and the extent to which the earth's centrifugal force is freed from the restraint imposed by that gravitation through which the original theoretical equilibrium is constantly changed.

When the moon is on the equator, at the time of her transit, the action of tidal force may be assumed as parallel to the meridian; as the moon passes into north or south declination, her influence will be greater on one side of the equator than on the other, varying with her distance north or south; as the extreme range of declination is an area of nearly 60 deg., representing, from position, one-third of the earth's periphery, her influence in the region of this extreme northern declination will be very much greater than at the same distance south of the equator; and two results may follow: (1) The amount of centrifugal force liberated will be greater in the northern

than in the southern hemisphere, and consequently the tidal elevation be greater; (2) that part of the force which is in the northern hemisphere may be somewhat in advance of the southern part, and consequently the line of motion will not be parallel to the meridian, but more or less oblique, according to the amount of declination; from which conditions two tendencies arise: (1) That the motion of the tidal force will be conoidal, that is, revolving round the part which moves more slowly; (2) that the water raised to the greater altitude will flow downwards. Thus the height and progress of the force may be subjected to irregularities, irrespective of the local and temporary incidents to which attention will now be directed.

Though gravity is said to be a force which is transmitted, not during any interval of time, but instantaneously, its velocity, according to Laplace, being, if not infinite, at least fifty millions of times greater than that of light, yet the greatest and least tides do not happen exactly at the time of new and full moon, but at least two or three days after, even in places directly exposed to the ocean. These variations are no doubt to be ascribed in part to the irregular form and depth of the ocean, the inertia of the water, friction, atmospheric pressure and other causes, but principally to the time which elapses before the centrifugal force acquires its greatest strength and momentum. The mud thrown off a carriage wheel does not quit the tire at its highest level vertically over the axle, otherwise the mud would fly upwards; its backward course shows clearly that it leaves the tire at some measurable distance behind the vertical radius of the wheel. Similarly, the water which has a tendency to leave the earth does not rise to the zenith, but is thrown backward, that is westward of the meridian on which that tendency is acquired. Consequently those particles of water which are nearest to the moon, that is, are on the meridian she is actually crossing, have a tendency to fly backwards over the more westernly water in a film of thickness varying with the centrifugal energy, thin near the moon, and thickening to the utmost limit of the earth's hemisphere nearest to her—that is, at the distance of say 45 deg. from the meridian on which she is for the moment. Thus, the original equilibrium of three forces would be destroyed, and one of four forces take its place; in compliance with which the water in the hemisphere furthest from the moon would also be propelled eastward of her meridian, and form another cycloidal excrescence balancing that first formed.

If tidal action is referable to centrifugal force, in every body of water, however small there will be a tendency to that action; but probably, perceptible tides require for their genesis a considerable bulk of water. It may be that a minimum expanse must be combined with a minimum depth; probably from recorded observations, some indefinite bulk is necessary, irrespective of the ratio of expanse and depth; with a considerable surface extending east and west. In the Mediterranean, for example, it is not at all likely a wave originated in the Southern Ocean would give any motion to the water. Of the tides at Toulon it has been observed that the coincidence of phase of the main lunar and solar semi-diurnal tides, happening some four or five hours after the time of new and full moon, would point to the conclusion they were wholly generated in the Mediterranean, and were scarcely, if at all, influenced by any action from the North Atlantic, through the Straits of Gibraltar, the amount of retardation of coincidence of phase for those components, amounting on the western coast of Europe to between thirty and forty hours.

The great depth of this sea, in some places exceeding 1,000 fathoms, combined with its great length from east to west, makes its mass very considerable. Its seems not improbable as much centrifugal force may be liberated as will produce the effects observed; the absence of tides in the Levant is consistent with such an origin.

From the assumption that tides are produced by centrifugal action, it is apparent that the quantity of water removed by the tides is comparatively trivial. In the Amazon, sometimes called the Mediterranean of America, the basin is estimated to contain, exclusive of the Para and Tocantins, an area of 2,330,000, English miles; that is, more than one-third of South America, and equal to

two-thirds of Europe. The average discharge is 750,000 cubic feet, or more than 4,500,000 gallons per second. Wallace states that with the tide the water rises; but during the flood, as well as the ebb, the current is moving rapidly down. This takes place, he says, at the very mouth, for at the island of Mexiana, exposed to the open sea, the water is always fresh, and is used for drinking all the year round; though it appears from Bates that the lower courses, as well as the channels and bays of the Delta—that is, for 150 miles from the sea, have no proper downward current, but ebb and flow with the tide. The apparent discrepancy willl disappear if it be assumed that the flood does not carry any sea-water with it, does not effect any translation of water, the land water forcing its way out though unable to neutralize the tidal action. Being on the equator, the centrifugal force is very great. Some idea of its strength is suggested by the distance up the Amazon to which it penetrates. It was observed by Wallace in the Tapagóz, a branch of the great river. In that river, at the end of the dry season, there is but a small quantity of water, and the current is very sluggish. The Amazon, however, rises very considerably with the tides, and its waters become higher than those of the Tapagóz, therefore they enter that river and force it back; the Amazon itself is then seen to be flowing rapidly down, while the Tapagóz is flowing up. The tide rises in the Amazon considerably above Santarem, but the water never flows up; the surface merely rises and falls. Bates observed the water to rise daily with the tide 2 in. or 3 in. in a small creek of the Cupari branch, 530 miles from the sea; therefore the current in the creek was not strong enough to neutralize the tidal force, as it was neutralized near the sea.

At Para, springs rise as much as 11 ft., yet a tidal current is not perceptible. It is said there are no fewer than seven tides in the Amazon, within a length of 600 miles. The Gulf Stream has a course of 3,000 miles, 60 miles wide and 100 fathoms deep, with a velocity varying from five miles an hour to only 10 miles a day, unbroken by tidal ebb or flow.

There is a large space of still water in the Irish Sea, between Carlingford and the Isle of Man, where occurs the phenomenon of water rising and falling without any perceptible stream. This space of still water is marked by a bottom of blue mud, the surface probably of a deposit of blue clay, an unfailing indication of the absence of disturbance, since probably it is largely impregnated with vegetable matter, from which its color is derived, and characteristic of riparian deposits. The stream by which the mud is conveyed to this spot must, therefore, be very gentle.

The maximum elevation of a lunar tide is estimated to be 5 ft., that of a solar tide 2 ft. The sum of those figures represents the highest springs, and their difference the lowest neaps. The elevation of 7 ft. above the surface of the ocean is but the infinitesimal part of the earth's diameter of nearly 8,000 miles, and has a much smaller ratio to the moon's distance of 237,000 miles, or thirty diameters. The ratio of the centrifugal force, released by the influence of the moon and sun, to the earth's centripetal force, must also be very small, and to produce the effects assigned to it may be conceived as acting hydrodynamically like an hydraulic ram; that is, the centrifugal force is, as it were, in a closed vessel full of water, the top of which is, in a degree, elastic. When the force is too weak to expand the cover is confined, and under pressure; as the force is increased the cover is expanded, but when the expansion reaches the limit of elasticity the water is completely bound, and great force may be transmitted through its agency. For the elastic cover or web, acting on the surface of the water, substitute elastic gravitation acting upon every particle of water, it is obvious the effect is similar, and pressure applied at one end of the vessel of water will rapidly produce effect at the other end, whatever the distance, if the channel be unobstructed.

This conception of the resistance offered by centripetal to centrifugal force appears identical with that of "the practical rigidity conferred by rotation" on frictionless particles, which has been proposed, almost simultaneously, by General Barnard and Sir William Thomson. But they assume "an infinitely rigid envelope," instead of one partially elastic, which appears to me to represent

the action of gravitation; the phrase "*indefinitely* rigid envelope" will appear to be unexceptionable.

Continents and islands, some of varied outline, occasion interruption and deviation in the course of centrifugal force. It is not likely any appreciable force is released from land, because of the cohesion of its particles, and its density is very much more than that of water with, say, twice the specific gravity. Land surface, therefore, does not present the undulating motion so characteristic of tidal water. So, likewise, it is to be observed that, on the western side of land, the inshore water will not be raised by the passage of centrifugal force from the eastward; and the tendency of the force generated westward of the land is to lower the surface of the inshore water, through the increased energy temporarily given to centripetal force, thus producing low water; half-tide level marking the brief interval when the centrifugal and centripetal forces balance each other. This is, theoretically, the normal level of the sea when undisturbed by the influence of the moon and sun. Thus the phenomenon of contemporaneous high and low water, on the same meridian but in different latitudes, is observable in the ocean, as on the West Coast of Africa, when the level of the inshore water is reduced, there may be high water to the southward.

Another feature in the action of centrifugal force must not be overlooked, which is its greater development in equatorial than in polar regions. At the poles themselves there is not any rotation, and consequently there is not any centrifugal force: but its genesis has increasing energy till the greatest is acquired on the equator or between the tropics; thence the excess may pass north and south in the lines of least resistance, giving motion to circum-polar waters, losing therein its centrifugal character, becoming simply hydrodynamical, and, as such, returning towards the tropics, and giving to inshore waters that tidal influence which could not be bestowed by centrifugal force in its normal course.

These suggestions receive confirmation from the tidal observations made in the Arctic expedition, 1875-6. On these Professor Haughton remarks that the expedition, proceeding northwards up Smith's Sound, met the tide coming from the north, at or near Cape Frazer, lat. 79 deg. 40 min., and left the tides of Baffin's Bay. The new tidal wave, observed on both ships, is specifically distinct from the Baffin's Bay tide, and from the tide which enters the Arctic Ocean through Behring Straits; and it is, without question, a tide which has passed from the Atlantic Ocean round Greenland northward, and then westward. The Artic Ocean being for the most part covered with thick ice, may be regarded as an accumulating chamber, into which the tidal forces from the North Pacific and North Atlantic are combined under pressure, and issue southwards to produce the tides of North-Western Europe. It would be inappropriate here to enter more at length into the discussion of the general question of the genesis of tides; those who wish to do so are referred to a paper on the subject in the *British Architect and Northern Engineer* for August 1st and 8th, 1879; but preparatory to applying the views advocated to the special purpose of this paper, it is desirable to resume so much of the discussion as relates to the North Atlantic.

From the preceding suggestions it would be expected that in the North Atlantic tides of the same age on the western shores of Europe would follow those on the eastern shores of America; that is, that the time of H. W., F. and C., would be later in Europe, because the easternmost part of the Atlantic is inshore. This anticipation is confirmed by Whewell, in his remarks upon simultaneous observations, and from the tables published by the Admiralty annually. First of all, it may be mentioned that on the coast of America the tide advances from the equator in a northerly and westernly direction, resulting from the resistance presented by that continent and the islands of America to the centrifugal force. That being most developed in equatorial regions is deflected off the coast of South America northwards, and, in combination with the various degrees of force generated in each latitude, advances westward, touching first at Cape Hatteras, and thence traveling southward to Cape Fear, Charlestown, Savannah, and St. Augustine;

northward to Delaware, New York, and into the bays of Massachusetts, Boston and Fundy. At Newport the tide-hour is about midway between that of Cape Hatteras and those of Delaware and New York, its meridian being more eastern. The lateness of the hour northward of Newport is to be ascribed to the irregularities of the coast line, for headlands and islands obstruct the progress of the tidal force, as has been illustrated by the delay in that part of the force which, splitting off at the south point of Greenland, travels along the eastern coast into the Arctic Ocean, forming "the new tidal wave" referred to by Professor Haughton as observed by the Arctic expedition.

Those irregularties are, however, very trivial compared with the marked differences between the tidal hours in North America and in Europe; in the latter "the moon's crossing the equator is not felt in its effects until two or three days afterwards." Dr. Whewell further observes (27), "The different epoch of the diurnal inequality in different parts of the world is a very curious fact; the more so, since it is inconsistent with the mode hitherto adopted of explaining the circumstances of the tides, by conceiving a tide-wave to travel along all the shores in succession, In accordance with that view the tides on the shore of America had been considered as identical with the tide on the coasts of Spain and Portugal, which occurs about the same moment; nor does it appear easy to imagine the form of the tidal-waves so that this shall not be the case. Yet we find that the tides on these two sides of the Atlantic cannot be identical in all respects, for on 9th, 10th, and 11th June, when the diurnal inequality was great in America, it was nothing in the west of Europe; and on the 18th and 19th, when this inequality had vanished in America, it was great in Europe. It would seem as if the tidal phenomena on this side of the Atlantic corresponded to an epoch two or three days later than the same phenomena in America; and we may, perhaps, add, that different kinds of phenomena do not appear to travel at the same rate. Thus the equilibrium theory, though it may explain the general form of the inequalities, cannot give their epochs and amounts by any possible adjustment of constants. I may add, he says, that the notion of the progress of the tide-wave from south to north in the Atlantic is still further involved in difficulties by its appearing at the Cape of Good Hope, that the diurnal inequality showed itself most clearly on 17th, 18th, and 19th June, that is as late as in Spain and Portugal."

Just as the lateness of the tides on the western shores of Europe appears due to the time occupied by the return of the tidal force from the east of North America, after its northern deflection, so the similar lateness on the west coast of South Africa may be due to the return of the motor from the east coast of South America. There is much complexity in the tidal action off the west coast of South America, arising probably from the combined influence of the two deflections, caused by the east coast of Africa and the east coast of South America; and uncertainty as the distribution of land and water in antarctic regions precludes the possibility of a satisfactory explanation.

On referring to the tidal hours of the British Isles, it will be found that the earliest point of contact is the islet of Rockhall, long. 13 to 14 deg. W., lat. 58 deg. N., where H.W., F. and C. is at 3.30. On the west coast of Ireland the earliest time is also 3.30; that is at the Blaskets, off the extreme point of Kerry, long. 12 deg. At the extreme west point of France, the Isle of Ushant, the hour is practically the same, or 3.32; at Cape Ortegal and Finisterre, the north-west extremity of Spain, the time is 3.0; and at Belem, Lisbon, 2.30, and the hour gradually becomes earlier—or later—to the Straits of Gibraltar, and the West Coast of Africa, suggestive of a meeting in the Bay of Biscay between the North Atlantic return and an offshoot from the equatorial regions of the African Coast. Rockhall at the Blaskets are nearly on the same meridian. From the latter the tide hour gradually advances eastward, not westward, reaching Carnsore Point, on the south, at 6.0, Tuscar at 5.45, and Ballycastle Bay, on the north, at 6.25. From Carnsore, the hour advances rapidly to 11.0 at Carlingford Point. On the north the tidal force is detained in the narrow passage between Fairhead and the Mull of Cantire, and does not pass Red Bay until 10.31, reaching Don-

aghadee at 11.13. On approaching Scotland the tide hour at St. Kilda is 5.30, at Bernera in the Western Isles 6.11; its further progress eastward is very irregular and slow until it passes the Mull of Cantire at 10.45, reaching Morecambe Bay at 11.26; the numerous islands and litoral irregularities of the west coast of Scotland cause much delay. At the extreme north of the Shetland Isles, where the force is divided, the tide hour is 9.45, thence it advances southward and eastward, with considerable regularity of increase, to the mouth of the Thames, the hour being very uniform at places in the latitude of the Moray Frith and Kinnaird's Head; and just twelve hours later at the Thames.

On the west coast of England the tide hour from the south advances from 4.30 at the Scilly Isles, until it meets that from the north around the Isle of Man. Passing eastward through the English Channel, the hour advances steadily to Portland Bill, a little eastward of which it is detained for two or three hours by the contraction arising from the projection of Cape de la Hogue; afterwards it advances slowly and gradually through the Straits of Dover to the mouth of the Thames.

On the coast of Norway the tide hour at the Romdals Islands, a few degrees north of the Shetlands, and about 6 deg. east, the tide hour is 10.45, or just an hour later than at the north of the Shetlands. At the Loffoden Isles, considerably north and east of the Romdals, the hour is noon. Going southwards there is a detention between the Shetlands and Norway, and the hour at Bergen is 1.30, about the same as it is at Arbroath in Scotland, showing a drag, caused doubtless by the rugged coast of Norway and the in-draught to the Baltic. At the Skawt, south of the entrance to the Baltic, the hour is 5.56; here the hour from the north meets that which traveled through the English Channel along the coasts of France, Belgium, Holland, Germany and Denmark. In thus tracing the progress of the several divisions of the tidal force in these parts of Western Europe observation has been confined to the most prominent features of the respective coasts, such as headlands or islands, so far as they can be obtained from the Admiralty tables. The detention caused by islands has been referred to in the case of Greenland and the isles of Scotland; there are two other instances, on the coasts of England and Ireland, which are worth special notice. In the Solent, and as far to the westward as Portland, there are what are termed the first and second high waters. After low-water the tide at Southampton rises pretty steadily for seven hours, which may be considered as the first or proper high-water; then for an hour it ebbs 9 in., at the end of which time it begins again to rise, and in about 1¼ hours reaches its former level, and sometimes higher; this is called the second high-water. The tidal level is therefore nearly stationary for rather more than two hours; similar first and second high-waters occur on either shore of the Solent. This phenomenon is ascribable to the tidal force being divided at the Needles, one part traveling up the Solent, passing Hurst Castle at ten o'clock and West Cowes at 10.45, the other passing to the southward, and turning round Bembridge Point at eleven o'clock into Spithead, reaching West Cowes at 11.45 and Hurst Castle at noon. At Havre the water remains stationary for an hour, with a rise and fall of 3 in. or 4 in. for another hour, and it rises and falls 13 in. only for the space of three hours. This irregularity, no doubt, arises from the sudden projection of Cape de la Hogue, combined with the peculiar formation of the coast of Havre. On the coast of Wicklow County, and abreast of the Arklow Bank, is Courtown, a place which is termed a node or hinge of the tide, where it is often said the tide neither rises nor falls. This spot appears to me another example of the division of tidal forces here by the Bank of Arklow. Courtown is about midway between Wexford Harbor and Wicklow, and protected by the bank from any direct approach from the sea. There is a difference of four hours in the tidal establishments of Wexford and Wicklow; that is, high-water is four hours earlier at Wexford Haven than at Wicklow Head. At Kilmichael Point, a little north of Courtown, the establishment is exactly two hours later than at Wexford Haven, and two hours earlier than at Wicklow Head. The tidal range is 2 ft. at Wexford, and Wicklow six or seven

feet. It is clear the times of high-water at one end of the channel of Courtown and of low-water at the other so nearly coincide, the tides become complementary, and nearly balance each other at Courtown, producing a state of almost no-tide. To a limited extent the phenomenon of two tides is found in the Mersey, the flood through the Horse or Hoose and Rock channels, being twenty minutes to half an hour in advance of that through the main channels, which is detained by Great Burbo and other banks. On the ebb the tide, I think, usually turns earlier on the Cheshire than on the Lancashire shore, but not always; in making observation it is necessary to be very careful, as light bodies on the surface may drift down the river while heavier bodies are still carried upwards.

From observations reported by Mr. Shoolbred, C. E., it appears that the time of high-water of an equinoctial spring tide, April 8th, 1875, is nearly uniform at Whitehaven, Fleetwood, Liverpool, Belfast, Dundalk, Dublin and Kingstown, the greatest difference between any two places not exceeding forty-five minutes, whilst it is the same in four places out of seven, two of the other three being identical. At Barrow and Holyhead the variations were considerable, being much earlier at Holyhead and much later at Barrow. All these places border the area round the Isle of Man, in which the forces from the north and south unite. Admiral Beechey observed that the time of low-water at the northern and southern entrances of the Irish Sea, and at the entrance of the English Channel, were identical with that of high-water in Morecambe Bay and the Straits of Dover, and the reverse. Taking Mr. Shoolbred's observations, for the convenient comparison of the tidal range on the west coast of England and the east coast of Ireland, it appears that at—

	Feet.
Whitehaven the extreme range is...	27
Barrow " " " ...	30.66
Fleetwood " " " ...	28.93
Liverpool " " " ...	29.75
Holyhead " " " ...	18.5
	5)134.83
Mean........	26.96

and at

Belfast " " " ...	10.0
Dundalk " " " ...	14.33
Dublin " " " ...	13
Kingstown " " " ...	12.5
	4) 49.83
	12.46

That is, the range in England is twice as great as it is at the other side of the Channel in Ireland; and, where the times of high-water on each side coincide, the surface is inclined from east to west; at low-water the inclination is reversed, or, as it may be briefly expressed, at high-water the current is from England and Wales to Ireland: at low-water from Ireland to England and Wales. The explanation of this phenomenon is to be found probably in two facts, viz., the reunion of tidal force in the area round the Isle of Man and in Morecambe Bay, and the much greater volume and velocity of the streams in England and Wales as compared with those in Ireland. If a person carrying water in an open vessel stop suddenly the surface of the water will rise higher, if he has been moving quickly, than with a slow movement; and so, though in the ocean the surface of the water is not usually much elevated, yet, if any obstruction present itself, the rebound of the force causes an increased elevation according to the size and abruptness of the obstruction. In meeting a land stream the result is similar, with this additional feature, that the surface of that stream is also raised, action and reaction being equal; and when the obstruction is removed by the advance of the tidal force a reaction follows which carries the surface of the water about as much below its mean level as it had been raised above, unless the basin is too shallow. The ebb, then, is not caused by any continued tidal influence, but is the return of the water which had been dammed back by the tidal force during its passage up the river, in which it finally expires at the highest attainable limit.

The motion of this force may be conceived of as resembling the undulation of a carpet or table-cloth when a little air has been caught between it and the floor or table, but the particles of the carpet or cloth do not flow down each side of the undulation as do the particles of water. In the paper before mentioned I

have compared the action of tidal force in channels of irregular section to the rapid propulsion of an elastic dam, which contracts and expands with the form and size of the channel. Assuming the vertical section to be parabolic, the dam forms a weir of two slopes, up which at each stage of advance the upland waters rise on the one side to descend upon the other; there being two streams on the upward face of the dam, one over the other, the depth of each being undefined, analogous to the contrary movements in bodies of different draught referred to above. Other things being equal, the height of the weir will vary with the depth of the channel, because the level of the crown of the weir, that is, of high water of any tide, is uniform, or nearly uniform, throughout the tidal portion of a river. Thus the reason why dredging has been so serviceable becomes apparent, not, as is usually said, because more tidal water passes into the river, for, if the views enunciated above are correct, tidal force is as viewless as the wind and as immaterial, but because more upland water is pent up and afterwards discharged, as when sewers are flushed by a shallow stream. All attempts to force or to coax more sea-water into a river have almost always, if not uniformly failed; where said to be successful, the increased elevation of the level of high-water has been very trivial. On the other hand, where the altitude of the tidal weir has been increased by lowering its base, that is by dredging, the effective force of the upland waters has been increased and the channel deepened. This result is conspicuous in the Tyne, the Clyde, the Tay, the Ribble, the Liffey, and other tidal harbors.

I have been favored by Mr. Stoney, of Dublin, with interesting information respecting the improvements in the port of Dublin. The history of the operations in Dublin Bay has been recorded by Mr. J. P. Griffith in a communication to the Institution of Civil Engineers, May, 1879. The bar is from five to six miles east of Carlisle Bridge; in 1819 there were 6½ ft. of water on the bar at low water; in 1822, 8½ ft.; in 1838, 10½ ft.; in 1856 13 ft.; in 1868, 15 ft.; and in 1873, the date of the last Admiralty survey, 16 ft. There does not appear to have been any subsequent survey.

The first attempt at improvement was the adoption of dredging by hand, if that can be called dredging, and the continuation of the south bank of the Liffey to Poolbeg lighthouse, just within the bar, combined with the construction of the great north wall from the shore at Clontarf to about 1,000 ft. short of the Poolbeg light, thus leaving an entrance of that width. This work was completed by 1825, and in 1838 the depth of water on the bar had been increased to 10½ ft., or by 2 ft. since 1822, giving an average of 1¼ in. per annum. Between 1838 and 1873 the depth was further increased to 16 ft., giving an average annual increase of 1.88 in.; the rate for the five years between 1868 and 1873 being 2.48 in., and the greatest on record. This increased rate appears to be due to the great increase in the amount of dredging. Mr. Griffith appears to ascribe the deepening of the bar almost exclusively to the great northern wall, the effect of which would seem to be merely that of contracting the sluice through which the upland waters are discharged, after the water level has fallen below half-tide level, that being the height of the southern part of the wall for a length of 500 ft. It is obvions that the water which enters the harbor over that part of the wall, and through the gap at Sutton, will return by the same route; and that the most obvious way now available for increasing the scour over the bar is to increase the capacity of the harbor below half-tide level, or to raise the lower part of the northern wall.

Messrs. Giles and Halpin in their report of 6th May, 1819, explain their reasons for recommending the construction of the great north wall to be the sheltering of the harbor, the prevention of the sand passing from the North Bull into the harbor, and the admission of as large a body as possible of tide-water into the harbor, and its return past the lighthouse within such limits and in such direction as will produce the best scouring power to deepen the bar, combined with the least obstruction to the navigation. They proposed that the wall should extend to the distance of 2,000 yards south-east from the shore of Clontarf, and there to commence a parabolic curve towards a point due north of the lighthouse, by which curve the direction of

the ebb current would be graduated to a suitable course for effecting the best scour at the entrance and on the bar; and the proper width of the entrance will be practically determined as the embankment proceeds by its operation upon the currents in the channel.

When Mr. Telford made his report of 16th August, 1822, the wall had been carried for a distance of about 5500ft. to its full height, about 1500ft. further to the level of H. W. N. T., and about 500ft. more to the level of half flood. The effects of the wall were very evident, for the sandbanks inside, having been deprived of their usual supply from outside, had been lowered on the average more than 3ft.; that part of the bar immediately opposite to the harbor mouth had likewise been lowered from 6½ft. to 8ft., and even 9ft.; the western part of the bar, adjacent to the old channel, remaining 7ft. and 7½ft. only. Telford was of the opinion that the entrance would still be considerably too wide to produce the full effect of deepening the bar, or forming a perfect and direct channel within the south wall; and, therefore, strongly recommended the adoption of Mr. Halpin's proposal to add a still further extension of about 500ft., to be carried forward with great caution, observing the effects produced upon the current of the flowing and ebbing tides during the ensuing winter; the extension, at present, not to be raised above the level of half tide until the effects on the current have been fully and carefully ascertained, after which the most advisable measures for completing this great work, both as to shape, extent, and height, may with propriety be determined. The mean annual discharge of water is stated to be 51,000 cubic feet per minute; the limit of tidal range up the river, a weir at Island Bridge, three and a half miles above Ringsend, or eight miles above Poolbeg; the greatest velocity of the tidal current over the bar at half ebb H. W. S. T. three and a half miles per hour; but in all tides the velocity diminishes considerably as the stream spreads and enters the deep water east of the bar. The depth of water on the bar H. W. O. S. is 28ft. Mr. Griffith, in his reply to my inquiry made before seeing Mr. Mann's book, informs me that the mean discharge of the Liffey and Dodder combined may be taken as 40,000 cubic feet per minute; and adds, no accurate gaugings have ever been taken, but the foregoing estimate, he believes, is near the truth. It is very desirable that the mean discharge of all important rivers be accurately ascertained, not only as a basis of comparison with other rivers, but also for determining the utmost amount of scour which can be made available; the amount actually made use of being as much as is required for the purpose. Before proceeding to review other harbor works, attention should be directed to the excellent method adopted by Mr. Storey for constructing the basement of retaining walls below low-water level without the aid of coffer dams, pumping, and staging. Blocks of masonry 29ft. high, 11½ft. long, and 21ft. 4in. broad at the base, are built on a wharf, and about three months after completion are lifted by powerful shears and conveyed to their destination, where each forms 4½ft. in length of the lower portion of the wall as high as low-water level. When a number of blocks has been thus laid in position, the superstructure is carried up to the coping level by tidal work in the usual manner; the total height of the wall being 45ft. Each block weighs 350 tons.

In the Clyde the improvements have been specially directed towards widening and deepening the harbor of Glasgow. The general condition of the river appears to have received very little attention. Practically, the channel from Greenock to Glasgow is now level, with the same rise of tide at each end, about 24 miles apart. The works carried out since 1758 have lowered the level of low water 8ft. at the rate of .84in. per annum; since 1853 the depression has been fully 18in., at the rate of 1in. per annum; the level of high water in 1872 was about 10in. higher than in 1853. The river has been very much widened at Glasgow. A few years since the weir, which, at 120 yards above Albert Bridge, formed the eastern limit of the harbor, was removed, with the effect of lowering rather than raising the level of high water. The weir may have caused a slight local heaping up of tide by its obstruction. The bed of the river above the weir has been lowered some feet, chiefly through the action of floods, the

soil carried away having been deposited in the harbor, and thence removed by dredging. Smeaton reported that at Pointhouse Ford, now the western end of the harbor, there were depths at low water of 1ft. 3in., and at high water 3ft. 8in.; and at Hirstpoint, now within the harbor, there were depths of 1ft. 6in. at low water, and of 3ft. 3in. at high water; in 1872 there were 14ft. at low water, and 24ft. at high water at each place. Thus the bed of the river has been lowered nearly 13ft. In 1837 springs rose and fell at Port Glasgow about 11ft., and at Glasgow 7ft. At Port Glasgow they are still about the same, but at Glasgow springs now rise 10½ ft., neaps 9¼ft.; in 1837 the time of high water was 1 hour 20 minutes later at Glasgow than at Port Glasgow—now 1 hour 5 minutes. In the beginning of the century the difference was three hours. Since 1856 the city has not been inundated; in that year the water just rose above the quay. In April, 1880, and March, 1881, two floods occurred, the first said to be the greatest for fifty years, and the second heavier still. Both caused a strong current in the river, and the removal of much sand and soil from the upper reaches of the river, which were deposited in the harbor, from which they had to be removed by dredging. After the flood of 1880 it was necessary to dredge 300,000 cubic yards, at the cost of £7,500, to restore the harbor to its previous depth; the flood of this year, 1881, would probably render as much dredging necessary for the removal of the silt. During the quarter which ended about the time of the flood, 318,112 cubic yards had been dredged, of which 273,936 yards were new material, and nearly 17,000 yards were deposit. According to Mr. Ure, the ordinary rate of water discharge is 48,000 cubic feet per minute. Since the removal of the weir mentioned above, the influence of the tide has been observed 1,400 yards beyond its former range. These particulars are compiled chiefly from a communication to the Inst. C. E., "Proceedings," 1872–73, by Mr. Deas, engineer-in-chief to the Clyde Navigation. I have been favored by Mr. Deas with copies of his report on the bed and banks of the river Clyde, from the site of the weir above Albert Bridge to Westthorn, 1880, and of his quarterly reports, July, 1880, and April, 1881. I have also to acknowledge the courteous assistance of Mr. John D. Parker.

In the river Tay the mean or ordinary discharge is 274,000 cubic feet or 7,645 tons of water per minute; the river is navigable to Perth, which is twenty-two miles above Dundee, and thirty-two miles from the German Ocean. Before the commencement of the works for improving the navigation, which appear to have been confined to a length of nearly 12,000 yards below Perth, the passage was obstructed by certain ridges called fords, which stretched across at different points between Perth and Newburgh, and vessels drawing from 10ft. to 11ft. were unable, without great difficulty, to make their way from Newburgh to Perth during the highest tides. The most objectionable of these fords were about six in number, and the depth of water upon them varied from 1ft. 9in. to 2ft. 6in. at low water, and from 11ft. 9in. to 14ft. at H. W. S. T., so that the regulating navigable depth could not be reckoned at more than 11ft. under the most favorable circumstances. In addition many detached boulders were scattered over the bottom, and numerous fish cairns of stone and gravel had been constructed by the salmon fishers. The works for improving the navigation, extended over six working seasons, consisted of removing the boulders and fish cairns, and of dredging the fords, harrowing being adopted in some of the softer banks in the lower part of the river; three subsidiary channels were cut off by embankments, and in the most contracted parts the banks on both sides were excavated below low-water mark so as to equalize the currents. The depths at the shallowest places were made pretty uniform, being 5ft. at low water, and 15ft. at H. W. O. S.; steamers of small draught can now ply at low water, and vessels drawing 14ft. can get up to Perth in one tide with ease and safety. The velocity of the tidal force in this part of the river has been increased more than 1⅔ miles per hour, and the period of its influence at Perth has been extended fifty minutes.

In 1883 spring tides flowed...... 2h. 20m.
" " ebbed...... 7h. 0m.
Neap tides flowed...... 3h. 15m.
" " ebbed...... 7h. 0m.

In 1844 spring tides flowed	......	8h. 10m.
" " ebbed	......	7h. 0m.
Neap tides flowed	......	3h. 10m.
" " ebbed	......	7h. 0m.

The fall on the surface of the river from the tide basin at Perth to Newburgh, in 1833 was 4ft., and is reduced to 2ft. Between Dundee and Newburgh the river has not been improved and the tidal phenomena are unchanged. In respect to the Tay, a remarkable instance of the difficulty experienced in ascertaining facts is presented by the reports of Captain Washington, R. N., and Mr. James Walker, C. E. The former in a report to the *Hydrographer* to the Admiralty, dated 1st October, 1844, says:—"Tay bar no longer exists, and vessels drawing 24ft. of water may now enter the river and bring up in safety within its shelter at the lowest water of spring tides." Mr. Walker, writing 21st January, 1845, less than four months later, says:—"The general result of the various information I received, and my conviction from that information, is that the depth of the entrance has not varied during the last century and a-half—being 18ft."

In the Ribble the ordinary discharge of water per minute is 139,935 cubic feet.

Before the improvements were commenced a solid ridge of sandstone, about 300 yards in length, stretched right across the channel about half a mile below Preston; its surface was from 2ft. to 5ft. above the general bed of the river, and the higher parts of the rock were occasionally left dry during the long droughts of summer. At Lytham, 12 miles below Preston, the ordinary rise of spring tides is 19ft. and of neap tides 14ft.; at Preston the rise of spring tides did not exceed 6ft., and the neap tides did not reach Preston, At "the chain" about two miles below the town, the level of low water is now left 8in. lower than in 1841, before which time the works had begun to show their effects; so it may be concluded that the total lowering is between 7ft. and 8ft., and that the tidal range has been increased to the same extent. The tidal propagation has been accelerated upwards of an hour. Training walls and dredging have combined to produce these satisfactory results, through which vessels, to which the navigation was formerly closed at all times, can now get up to the quays at Preston, even at neap tides, with comparative ease and safety.

The works on the Tyne are the most important which I propose to bring under your notice; they extend as completed, from the outside of the site of the former bar to 2½ miles above Newcastle Bridge, a total length of 13 miles, and they are in progress upwards for a further length of 6¼ miles. The works were commenced in 1853 by enclosing a bight of the river about three miles from the bar, and converting it into the Northumberland Dock of 55 acres of water, exclusive of basin and lock; it was opened in October, 1857. In 1856 were commenced the piers at each side of the river entrance. They were designed by Mr. James Walker, C. E., for the protection of vessels from the prevalent and destructive N. E. to S. E gales, and for facilitating the removal of the bar. In 1859 Mr. J. F. Ure, C. E., formerly of the Clyde, prepared a comprehensive scheme for the improvement of the tidal portions of the river, extending from the bar to the boundary stone, Hedwin springs, a distance of 19¼ miles; the requisite parliamentary powers were obtained in 1861, and the following results realized:—The bar has been removed, and, where formerly there was only a depth of 6ft. at low water, there are now more than 20ft., a depth which is maintained for a considerable width into the harbor. In 1879 the depth of water at the entrance of the Tyne was not less than 22ft. at low water, or 37ft. at H. W. S. T. The Narrows, below South Shields, have been widened from 400ft. to 670ft. In Shields harbor dangerous shoals have been removed, and for a length of 8000ft. vessels can moor in a depth of more than 30ft. L. W. S. T., and instead of the tortuous channel through which it was formerly difficult to navigate one vessel at high water, three or four may now be safely towed abreast at any time of tide. Vessels which formerly were detained for months before the bar could be crossed in safety even at high water, can now leave in any weather in which it is fit to go to sea, at or within a short time of low water. For the whole distance between Shields harbor and Newcastle there is now a depth of 20ft. L. W. S. T.,

where river steamers drawing 3ft. or 4ft. water used to ground for hours. For 2½ miles above Newcastle the river has been deepened 18ft. low water, and at Blaydon a depth of 12ft. has been made. In the upper part of the river a new cut, 400ft. wide, has been made through Lemington point,and opposite Blaydon the river has been widened from 150ft. to 400ft., thus enlarging the tidal receptacle. There has not been much alteration in the tidal range, the greatest, 2½ft. is at Hebburn quay, where L. W. S., ebb upwards of 2ft. lower.

In the floods the liability to inundation has apparently been removed, as the flood line on the 10th of March last was considerably lower than in the floods of November, 1856 and 1861, though at Wylam Bridge, above the boundary stone, the highest level in 1881 was 3in. above that in 1861, and 8in. only below that of 1856. Before 1860 high water at Newcastle Bridge was sixty minutes later than at the entrance of Shields Harbor, a distance of about 9½ miles, and at Newburn, about 7½ miles above Newcastle, it was twenty-nine minutes later still. In 1879 high water at Newcastle was only twelve minutes later than at Shields, being a gain of forty-eight minutes, and the gain at Newburn is twenty-one minutes. It does not appear that the mean annual discharge has ever been ascertained. For the interesting information respecting the Tyne, except as specially mentioned, I am indebted to Mr. Philip J. Messent, the acting engineer to the Tyne Commission.

The beneficial effects of the works referred to may be presented in the works of Mr. David Stevenson, who has summarized them as follows: (1) To depress the level of low water; (2) to increase the range of tide; (3) to accelerate the propagation of the tide through the channel of the river; (4) to prolong the duration of tide in the river; (5) to equalize the velocity of the tidal currents, removing rapids and bores; (6) to add to the beneficial scouring power of the river; (7) to increase the navigable depth.

It has been shown that in the harbor of Glasgow the bed of the Clyde has been made practically level; but Mr. Stevenson, speaking from his own experience, suggests that an engineer may calculate on reducing the slope of tidal navigation to about 3in. or 4in. per mile—equal to a gradient of 1 in 15,840; and that the gradient should not, if possible, exceed 10in. per mile, which is equal to a gradient of 1 in 6,336. It is therefore most desirable that all strictures, horizontal or vertical, should, as far as possible, be removed, especially those which abruptly interfere with tidal propagation. Their removal, while prolonging the period of tidal action, will reduce the velocity of tidal currents, and so facilitate navigation and relieve it from hazardous contingencies.

That the widening or deepening of any part of a river increases its tidal capacity is sufficently obvious, but the manner in which that capacity becomes available may be worth some consideration. It is usual to say, if the receptacle be provided, the tide will throw into it a larger volume of sea-water; but that statement is based on the assumption that the tide acts as a wave, and in some situations effects the translation of a large body of water. If the suggestions offered in the first part of this paper are well founded, tidal action, so long as it lasts, is not intermittent, but continuous, whereas all wave action is essentially intermittent; in the ocean, sea, and river, usually caused by wind. No doubt the rise or fall of the tide is generally accompanied by wave motion, if only a ripple; but, as before stated, there is in the Irish Sea a spot where the water rises without any current or wave, except such as is due to local and special disturbance, and in very calm weather the tide may be observed to rise upon any shore with scarcely a ripple to break the surface of the water. Therefore, instead of assuming that the tide forces sea-water into an estuary or river, it would appear to be more correct to say the tidal force dams back the fresh water brought down the river, and causes an indefinite number of flushes, or a continuous flush, the place of discharge being momentarily removed higher and higher, until the flush ceases, because the level of the top of a weir is reached. Thus, then, the element in calculating the extent to which a receptable should be enlarged is the quantity of water discharged by the river in the period during which the tide is rising at any part of the river's course.

The more the course of the weir is

left free from obstruction arising from irregularities in the bed or shores of a river, the more rapidly will the tidal force advance up the river and the less inclined will be the plane presented by that force to the descending water, consequently the more rapidly will that water rise, and it will attain to a greater altitude, just as in a stream, the level above an obstructive boulder is greater than it is above a pebble.

Attention has been drawn to the existence of contrary streams in tidal rivers, arising from the greater specific gravity of salt water as compared with fresh. This was first observed by Mr. Robert Stevenson in 1812, in the Aberdeenshire Dee; he noticed that the current of the river continued to flow towards the sea with as much apparent velocity during flood as during ebb-tide, while the surface of the river rose and fell in a regular manner with the waters of the ocean. By the use of a hydrophore, Mr. Stevenson ascertained that the salt or tidal waters of the ocean flowed up the channel of the Dee, and also up Footdee and Torryburn, in a distinct stratum next the bottom, and under the fresh water, which continued perfectly fresh and flowing in its usual course towards the sea, the only change discoverable being in its level, which was raised by the salt water forcing its way under. The tidal water so forced up continued salt, and when the specific gravity of specimens from the bottom were tried and compared with those taken at the surface, the lower stratum was always found to possess the greater specific gravity due to salt over fresh water. Mr. Alan Stevenson, judging from marine productions, considered that salt water in the Tay rose to a very short distance above Mugdrum Island, that is, twenty-five miles from the mouth of the river and ten miles below the tidal limit.

I have not been able to obtain any information as to the thickness of the stratum of salt water, though it is impossible to form any accurate idea of the quantity of water forced in from the sea, without the measurements of length, breadth, and thickness. On the assumption that tidal force is exerted as a weir, a small quantity of sea-water would be carried by the force up the river, but in consequence of its greater specific gravity, the water so carried in would have a tendency to the bottom of the weir. There, through irregularities on the bed and sides of the river, it would be gradually mixed with the fresh water until its marine character was lost, the quantity of salt water, always a vanishing quantity, being gradually reduced from the original amount, which, probably, is very small compared with the fresh water brought down the river.

It has been shown that the effect of improving the navigation of a river is to increase the velocity of the power which produces tides, and to diminish the velocity of tidal currents; and if the currents flow with less rapidity in the same time, it is clear the quantity of water translated by tidal influence must be less, and consequently, that less water is forced into the river from the sea. Thus it follows that the back-water, on which the scour of rivers depends, is so much of the upland waters as is penned back by the weir, until it accumulates to high water, the highest attainable level, and is then discharged. In improving harbors, therefore, the aim should be so to enlarge the receptacle above the weir that it may retain all the water the river discharges during the rise of an ordinary spring tide. If the receptacle is less than will accomplish this, useful water is lost. If it is larger, the cost of the works will probably be more than the value of the water stored, and deposits of silt will be formed. I am disposed, however, to accept the conclusion of the special commission on the Tyne, appointed in 1854—"that not only should every river, as observed by Mr. Rendel, be treated according to its own peculiar character, but, strictly speaking, each part of the same river may require different management.

The comparatively small influence usually ascribed to floods upon the enduring features in the tidal part of a river confirms the opinion that there is no adequate advantage in increasing the means of retaining water beyond the level of O. S. T., because the scouring tendencies of such excessive back-water are to some extent counteracted by the recurrence of tidal opposition. To that circumstance must be ascribed the large deposit in Glasgow harbor after the great flood in March last.

It is important that the amount of back-water, should yield the requisite amount of scour before the return of the tide; any excess tends to cause streams of inconvenient velocity until they are reduced by tidal opposition, and deposit the silt they have taken up to repletion.

What is termed the tide of ebb is generally regarded as the most effective scour, and the principal agent for maintaining the accessibility of harbors. If, however, tides are caused by centrifugal force, the term ebb tide is incorrect, as the downward current of a stream is due to gravity relieved from the opposition of centrifugal force; that is, the upland waters descend not only during the ebb, usually so-called—that is, until the tide water subsides to the river's normal level—but also during that further interval until the incoming tide produces slack-water.

NOTES ON THE RELATIONS OF MANGANESE AND CARBON IN IRON AND STEEL.

By MONS. ALEXANDRE POURCEL, TERRENOIR, LOIRE, FRANCE.

Transactions of the American Institute of Mining Engineers (advance sheets).

THE perusal of Mr. Willard P. Ward's "Notes on the Behavior of Manganese to Carbon," presented at the Washington Institute in February, 1882, has suggested further reflections on the same general topic, and has led to the preparation of the present paper.

The same observation that Mr. Ward has put on record in his "Notes" was also made by myself at about the same time (in August, 1875), and under almost the same conditions. From a blast-furnace that was *very hot*, as was the furnace mentioned by Mr. Ward, I obtained a pig-iron containing about fifteen per cent. of manganese, gray in color, and very tough. It could be pulverized, but could not be cut with a chisel. I analyzed this iron and found that it contained, as I had suspected, a large amount of silicon. From this fact I drew the conclusion that the silicon had deprived the manganese of its power of dissolving carbon, since the latter, instead of occurring in the pig in combination appeared as graphitic carbon. I thus saw reproduced on a large scale, and demonstrated in a visible way, the property that Colonel Caron, a French scientist, had discovered in silicon—the property of obstructing the process of hardening in steels by keeping the carbon in the graphitic condition.

An attentive study of the conditions under which the phenomenon observed by Mr. Ward takes place, led me to go back to operations of synthesis, and to make as I wanted them pig-irons containing varying quantities of silicon, manganese, and carbon. An iron thus prepared was intended to serve me as a chemical reagent in the production of steels cast without blow-holes, such as my lamented friend, Mr. A. L. Holley, has introduced and made known to the United States. What I needed in order to make very soft steels, cast without blow-holes, was an iron which, when it was added to the bath of steel, introduced into the bath a sufficient amount of silicon and of manganese, with the smallest possible proportion of carbon. Now, in analyzing an iron similar in character to that obtained by Mr. Ward, I found that the amount of combined carbon in the iron was almost nothing, and that the total carbon was between 3 and 3½ per cent., instead of being from 5 to 5½ per cent., as in ordinary spiegels containing 15 to 16 per cent. of manganese.

I then sought for a way of still further diminishing the carbon by increasing the silicon and manganese, and after a few trials I found that when the manganese and silicon are present in the ratio of their chemical equivalents, the carbon reaches a minimum. It is well understood that the higher the percentage of manganese and of silicon in the pig is raised, the lower the percentage of carbon will be; an almost complete elimination of carbon might, indeed, be obtained by means of silicon, but the law which determines that the percentage of carbon

shall reach its minimum is fixed by the ratio Mn : Si. When the manganese increases, the carbon increases also. For example, I have produced a number of tons of iron with from 11 to 13.5 per cent. of silicon, and from 17 to 19 per cent. of manganese, and the percentage of carbon has been the least—2 per cent.,—with 13.2 per cent of silicon and 17 per cent. of maganese, that is to say, when the two substances are present in the ratio Mn : Si.*

What are the reactions that take place in the blast-furnace when a pig-iron, or rather an alloy, of this kind is produced? Are the phenomena simultaneous or successive? My opinion is that they are successive, and that the carburet of manganese is the reagent that reduces the silica from which silicon is derived. We can in fact repeat that laboratory experiment which consists in maintaining a quantity of ferromanganese in a molten condition for several hours in a thick crucible, such as is used in the melting of steel. According to the length of time, more or less, that the ferromanganese is kept in the molten state, we find the walls of the crucible to be more or less attacked; the metal incorporates with itself a notable quantity of silicon, and loses some of its manganese and carbon. In this experiment there can be no doubt that the carburet of manganese is the reagent by whose action the silicon is derived from the silica in the walls of the crucible.

The laws of thermochemistry that have been established by Berthelot's numerous fine experiments equally confirm the opinion, to which I some time ago gave utterance, that when silicon and manganese occur together in a pig-iron or in a steel, they are in a state of chemical combination, as a silicide of manganese, if the percentages of the two substances are in the ratio, at least, of Mn: Si. It may, indeed, be affirmed that silicon when neutralized by manganese, that is to say, when for each chemical equivalent of silicon there is present a little more than an equivalent of manganese, does not diminish in the least the hardening property of steels. When the amount of manganese increases, the hardening property increases, since the manganese possesses the property of dissolving carbon, that is to say, of keeping it in the combined state.

* Mn = 27.5, Si = 21.

As to the opinion of Mr. Ward that maganese has no injurious effect on the wear of rails, I may say that I hold the same opinion, though for an entirely different reason from that given by Mr. Ward. The deterioration of rails from atmospheric causes, which may be likened to chemical action, is due especially to their physical condition rather than to the chemical composition of the ingot, from which the rail was made. A porous ingot, full of blow-holes, will produce a rail, on which, after a few months of service, the surface exposed to wear will be covered with numberless little rays or streaks, which are just so many more points of attack for atmospheric agents. Such a rail if laid in a damp tunnel will very quickly become useless. Possibly it would be used up a little more rapidly if it contained a high percentage of manganese, but in no case would the presence of that element be a principal cause of the effect produced.

If two rails, made from two ingots perfectly sound and free from blow-holes, are compared with each other as regards mechanical wear, my opinion, based on experience, is that the rail whose hardness lies within the limits I am about to point out, will resist wear more effectually than the softer one. The maximum of rigidity, combining resistance to bending with great power of resisting shocks, has been reached in rails of the following composition:

Carbon..........	0.50 to 0.45	per	cent.
Manganese.......	0.90 " 1.10	"	"
Phosphorus......	0.08 " 0.10	"	"
Sulphur....	0.05	"	"
Silicon.....	0.02	"	"

These rails made from perfectly sound ingots, and laid on one of the busiest portions of a great network of French railways, after *three years* of trial have not given occasion for a *single* rejection, and the wear observed has been insignificant. Of other rails made from ingots equally sound, and differing from the preceding only in having a smaller amount—from 0.5 to 0.7 per cent.—of manganese, some indeed have always been rejected after the regular test of three years, but that which has been especially remarked is that there

has always been a notable wear of the top of the rail.

It is also known to me that the rails which have best stood the rude tests of percussion and bending, demanded by the Russian railways, contain about 0.3 per cent. of carbon, and from 1.1 to 1.2 per cent. of manganese. The manganese, without sensibly diminishing the elongation of the steel, increases its tenacity and rigidity, as well as its power of resisting shocks. It gives to the steel this grand quality of *hardness* without *brittleness*.

In the month of August, 1881, I had at my disposition quite a large number of old steel rails, made at different steel works in Germany, and taken from the railways of Alsace-Lorraine. These rails had been worn out quite rapidly; they were all in very bad condition. The oldest of them bore the date 1874, and the mark "Bochum;" the most recent came from the steel-works, "G. H. Hütte," and were dated 1879! These rails in their chemical composition corresponded for the most part with the formula of Dr. Dudley,—those, at least, which did not have any excess of phosphorus or of silicon.—but their resistance to wear has not confirmed Dr. Dudley's opinion.

I have also been able to submit to the test of a blow a rail from the Phœnix steel-works, one from the Osnabrück steel-works, and a third made by Hoesch. The Phœnix rail showed the greatest power of resistance, but the metal is soft, it changes its form considerably, and lacks in rigidity. The Hoesch rail changes in form still more, and, besides, it is brittle. It broke under the shock of 300 kilogrammes falling through 3½ meters, the anvil weighing 12 tons (tonnes). The Phœnix rail withstood the shock of the same weight falling through 4½ meters. The Osnabrück rail, like that of Hoesch, is brittle, but it changes its form less easily.

In conclusion, like Dr. Dudley, I am of the opinion that elements like phosphorus, silicon, and sulphur, must be reduced to an absolute minimum in a good rail. I should insist especially on the phosphorus and the silicon, and less on the sulphur, but I do not put manganese in the category of ingredients that are injurious, either to the rolling or to the use of the rail. I should give the preference to a metal containing manganese to the amount of 1 per cent. as I have indicated above, a metal which is excellent for rolling and gives a rail of superior wearing qualities.

Table Showing Partial Composition of Different Rails.

Description of Rail.	Mn.	C.	Si.	S.	P.
Phœnix.........	0.373	0.490	0.093	0.034	0.102
Krupp..........	0.373	0.323	0.139	0.036	0.146
Bochum........	0.240	0.200	0.116	0.026	0.067
Union Dortmund	0.240	0.284	0.046	0.039	0.239
G. H. Hütte....	0.480	0.382	0.139	0.039	0.080
Osnabrück......	0 586	0.170	0.466	0.045	0.174
Hoesch.........	0.453	0 330	0.291	0.038	0.119

THE STRENGTH OF BOILER FLUES.

From "The Engineer."

A paper on the strength of boiler flues, by Mr. W. Martin, was read before the Society of Engineers in December. The paper was in some respects peculiar. It collected and put into a convenient shape much that is not as readily accessible as is perhaps desirable. The author's purpose was to call prominent attention to the circumstance that little or no knowledge, derived from actual experiment, is in existence at this moment concerning the strength of the flues of the Lancashire and Cornish boilers. In the words of his own abstract of his paper, the only systematic series of experiments which have been made at all, were made by Sir William Fairbairn twenty-four years ago, and the formula deduced by him from these experiments is still the one in use by engineers. The trustworthiness of this formula is not, however, beyond question; and its application to different cases can be made to produce somewhat anomalous results. It cannot be denied that the results obtained by Sir William Fairbairn, consid-

ered with regard to the ignorance upon the subject prevailing at the time, were of the highest importance; and they immediately suggested simple methods of effectively strenghtening flues, both old and new. Nevertheless, the experimental tubes used were very unlike actual flues in construction; they were very small, and were submitted to pressure in a totally different way from that in which flues undergo strains; and since, moreover, almost all the comparable experiments were with tubes of the same thickness, it is evident that the result deduced from them cannot be regarded as final. It is much to be desired that this subject could receive a new experimental investigation, and in such a case the experiments should be with objects corresponding in shape and conditions of strain, though not necessarily in size, to actual flues. Now we have no intention to dispute the soundness of Mr. Martin's views as thus expressed, or the value of his suggestions. It is, of course, desirable that something more should be known than is known on the subject; but we may point out that any experiments made with cold water pressure would be entirely misleading, and that to carry them out in the only way that would be really useful would be very costly. What that way is we shall explain further on. Before doing so it is necessary to show why the ordinary method of testing would be useless.

It will, we think, be conceded that any experiment or series of experiments in physical science carried out under one set of conditions, can give but vague and insufficient information as to what would take place under another set of conditions. Thus, for example, to argue that because a steamship in a calm sea attained a velocity of 15 knots an hour, she could maintain a similar speed while crossing the Atlantic in the winter would be simply fatuous. The only method at present practiced for testing boiler flues consists in submitting them to cold-water pressure until they give way. To deduce from experiments such as these the behavior of similar flues when at work in a boiler is not more reasonable than would be the line of argument concerning the probable speed of a steamship which we have just considered. A powerful indirect confirmation of the truth of this was supplied on Monday night in the course of the discussion on Mr. Martin's paper. Professor Unwin said that he had carefully investigated Fairbairn's experiments, and had improved on his well-known formula by strict mathematical reasoning, and he had obtained particulars of one case of collapse, in which the result exactly coincided with his theory. "But," he went on to say, "there is a skeleton in every cupboard, and I am fain to admit that my skeleton is especially large and grissly. I have obtained particulars from Mr. Lavington Fletcher of about two dozen collapses, and not one of them agreed with my formula, the flues giving way with pressures from 50 to 80 per cent. less than I calculated." In one word, the strength of a boiler flue is in all cases less when in work than that of a similar flue tested cold with a force pump. The reason is that the flue is, when steam is up, exposed to various strains which have no existence when the force pump is used. One of the most important of these is that due to expansion caused by heat. Let us take, for example, the case of a flue 30ft. long, ½in. thick, and 3ft. in diameter. If we suppose the pressure of the steam to be 70lbs. then its temperature will be 324 deg., but the metal of the flue will be hotter than this. At the inside next the fire its temperature cannot be much less than 500 deg., while next the water it will probably be 350 deg. Here, then, we have one set of circumferential strains set up. But at a ring seam the whole of the inside lap will probably attain a temperature of 500 deg., while the whole of the outside lap will be 350 deg. The inner plates must stretch the outer, and when the boiler cools again, unless the elasticity of the outer plate is sufficient to bring it back to its original diameter, there will be a leak at the ring seam. This is no fanciful idea. It is well known that the ring seams of thick flues are a constant source of trouble unless the iron and workmanship are alike admirable. But the flue is exposed to much worse strains than these. The top is, while steam is being got up at all events, at least 200 deg. hotter than the bottom. In a length of 30ft. the top of the flue will expand until the top is nearly half an inch longer than the bottom. If it is free to do this the middle of the

length of the tube will rise, and the tube will be cambered upwards; but if the ends of the boiler are firmly tied in with gusset stays, it will not be free to rise, and a very heavy longitudinal strain will be brought to bear on the plates and seams. Furthermore, we know that no boilers are at work the surfaces of the flues and furnaces of which are quite clean; on the contrary, they always have a coating of scale on them of greater or less thickness. The result of this is that the flues must be of necessity very much hotter than they would be otherwise; and, in point of fact, no one knows how hot a furnace crown is when a boiler is at work. But it is very well known that as the temperature of iron is augmented its stiffness greatly diminishes, and inasmuch as flues are never circular, they depend for their strength very much on their stiffness, and if this be impaired the flue is weakened. This point is not so fully understood as it ought to be. We may regard any section of the circumference of a flue as an arch, built up of practically incompressible voussoirs. If these voussoirs were united in such a manner that they could not move on each other, then we should have a rigid arch, which could only fail by the compression of the material of which it was made; but if the arch was not so tied together, then it would remain free from deformation only so long as the line of pressures fell between the extrados and the intrados of the arch—that is to say, within the thickness of the plate. Now the comparatively thin plate of a flue can be bent, and if any of our readers will take the trouble to get out on his drawing board a section of the circumference of a flue, he will see that the bending in or out of that section is precisely analogous to causing the rotation of voussoirs on their inner or outward edges. In other words, the fact that the iron can be bent is analogous to the use of a weak or flexible cement to unite voussoirs. This being the case, it becomes evident that the more readily the iron can be bent the weaker is the arch. This would not be true if the flue were a perfect cylinder, for then the line of pressures must fall in the thickness of the plate. But it is easy to see that in the case of, say, a 3ft. flue, $\frac{3}{8}$in. thick, a deformation to the extent of but $\frac{3}{8}$in. would throw the line of pressures within or without the plate. All flues with lap joints are out of truth, and were it not for the stiffness which the lap imparts, and the fact that the thickness being doubled at the lap the line of strain still falls within the metal, such flues would be much weaker than they are. If our reader has followed us thus far, they will see that the heating of a flue by depriving it of its stiffness has just the same effect as the weakening, let us say, of the mortar in a brick arch; and it must be clearly understood that this has nothing at all to do with the ultimate strength of the iron. That may or may not be greater at the temperature really attained by the flue than it is at any lower heat. If, then, it can be shown that iron is less able to resist bending strains when hot than when cold, it follows that comparatively moderate increases in temperature may much weaken a flue. It is probable that at a temperature of about 800 deg. the stiffnes of plate iron, in other words the resistance which it can offer to bending strains, is reduced by one-half; and if this be so we have at once an hypothesis which will explain many cases of collapse now regarded as mysterious. A moderate thickness of scale will permit the temperature of a furnace crown to rise much above that of the water. The overheating may not be sufficient to leave any mark on the iron, nor may it suffice to reduce the tensile strength of the metal to any appreciable extent, but it will take the stiffness out of it, and then a collapse may ensue. It is only necessary to establish the soundness of these propositions—(1) that the flue should not be quite cylindrical, and (2) that it is easier to bend a bar of iron when it is hot than when it is cold, and we do not think this can be disputed.

If, then, we desire to obtain any definite information concerning the strength of boiler flues, they must be tested under conditions similiar, as far as possible, to those which obtain in practice. In the United States and on the Continent, in a few cases tests have been made by filling up the boiler as full as it will hold with cold water, and then lighting a small fire in the furnace; the water will then rapidly expand, and while still nearly cold will put on the requisite pressure. Such testing requires to be very carefully done or the boiler may be irreparably injured,

and the system has the disadvantage that the test is still a cold water test. The method of experimenting on boiler flues which we advocate is, so far as we know, new. The flue must be fitted in a boiler shell having a very much higher bursting pressure than the collapsing pressure of the flue. Let this shell be filled up with cold water within a few inches of the top, and let the fires be lighted and hard pushed, just as they would be in practice in getting up steam, and as soon as the water has reached 212 deg. let the force pump be put on, and the flue tested to destruction, the fires being left burning all the time. The boiler being in a suitable building, with no one in it, no accident would occur when the boiling water began to run out. A large force pump should be used, competent to run up the pressure to the required point in a couple of minutes at most, and to crush the flue, even after ring seams had been started. It would occupy more space than we can spare to describe minutely all the precautions which would be necessary to secure accurate results. They will suggest themselves, we venture to think, to any one who has had fair experience in testing boilers. In this way, and in this way only, can flues be tested under nearly the same conditions in which they are strained im boilers; and if the experiment is ever carried out, we can promise that the result will be found interesting, if not startling.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers. —Meeting of December 6, 1882. Vice-President William H. Paine in the chair. John Bogart, Secretary.

The supply of water for cities and towns from subterranean sources, or ground water, as developed in the United States since 1870, was described by Mr. J. J. R. Croes, M. Am. Soc. C. E. It was at first supposed that such supply could be obtained by filtration of river or lake water through the gravel of its banks. It was discovered, however, that in fact, much more water came from the land side than from the river, and that wherever such a source of supply is successful, the water really comes from the underground reservoirs or streams which are found generally in all valleys containing much gravel. The wells, galleries and basins constructed in various places, were described, and their success or failure indicated. It was stated that experience was generally against the construction of open galleries or canals on account of the vegetable growth which always occurred in such cases. The paper was discussed by members present, and will be published in the transactions.

December 20, 1882.—Mr. William P. Shinn, M. Am. Soc. C. E., read a paper on the "Increased Efficiency of Railways for the Transportation of Freight."

The first portion of this paper gave, from carefully gathered statistics, a valuable amount of information in regard to the actual increase of traffic upon American Railways. In 1860 the tonnage mileage of the New York Central and Hudson River Railroad, the Erie Railway, and the Pennsylvania Railroad was about equal, and amounted in the aggregate to a little over three-fourths of that of the New York State Canals, and in 1870 each of these railroads averaged about the tonnage of the canals, and in 1880 they averaged each nearly double that of the canals.

The aggregate tonnage mileage of the other railroads was, in 1881, 1,217 per cent. more than 1860. Statistics were also given showing the increase of population of railroad mileage, of the production and export of grain and other leading exports. The means by which this rapid increase of freight transportation had been developed was considered under two general heads, namely, improvements in the physical conditions of the railroads, and improvements in the administration. The improvements in the physical condition were treated on under these heads:

1. Improved track or "permanent way," including bridge structure.
2. Additional sidings, and second, third and fourth tracks.
3. Increased capacity and strict classification of locomotives.
4. Increased capacity of freight cars.
5. Additions to terminal facilities.

The improvements in the administration were referred to under the following heads:

6. Improved methods of signaling.
7. Running locomotives "first in first out," and running freight trains at higher rates of speed.
8. Consolidation of connecting lines under one management by purchase, lease, amalgamation or otherwise.
9. Running freight cars through from point of production to tide-water without trans-shipment.
10. Issuing through bills of lading (or freight contracts) from western points of shipment to Atlantic and European ports.

The general introduction of steel rails was stated to be the very corner stone of increased efficiency. The improvements in all the directions referred to were treated of and described at considerable length.

The second portion of the paper presented the views of the writer as to the means whereby still greater efficiency could be most economically obtained. The constant demand is for more transportation facilities, for more cars. In the opinion of the writer, what is needed is not so much more cars, as more movement of cars. Freight blockades will be prevented, not by having more tracks to stand cars upon,

but by having fewer standing cars. It was shown that upon one railway there had been a decrease in the miles run by the cars of 21 per cent. between 1868 and 1881—and that the Union Line cars between 1879 and 1882 were increased 49 per cent. in number, while the mileage run by them decreased 16 per cent. in the same period. The remedies suggested by Mr. Shinn were, more main tracks, more locomotives, more trains, the improvement of the making up of trains at the points where cars are loaded. The detention of cars at stations and private sidings, and the absence of cars on foreign railroads were considered as among the greatest causes of loss, and the writer suggests that the remedy will be, to charge a per diam charge for cars when on foreign roads, and that this charge should be based upon the average economic value of its cars in use to their owners.

The paper was discussed by Messrs T. C. Clarke, G. S. Greene, Jr., W. C. Andrews, C. Macdonald, C. E. Emery, and by the author.

By a vote of the Society it was directed that this paper should be discussed at the annual meeting. Members of the Society and others interested in this subject are requested to contribute to this discussion. The annual meeting of the Society will occur January 17 and 18, at the Society house in New York. The first session of the meeting will be at 10 A. M., January 17, 1883.

Engineers' Club of Philadelphia, December 16th, 1882. Mr. Henry G. Morris in the chair.

Mr. E. F. Loiseau, introduced by Mr. Washington Jones, read a paper on the subject of his Artificial Fuel, which he exhibited, in process of consumption, in the Club room grate. After giving a short historical sketch of the manufacture of artificial fuel in Europe, where "briquettes" have been made for years past, Mr. Loiseau said that the aim of inventors has been to manufacture small lumps in paying quantities and, so far, the attempts have been failures. Sixty-eight fuel factories are successfully operated in Europe, but they all make brick-shaped lumps, too large for family use. Mr. Loiseau claims to have solved the problem, and states that the Loiseau Fuel Company, at Port Richmond, cannot supply the rapidly increasing demand for the small egg-shaped lumps (weighing about 2 oz.), which they manufacture at present.

Mr. Ashburner presented the following. I desire to call the attention of the Engineers' Club to the first finished maps and sections of the Anthracite Survey (2d Geol. Surv. of Pa), and to state briefly the general plan of publication which has been adopted. Thirteen sheets have just been engraved and printed, to illustrate the mining and structural geology of the Panther Creek basin, which lies at the extreme eastern end of the Southern Coal Field between the Little Schuylkill River at Tamaqua and the Lehigh River at Mauch Chunk. These illustrate the character of the charts which will be constructed in the other parts of the region.

The basis for the work is the surveys of the corporate and individual coal operators in the region, which have been made with so much care and precision. These are afterwards connected and extended by the field work of the Geological Survey corps. The charts are to be published of uniform size (26 by 32 inches,) and on the following scales:

1. Mine maps, showing the mine workings and the structure of the coal-beds, by underground contour curves 50 feet vertically apart. Scale 800 feet=1 inch.

2. Topographical maps of the surface of the coal basins, in contour curves 10 and 20 feet vertically apart Scale 1,600 feet=1 inch.

3. Vertical cross sections of the coal basins. Scale 400 feet=1 inch.

4. Columnar sections of the coal measures, showing the relation of the coal-beds and the character of the rock intervals. Scale 40 feet =1 inch.

5. Columnar sections of the individual coal-beds. Scale 10 feet=1 inch.

These sheets will be supplemented by others of a miscellaneous character.

Prof. L. M. Haupt presented a short description from Mr. John C. Trautwine, Honorary Member of the Club, and Mr. W. E. Babbit, C.V.R.R., of the floating drawbridge at Rouses Point, and also a drawing, with notes, of a wooden arch bridge across the Genesee River below Rochester, of 352 feet span, 54 feet rise, which failed by descending at the haunches and rising at the crown.

ENGINEERING NOTES.

The Great Wall of China.—An American engineer, who, being engaged in the construction of a railway in China, has had unusually favorable opportunities of examining the famous Great Wall, built to obstruct the incursions of the Tartars, gives the following account of this wonderful work: The wall is 1728 miles long, 18 feet wide, and 15 feet thick at the top. The foundation, throughout, is of solid granite, the remainder of compact masonry. At intervals of between 200 and 300 yards towers rise up, 25 to 40 feet high, and 24 feet in diameter. On the top of the wall, and on both sides of it, are masonry parapets, to enable the defenders to pass unseen from one tower to another. The wall itself is carried from point to point in a perfectly straight line, across valleys and plains and over hills, without the slightest regard to the configuration of the ground; sometimes plunging down into abysses a thousand feet deep. Brooks and rivers are bridged over by the wall, while on both banks of larger streams strong flanking towers are placed.

The canal of the Isthmus of Corinth, which was commenced on the 2nd May, 1882, with 800 men, mostly Italians, and which is to be finished in five years, will be like the Suez Canal, about 78ft. broad and 27ft. deep. For ships from the Adriatic 1fr. per ton will be charged; for ships from the Eastern Mediterranean 50c. per ton; for passengers, 1fr. each.

The projected tunnel under the St. Lawrence, at Montreal, is to be 16,000ft. long, or almost exactly three times the length of the Hudson River tunnel. and thirteen and one-third times that of the Thames tunnel. The greatest depth will be 166ft. below the entrances. The grades are not as steep as those of the Hudson River tunnel. The contract requires the completion of the work (by Mr. Romillard) in three years.

RAILWAY NOTES.

RAILWAY SPEEDS.—A German journal, *Die Verkehrszeitung*, has recently published a tabular statement of the maximum speeds of trains in Great Britain, Europe, and the United States. The table is inaccurate and incomplete to a very considerable degree, but it contains nevertheless some suggestive information. The American *Railroad Gazette* has supplemented it by a statement of the speeds of some of the fastest trains in the United States, and this statement has been in turn supplemented and corrected by two correspondents. The prominent fact which comes out is that the time taken to traverse any given distance between two places connected by rails shows that speeds are much slower than most people suppose. If we take, for instance, the run from London to Edinburgh, a distance of 397 miles *via* York, this is made in nine hours by Great Northern trains, the average speed being thus 44.1 miles per hour. From Euston the distance is 401 miles, and London and North-Western trains make the run in 10 hours, or 40.1 miles an hour. By the Midland Railway the distance is 404 miles and the time 10 hours 5 minutes, or very nearly the same speed. The German journal to which we have just alluded makes the distance 116 miles and the speed 41 miles an hour, which is an error. Some of the fastest trains in the world are those run between Leeds and London. From King's Cross the distance by the Great Northern is 186¼ miles. From St. Pancras by the Midland it is 196 miles. The fastest train on the Great Northern makes the run in 4 hours 30 minutes, giving an average velocity of 43.5 miles an hour. The fastest train in the world is the Flying Dutchman, broad gauge, which makes the run to Swindon at 53¼ miles an hour. The Great Northern trains run from London to York, 188 miles, at 48 miles an hour, and at least one train runs to Peterborough at 51 miles an hour. The run from London to Grantham has been made repeatedly al 51 miles an hour. On the United States railways the quickest time appears to be that made between Jersey City and Philadelphia, 89 miles, made at the rate of 47 2-3 miles an hour. There is not in the world a train timed to run at 60 miles an hour, although it is, of course, certain that that velocity is often exceeded. If a speed of 60 miles an hour could be maintained continually between London and Edinburgh, the journey would occupy only 6 hours 36 minutes; and allowing for three stops of ten minutes each on the route, the time would be under 7½ hours, instead of ten hours. So far as the machinery of a railway is concerned—by which we mean the road, the rolling stock, and the signals—there is nothing to prevent an average speed of 60 miles an hour being maintained. That it is not attained is certain. It is worth while to inquire why.

The first essential to great average speed is that the runs shall be long; that is to say, there must be long intervals between stopping places; and this is necessary not so much because of the time lost at a station, as that spent in getting into and out of it. The station must be approached and left at a comparatively slow speed if for no other reason, than because it would be dangerous to do otherwise. The next essential is that the train shall be light, because the engine ought to be able to maintain a high speed when running up hill as well as when on a level. High speed trains cannot fully avail themselves of the compensating effects of inclines. Let us suppose that the maximum speed attained must not exceed 70 miles an hour. Then if the up and down inclines balance each other, the speed of the train must never fall below 50 miles an hour or else the average velocity cannot be 60 miles an hour. If the maximum speed permitted was 60 miles an hour, and the average speed 40 miles an hour, then trains might ascend the banks at 20 miles an hour, and still keep time. It would not be advisable to run at a higher speed under any circumstances with ordinary rolling stock than 70 miles an hour, and there are no locomotives built which would never run at less speed than 50 miles an hour between London and Edinburgh with a greater load than about 55 tons; that is to say, 75 tons for engine and tender, and 45 tons for four passenger coaches and a brake van, or in all 130 tons. The maximum number of passengers carried would be about 200, so that the dead weight moved would be 13 cwt. per passenger. The first class fare now is 57s 6d. If we call it £3 we should have £600 as the total returns, or, say, 30s. per mile run. It is not at all impossible that such a train might be made to pay. No doubt there would be now and then a strong temptation to add another carriage, but to give way would be to ruin the whole scheme. Its success would depend entirely on keeping the load to be hauled so small that engines might be worked at a speed never under 50 miles an hour. Three engines would be employed on the run, each making a distance of about 135 miles; and we cannot see that any great difficulty would be encountered in doing this. It must not be forgotten, however, that although the load would be small, the excessive speed would make large demands on fuel and water. It would not be safe to reckon on a less consumption of coal than 40 lbs. a mile, so that the tender would have to carry about 8 tons; and allowing that each pound of coal evaporated 10lbs. of water, including waste, the tank must hold about 5,400 gallons, or 24 tons of water. It would, however, be highly objectionable to attempt to carry such a load as this, and Ramsbottom's water troughs supply a way out of the difficulty, and a tender capacity of 2,000 gallons would be ample. On a grate of 20 square feet the consumption would be at the rate of 120 lbs. an hour, which is much too fast for comfortable or regular

working, and a special design of engine would be absolutely indispensable, one with a very long grate being required to provide the requisite surface. It will be seen that the engine which could comply with the required conditions is yet to be designed. The fastest train in the kingdom is, as we have said, the Flying Dutchman, and this is 7ft. gauge. Many persons think that the utility of the wide gauge in this case is that it prevents risks of oversetting. This is an entire mistake. There is no speed, perhaps, at which a train can run which would entail the least risk of such an accident on a good road of 4ft. 8in. wide. The speed advantage conferred by the 7ft. gauge is that it permits an enormous boiler—and particularly an enormous grate—to be used. The Great Britain, for example, has nearly double the heating surface of powerful express engines on other lines, and there would be no difficulty in increasing its grate surface up to 35 square feet, if necessary. Engines of this class have 1,958 square feet of heating surface and 21 square feet of grate. The Flying Dutchman consists of engine and tender weighing 65 tons 18 cwt., one eight-wheeled van 18 tons, and five eight-wheeled coaches weighing 88 tons; total, 169 tons 18 cwt.

The principal reason why high speed trains are not run is that it is difficult to provide locomotives of sufficient steaming power. It is evident that if an engine could be produced which could do as much work with 20 lbs. of coal per mile as the existing engine does with 40 lbs., an enormous advantage would be gained; but it is not at all probable that any great saving can ever be effected in the consumption of coal. Mr. Webb's compound engine may do something, but the existing locomotive is "bad to beat" in this respect. The designing of a locomotive to make long runs at 60 miles an hour average speed presents, however, an interesting problem, which will, perhaps, be attacked some day. We have pointed out briefly a few of the more important conditions to be kept in mind. Locomotives are so absolutely interwoven in the dimensions of parts, if we may use the words, that to augment the area of a grate by one-half will suffice to almost revolutionize the proportions of an engine. That a good engine of this kind could be made we have, however, no doubt. It remains to be seen whether England or America will have one first. Every year the number of miles travelled by each member of the population increases, and this growth of travel will no doubt rapidly encourage the running of long distance express trains, by which alone can high mean speeds be attained or made to pay.—*The Engineer.*

The Prussian Minister for Public Works has given orders that cast steel rails should be laid in the various curves which occur on the newly-opened Stadt-Bahn, or local railway of Berlin. The *National Zeitung* states that on account of the heavy traffic and consequent wear of the rails at such points, it has been ordered that new rails are to be laid every three months at these portions of the line. If this is correct, either the rails must be very poor or the traffic is different to that anywhere else on the Continent.

Messrs. Van Der Zypen Bros., of Deutz, near Cologne, who employ 120 men in the smithy, are turning out railway wheels by the hydraulic press at the rate of 15,000 a year, finished and mounted, for Germany, Russia, Italy, Turkey and Australia. The spring ring is generally used for fixing the tire, and it is exclusively adopted by the German Government. The firm is now giving special attention to the manufacture of a carriage wheel like the Mansell, but of compressed paper, which is found to give good results.

ORDNANCE AND NAVAL.

Discoid Projectiles for Cannon.—Before the adoption of rifled cannon with oblong projectiles, several eminent artillerists conceived the idea that good effects might be obtained with a disc-shaped projectile, having a rotation about its axis of figure, and fired so as to present its equatorial surface to the air. Experiments were indeed made in France, not very long ago, with a portable arm and discord projectiles. The chief advantages hoped for were diminished air resistance, great stability of the axis of rotation, and consequent accuracy, penetrating power, etc. As these advantages were largely had with oblong projectiles, the matter dropped. In the *Revue d'Artillerie*, however, Captain Chapel has lately called attention to a new property of these discoid projectiles, and thinks it might, in some cases, prove valuable. This a tendency in the descending path, *to return*, and to strike the ground at an angle above 90 degrees. In this way, artillery placed opposite a line of defence would be able to strike the defenders from behind, and the ordinary methods of defence would require modification.

Armor-plate Tests.—We learn from the *Times* that Messrs. John Brown and Co. (Limited), Atlas Steel and Iron Works, Sheffield, have received orders for the entire armor for a new war-ship, to be called *Dmitri Domskoy*, to form one of a series of vessels to be constructed next year. The *Dmitri Domskoy* will not be an exceptionally powerful man-of-war, but the Russian Government are now making inquiries with regard to several very large ships which are to be commenced in 1883. The Sheffield manufacturers of compound armor expect that the results of the trial at Spezzia will be in their favor, and the order for the *Lepanto*—the sister ship to the *Italia*—is looked for very soon. One result of the trials will be the use of a larger number of bolts in the plates than have hitherto been used. The French all-steel plate, after it was smashed, kept its pieces together by means of its bolts, while the Sheffield plates fell away. It is obvious that the plates, if kept together on the ship's side, would still afford some protection against further shots, though there is no doubt that the additional bolts will weaken the armor. Austria is also making inquiries for armor on the compound

system. The plates for Russia will be of the "Ellis" type. The other manufacturers, Messrs. Charles Cammel and Co. (Limited) completed some time ago the plates ("Wilson" patent) for the Russian vessel *Vlidimir Monomach*, of which the *Dmitri Domskoy* is the sister ship. Messrs. Cammel and Co's plates came very satisfactorily out of the tests at St. Petersburg on the 24th ult. Two armor plates, 12 inches thick, each weighing about 12¼ tons, were tested. One of the plates was made of steel by Messrs. Schneider, Creusot Co., France, and the other was on Wilson's compound system, steel-faced, made by Cammel and Co. The gun used was an 11-inch Aboukof breechloader, range 350 feet, chilled cast-iron projectiles, each weighing 615 lb. The first shot, which had a striking velocity of 156 feet, broke the creusot plate into five pieces, the penetration being 13 inches. The second shot penetrated 16 inches, and broke the plate into nine separate pieces. The third shot still further broke up the plate, and carried away fully one-third of it. the projectile penetrating the whole target, and was found 740 yards to the rear, being apparently uninjured. The Wilson (Cammel's) plate was then tested under similar conditions. The first shot produced only a few slight cracks on the steel face of the plate, while the projectile was completely broken up, a small portion of the heading remaining in the plate. The penetration could not be obtained, but was believed to be 4 to 5 inches. The second projectile produced only a slight penetration, with no preceptible damage to the plate itself. The third shot was not fired, four of the bolts which held the plate being broken, and it is necessary to replace them before the experiment can be completed. It is clear from these trials, as well as those at Spezzia, that the Creusot Company are wise in using a large number of bolts to keep the plate together after it is partially destroyed. The result, so far as the test has gone, is very favorable for the Sheffield plate.

ARTILLERY TRIALS AT SPEZZIA.—Some experiments are being carried out at Spezzia with the 100 ton muzzle-loading gun which was fired last week at a compound plate made by Cammel, a compound plate by Brown, and a steel plate by Schneider, of Creusot. All three plates were of the same thickness, viz., 19 inches, with 4 feet of wood backing. The velocity of the shot was calculated to perforate a wrought-iron plate of the same thickness. According to the *Army and Navy Gazette*, the Cammel plate was not perforated, but the corner of it was separated by cracks through the whole thickness. The Schneider plate was less penetrated than the Cammel, and no cracks were apparent. The Brown plate was less penetrated than the Schneider, and only cracked in the steel facing. The wrought iron acted as a cushion; it bent a little, and so absorbed the force of the blow. On the experiments being continued, the first round was fired at Brown's plate, which broke into four large pieces and many small pieces. About three-quarters of the plate fell from the target, and the backing was much damaged. The second round was at Cammel's plate, which broke into five large and many small pieces, and fell completely from the target. The energy of the projectile in both rounds was about 33,900 tons. The plates were not penetrated in either case, but dashed to pieces. A ship would leak through the injury caused to the backing. The Whitworth shot fired at the Schneider plate failed to penetrate more than 8 inches, but completely broke up the plate, and drove in the backing. The Gregorini steel shot, fired afterwards, left the Schneider target a mass of ruin The results of these experiments are considered to reopen the whole question of naval gunnery and armor. The gun has not yet been fired with its full battering charge.

MR. JOHN HAUG, in presenting the Engineers' Club of Philadelphia with a copy of Lloyd's Rules for Iron Ships, stated the number and tonnage of ships built in Great Britain in 1881 as follows:

	Built in 1881.			Lost in 1881.	
	Number.	Tonnage.	Material used.	Number.	Tonnage.
Steel steamers.........	34	68,366	32,000 tons.	1	1,536
Steel sailing vessels.....	8	3,167	1,500 tons.		
Iron steamers.........	411	590,508	300,000 tons.	189	138,370
Iron sailing vessels.....	50	68,650	34,000 tons.	52	48,936
Wooden steamers .	30	1,659		18	1,704
Wooden sailing vessels.	259	16,448		821	168,579
Total......	787	748,793		1081	354,125

The principal changes in the rules for 1882 have been in water-tight bulkheads, of which more are now required in longer and larger vessels, and they are to be extended to the principal upper deck.

Vessels of extreme proportions (over 11 depths in length) have to be better strengthened in their top and bottom members, by doubling strokes, &c.

Treble rivetted buttstraps are required to a greater extent, as forming stronger joints.

The rules for boilers, machinery, pumping arrangements, spare parts for machinery, &c., have been extended and improved, with a view to greater safety at sea.

As ship-building of steel is increasing, a reduction of 20 per cent. from the scantlings required for iron is permitted, giving ships so much more carrying capacity. A complete set of rules for testing all materials, insures uniform quality in steel used. Steel castings, by the Siemens-Martin or Bessemer process are also now used, in place of large and expensive forgings, for stemports, rudders, stems, &c., and they have been found strong and tough; they are less expensive than scrap-iron forgings, and the risk of bad welds and inconvenience of rough and uneven shape is avoided.

The latest circular issued by Lloyd's Register offers to fix a proper load line for each vessel, according to its style, form, &c.; thus the rules not only provide for its proper strength, but

also its sea-going qualities, &c. This is of the greatest importance, in view of the many disasters that have occurred from the want of those qualities.

Mr. Haug also exhibited and described drawings of his own and other valve gears.

The one patented by Mr. Haug belongs to that class in which the longitudinal and lateral motion of an eccentric, or other rod, are both utilized for obtaining a variable cut off and reversing motion. In comparison with F. C. Marshall's and Joy's valve motions, which have both been extensively used, this valve motion consists of fewer parts and joints, can be more compactly arranged, and has the least amount of motion to all its parts, being thus more accessible for oiling, &c. Compared with the link motion, it is considerably simpler, has less friction (all sliding friction being replaced by bolt friction), requires less force to reverse, and will consequently throw less strain on all its members.

IRON AND STEEL NOTES.

Steel Iron.—The question of producing a metal possessing the physical properties of both iron and steel has for some time past received attention at the hands of practical metallurgists and others. One of the latests workers in this direction is Professor M. Keil, who has succeeded in producing a compound metal which is stated to possess the characteristics of both metals. The professor, in giving his experiences on the subject states that the difficulties can be obviated only if the two materials are intimately united into a whole. After many experiments, success has, it is claimed, at last attended them, and a material has been produced answering every requirement, and to which the name of "steel-iron" has been given, The following five descriptions have been made :—(1) steel by the side of iron ; (2) steel between two layers of iron; (3) iron between two layers of steel; (4) the core of steel, the surrounding shell of iron; (5) the core of iron, the surrounding shell of steel. This steel-iron is manufactured in the following manner. A cast-iron mould is divided into two parts by a thin sheet of iron securely fixed in it. The fluid steel, as well as the fluid wrought iron, which have been freed before smelting from substances preventing welding, are poured at the same time, and in the same quantity, into this double mould; the separating plate serving as the medium welding both parts, steel and iron, completely together, so that they form an inseparable whole. The plate serves as a separator and a welding agent at the same time. The success of the operation depends upon the quality and the thickness of the plate. The latter must be of a certain thickness, to prevent the two glowing and liquid masses burning through it ; and it must not be too thick, so that they are able to bring it up to welding-point while rising in the mould. The dimensions of the plates depend upon experience, and, naturally, are regulated by the dimensions of the castings. The manufacture of the above-mentioned five kinds are the same in principle. In numbers 2 and 3, however, the mould is divided into three equal parts, by two strips of plate ; in numbers 4 and 5 the core is formed by a sheet-iron pipe standing in the middle of the mould. It is stated by Professor Keil that the product thus obtained may be used for a great many purposes. Steel upon iron will be useful for rails, armor-plates, and anvils, the hard steel face reducing wear and tear, and also, as in the case of thief-proof safes and armor-plates, withstanding the attacks of even the hardest drill, while the iron prevents cracking consequent upon heavy blows. Parts of machinery and tools which are subject to powerful pressure, and are exposed at the same time to great vibration, are best made of the material with tough core and hard surface. The wear and tear would be slight, while the soft core imparts considerable strength and prevents fractures. From what has been said respecting the quality of this description of steel-iron, it will be seen that the extent of its application promises to be a wide one, partly on account of its undoubted excellence, partly also, from its many qualities, because it may be used for a great variety of manufactures.

The Dephosphorization Process.—It may be said that the problem of manufacturing steel from phosphoric pig has been solved, both in the converter and reverberatory furnace, by the employment of a magnesian lime lining. The elimination of phosphorus is by this means as satisfactory as possible, that of silicon is nearly complete, and even sulphur is removed to a large extent. From comparative analyses made in August and September, 1881, it is shown that basic steel is purer than acid steel, and has a more uniform composition. The tensile mechanical tests show that the results furnished by the basic Bessemer steel are sensibly more regular than those given by acid steel. Rails manufactured from these two varieties of steels give similar results under the static and dynamite tests. The inconvenience caused by the presence of blisters on the ingots, met with at starting the basic process, has been overcome by raising the temperature of the metal at the moment of pouring. The state has therefore been led to accept indifferently rails, made from either variety of steel. In the reverberatory furnace, the manufacture of basic steel is more easily carried out than in the conventer, and the dephosphorization is more complete. Metallurgists, therefore, are now in possession of two different methods of manufacturing steel, both in the converter and in the reverberatory furnace. The one consisting in treating pure products in an apparatus furnished with a siliceous lining ; the other in treating impure products in presence of a basic lining. The following question, therefore, naturally seems to present itself. Since, other things being equal, linings of magnesium lime offer a more complete purification than siliceous ones, why not treat pig metal, even that resulting from pure ores, in a basic apparatus ? By this means these latter should give steel of very great purity. So far as concerns the reverberatory furnace, it appears probable that

in the majority of cases the siliceous hearth will be replaced by one of magnesian lime; the carrying out of a basic operation presenting, as we have explained above, no difficulties. But with the converter the case is different; it would not be found advantageous to treat in a basic-lined converter metal such as that used in the acid process, since this metal is high in silicon; and, as we have shown above, this circumstance presents a grave obstacle. On the other hand, if the blast-furnace charges were so regulating that the resulting pig was but slightly siliceous, there would probably not be sufficient heat generated by the molecular combustion to assure the liquidity of the bath and slag. The treatment of pure metal, therefore, in the basic converter, is not without drawbacks. It might, it is true, be arrived at by the employment of the transfer method; that is, by removing silicon in an acid vessel, then running the metal into a basic lined vessel, and there finishing the blow. But this process offers the great drawbacks of being expensive and complicated. We will not, therefore, enlarge upon this question, the solution of which belongs to the future.

Thomas-Gilchrist Process.—There has been much discussion this week upon the iron exchanges in the Midlands, touching the relative merits of steel and iron and the progress of the Bessemer basic method. The twenty-five converters now being erected upon the Continent and the nine in England will, when at work and added to the converters now in operation, bring up the production to 1,196,600 tons per annum. Such an augmentation upon the current yearly continental and home make, which reaches 572,604 tons, should be encouraging to the patentees, while it certainly gives much additional value to the poorer iron-yielding ores wherever they are found Already the tap-cinder of the ironworks is worth more money; and in the United States accumulations are being stored at a figure which it would never before have been thought such bye products would realize. On 'Change in Wolverhampton on Wednesday, Mr. Alfred Hickman, blast furnace proprietor, who has taken up the system there with spirit, stated that he was about to erect two more furnaces at Spring Vale, close to the land selected for the works at which the Staffordshire Company will make basic-Bessemer steel. Mr. Joseph G. Wright, the chief proprietor of Monmoor Ironworks, in the same town, who had taken part in the discussion last week upon the paper which Mr. Gilchrist read at Dudley, confirmed on 'Change his statement at the formal discussion, that the cost of puddled bar in Staffordshire made from pig at 42s. per ton was 80s., and held that whatever might be the cost of the raw ingot, it would require 5s. 6d. a ton to be spent upon it before it was brought into an equally forward state with the puddled bar for subsequent manufacture. This computation leaves the difference in favor of crude steel from the same class of pig at 4s. 8d per ton, assuming Mr. Gilchrist to be correct in his estimate of 69s. 8d. as the cost of the ingot; and reduces Mr. Gilchrist's estimate in favor of steel by only 7d. per ton. But Mr. Wright is not, at the present stage of his knowledge of the capability of basic ingot iron, prepared to admit that that material would be equal to the puddled bar in in all-around value. The operation of the Staffordshire Steel and Ingot Iron Company will supply the ironmasters of the country with the precise statistical facts which they seek before they can make up their minds upon the merits of ingot iron is a raw material not for rails and ship-plates and sheets alone, but also for the general run of sorts demanded by the merchant. Meanwhile it is interesting to note that by the formation of "The Dephosphorizing and Basic Patents Company, Limited," with a capital of £80,000, for the purpose of purchasing "letters patent granted respectively to Messrs. Sidney Gilchrist-Thomas, G. J. Snelus, E. Riley, P. C. Gilchrist, and H. C. G. Harmet, for inventions relating to the manufacture, purification, and dephosphorization of iron and steel, and the manufacture of furnace lining and lining material," the several patentees of the Thomas-Gilchrist process would seem to be adjusting in a business-like fashion the ratio of their respective claims to the emoluments of the combined invention.

BOOK NOTICES

PUBLICATIONS RECEIVED.

Monograph of the Central Parts of Nebula of Orion. By Edward S. Holden. Washington: Government Printing Office.

Transactions of the American Institute of Mining Engineers. Report of the Colorado Meeting.

The Naval Use of the Dynamo Machine and Electric Light. By Lieut. J. B. Murdock, U. S. N.

Report of the Chief Signal Officer for 1880. Washington: Government Printing Office.

A School Course on Heat. By W. Larden, M. A. London: Sampson, Low & Rivington. Price, $2 00.

The scope of this work is so fairly stated in the preface, that an abstract from the latter will suffice to explain the character of the treatise:

"It is assumed that the reader has no previous knowledge of physical science at all, but is being introduced to it for the first time. Hence the reasoning and explanations are at first very elementary in character, greater brevity being only gradually attained.

"For the same reason the writer has thought it well to introduce matter not actually belonging to the subject of heat, wherever such introduction seemed necessary to the clear understanding of any matter that does belong to this subject.

"The mathematical parts are treated in a very careful manner. At the end of each chapter are given questions on the subject of that chapter."

To which we may add that so far as we know it is the book best calculated to satisfy the young artisans and engineers who wish a reliable aid to their efforts to supplement their school course of study.

The Transit Instrument. By Latimer Clark, M. I. C. E. London: Published by the author. Price, $2.00.

This is designed for amateurs who are inclined to practice with the astronomical transit in the determination of time.

The theory of the problem is clearly stated, and all necessary instructions are given for the practical solution. Diagrams descriptive of the construction and adjustments of the instrument are given.

Tables giving the Greenwich time of transits of the sun and of certain fixed stars are given at the end of the volume.

The book will prove valuable to gentlemen of leisure and of scientific inclinations.

Examples of Iron Roofs. By Thomas Timmins. Vol. I. London: Thomas Timmins. Price, $5.00.

A thin quarto volume of folding plates containing designs of iron roofs drawn to scale. The details of intersections are also given to an enlarged scale.

A graphic solution of each form is also given on a single plate.

Tables of strengths of columns and struts are also added.

The whole constitutes a convenient guide to the builder who desires to construct either of the given forms.

Life of James Clerk Maxwell. By Lewis Campbell, M. A., LL. D., and William Garnett, M. A. London: Macmillan. Price, $6.00.

This volume of 661 pages is in three parts, of quite unequal interest to the average reader. The first part presents the biography; the second, the contributions to science, and the third the original poems of Mr. Maxwell written at different times between 1844 and 1878.

The biographical portion, of course, presents outlines of some of the earlier scientific papers, partly by way of letters and partly as a narrative, of the growth and development of the hero.

There is much of the first and second parts that scientific readers will find intensely interesting. There is enough in the third portion to demonstrate that the subject of this interesting memoir was not a result of one-sided development. The book is handsomely printed and is well illustrated.

MISCELLANEOUS.

The Ordnance Survey of Scotland.—It has just been announced that the Ordnance Survey of Scotland and the adjacent islands, after being in progress over thirty-seven years, has been entirely completed. In order that this important work might be overtaken within a reasonable period of time, Scotland was divided into three sections; and during the last few years the average number of men employed upon the work has been from eighty to ninety. The bulk of the surveyors were civil assistants, but the superintendents of the survey were officers of the Royal Engineers—the Edinburgh division having been under the command of Captain Kirkwood during the past five years. With the exception of the counties of Edinburgh, Kirkcudbright, Wigtown, Haddington and Fife, which were surveyed and published on the 6 in. scale before the larger scale was adopted, the whole of the agricultural districts have been surveyed, and the results are now published on the scale of $\frac{1}{2500}$th, or about 23 in. to a mile. The uncultivated districts of the Highlands and islands are done on the 6 in. scale—the publication of the island portions, by the more rapid progress of zincography, being now quite completed. In these maps the height of every hill is given, and its contour is drawn, and, as showing the careful nature of this important work, it may be mentioned that all antiquities are noted, and, by the employment of different styles of lettering, their Roman, Druidical, Saxon, or Norman character is indicated. The engraving of the 1 in. general map, which, from the nature of the processes employed, is necessarily much slower, is now proceeding apace, and the sheets are being quickly published.—*Engineering.*

Manufacture of Artificial Parchment.—Messrs. Herold & Gawalowski, of Brunn, make as follows, a strong, artificial parchment, impermeable by water, and capable of serving for the diaphragm in osmotic operations on solutions of impure sugar, &c. The woolen or cotton tissues are freed, by washing, from the foreign substances, such as gum, starch, &c., which may cover them. They are then plaeed in a bath slightly charged with paper-pulp; and to make this pulp penetrate more deeply, they are passed between two rollers, which slightly compress them. The principal operation consists in steeping the product for a few seconds in a bath of concentrated sulphuric acid, after which it undergoes a series of washings in water and ammoniacal liquor, until it has lost all trace of acid or base. It is then compressed between two steel rollers, dried between two others, covered with felt, and finally calendered, when they are fit for use.

An interesting experiment on the transmission of power by electricity was made at the Munich Electrical Exhibition recently. Two Gramme dynamos were used, one located in Munich and the other at Meisbach, 35 miles distant. They were connected by an ordinary galvanized iron telegraph wire 4½ millimeters or about one-sixth of an inch in diameter, being, in fact, a telegraph line placed at the disposal of the experimenters by the German Telegraph Administration. A second wire was used instead of the earth for the return circuit. The total resistance of the wire was 950 ohms. The resistance of each dynamo was 470 ohms. The total resistance of the working circuit was therefore, 1,890 ohms. When the generator at Meisbach was driven 2,200 revolutions per minute, 1,500 revolutions per minute were obtained on the receiving dynamo at Munich. The per centage of power utilized at this dis-

tance was therefore $\frac{1500}{2400}$ = over 60 per cent. of that expended, and at the time of the experiment a heavy rain was falling, which must have considerably impaired the insulation of the line.

The Canal Age.—Apropos of the movement at present in progress for the construction of the ship canal between Liverpool and Manchester, a writer in an ably-conducted northcounty paper very pointedly draws attention to the probability of the remaining years of the nineteenth century being spoken of in history as "The Canal Age," his opinion being that the present indications are in the line of a large extension of inland water carriage by means of canals, and that the problem of quick international communication has now been solved, almost to "finality," by steamships and railways. Whether or not finality has been reached by those two great civilizing agencies, it is undoubtedly the case that the prospects of canalization on a great scale for the immediate future bulk very largely in the eyes both of commercial men and of engineers. Not only is there in hand the project of the Liverpool and Manchester ship canal, with its probable cost of £6,000,000, estimated to make an income enough to pay the shareholders if only a single ship of 4,000 tons pass both ways every day, but there are also various other great inland water-way schemes, of national and international importance, either in hand or actually carried into execution. The sum of £40,000,000 has recently been voted by the French Parliament for inland canalization works, and it is thought that at least five times that sum will have been spent upon such works before the system of inland water carriage in France has been completed. Many of our readers are familiar with the great engineering works which have resulted in the completion of a ship canal connecting the city of Amsterdam with the sea; and they scarcely require to be informed that it has proved to be a remarkable success, commercially and otherwise. Additional canals are likewise in course of construction or projected in Belgium, a country well adapted by nature for such works. Then, going into Prussia, we find that there is a prospect of a speedy beginning with the canal scheme which aims at connecting the Rhine, the Weser, and the Elbe, at an estimated cost of upwards of £7,000,000. Proceeding further east we should notice another proposal which bids fair to become an accomplished fact in the early future, which is a scheme to cut a ship canal to connect the river Danube with the Oder, and thereby joining the Black Sea with the Baltic. But in Russia it is proposed to enter upon even a much larger canal scheme, to wit, one to connect the river Dneister with the Vistula, and thereby to bring the great ports of Odessa and Dantzic into direct communication. One of the probabilities of the next few years is an Egyptian project, namely, a great inland waterway to rival the Suez Canal; and a ship canal through the Isthmus of Panama may be regarded as one of the certainties of the immediate future. More or less similar schemes are likewise contemplated in other parts of the world—in Canada, in Southern Europe, Southern Asia, etc.—*Engineering*.

In a report to the Board of Trade on a collision which occurred on the 30th ult. at Crewe Station on the London and Northwestern Railway, Major Marinden says, "In conclusion it should be observed that, as is usually the case when a driver has more than one class of brake at his command, some valuable time seems to have beon lost in the application of them. Had the train been fitted with a quickly-acting continuous brake, fitted to the engine and all the vehicles, the whole train might have been braked by the same simple action which was taken by the driver when he applied his engine steam-brake, and the speed would have been very much less than it was, even if the train had not been altogether stopped before any collision took place," and in concluding his report on the collision which occurred on the 28th of September at Chester, on the London and Northwestern Railway, when fourteen passengers and Post-office sorters and the front guard of the mail train were injured, says:—"It is worthy of remark that, although there were sufficient time for the driver to reverse his engine, and to whistle for the guard's brake, for his fireman to apply the tender-brake, and for the front guard to skid the wheels of his brake-van by means of his hand-brake, yet no attempt was made by either driver or guard to apply the patent chain brake, which, according to the regulations of the company, is to be used as an emergency brake only. This is quite in accord with my experience of the action of the servants of this company in other like instances, and it seems to me that it furnishes a strong argument in favor of the habitual, and not the casual, use of whatever continuous brake is adopted by any railway company."

One of the chief, or the greatest difficulty met with in plating iron an d steelwith other metals has been that, in the absence of a flux, the oxidation of the metals during the preliminary process of heating prevented a perfect weld. But it is stated that by the use of a metallic chloride vaporizable at a red heat, such as chloride of zinc or muriate of ammonia, a perfect union may be obtained, and this is better assured if a coat of tin-foil be placed between them as a welding medium. A flat bar of iron or steel treated in this manner and enveloped in the plating metal is brought to a red heat and drawn to any required dimension between cold rolls, and is said to be in use in Prussia.

At a recent meeting of the Berlin Polytechnic Society, Dr. Franke exhibited specimens of a condensed fuel prepared with a solution of dextrine and saltpetre, which, according to the quantity of the latter substance, burns from six to twelve hours, and is considered to be specially applicable to the heating of railway carriages and to other similar purposes. Such a fuel will, doubtless, save some trouble in German railway carriages, which are heated by cakes of a low-burning fuel placed in small horizontal boxes in each carriage, the carriages having iron frames and panels. The heating is much more efficient and less inconvenient to travelers than our makeshift hot-water boxes.

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NO. CLXXI.—MARCH, 1883.—VOL. XXVIII.

THE CONSTRUCTION OF ELECTRO-MAGNETS.

By TH. DU MONCEL.

Translated from the French for VAN NOSTRAND'S ENGINEERING MAGAZINE.

III.

VIII.—CONDITIONS GOVERNING THE CONSTRUCTION OF ELECTRO-MAGNETS.

The calculation of the elementary parts of electro-magnets is not all that is necessary. To secure the best results, it is also necessary to take into consideration the secondary causes which can influence them and their sources of action.

This question will now be considered, and will recall the conclusions reached by the writer, from numerous experiments tried during the past twenty years.

1*st. Conditions of electro-magnetic force which are dependent upon exterior causes.*—Descartes and many later physicists have shown that if a mass of iron be brought in contact with one pole of a bar magnet, that the attractive force of the other pole is augmented, and that the increase is somewhat proportioned to the mass. M. Dub, who wished to include a statement of this fact in the laws of electro-magnetism, considers that this increase is a result of the general law, that the force increases with the increase of length of the magnetic core. But this increase should by the law be proportioned only to the square roots of the lengths; whereas, according to the author's experiments, the increase is enormously greater, since the force is frequently more than trebled. Furthermore, the writer has shown that this increase is produced by iron masses of all shapes, some of which do not increase the length of the magnet to a notable extent, and also when not in immediate contact with the magnet proper. It has been observed during these experiments that the development of a magnetic surface has much to do with the phenomena. Now, the author's conclusion is, that the increase of energy is due principally to the effects of magnetic condensation developed by the contact of the two pieces, and that the stimulation of the free pole is a secondary effect, since the two polarities of any magnet bear a constant relation to each other.

This conclusion has been experimentally verified by alternately placing upon a long, soft, iron rod helices of different lengths but composed of the same lengths of wire. The helix which covers the entire rod is the one affording the least attractive force, although it has the greatest number of turns of wire. The helix which has the maximum effect is that which covers only one-third of the length, measured from the active pole.

Now, when these same helices are ap-

plied to iron rods of the same length as themselves, the effect is altogether different. Then the force increases as the length increases, but not in proportion to the length. The forces seem to increase in an arithmetical proportion, while the lengths are in geometrical ratio; and the rate of this increase seems to bear a direct relation to the number of cells of the battery. Only these last results will be here specified as applying to a particular case.

The effects about to be cited illustrate the action of electro-magnets when charged with a single helix upon one of the branches only, a kind upon which the author was the first to experiment, and which he calls *electro-aimants boiteux* (lame or crippled magnets), and which are nearly as energetic for the number of coils of wire as the magnets of two helices.

2*d*. *Relations of the electro-magnetic force to the form and disposition of the armatures.*—The author sums up his conclusions from his experiments in the following manner:

(*a*). The attractive force of electro-magnets, whether great or small, is greatest when that surface of the armature which is directly acted upon is most developed, and when the mass of iron exposed to the magnetic influence is most completely subjected to the magnetic forces.

(*b*). It results from this that the attraction of electro-magnets of two branches for flat prismatic armatures is greatest when the armatures are presented flatwise, if at a little distance from the magnet, while the converse is true for the condition of contact, and the greatest force is manifested when the armature is presented edgewise. For the attraction at a distance the ratio of the effects of the two positions may be as 59 to 92.

(*c*). The arrangement by which the armature is caused to rotate about a pivot near one of the poles is better than that by which it is allowed to move parallel to the line joining the two poles, as when it is made a cross-bar at the end of a tilting lever. The advantage of the former method is especially manifested in the case of magnets with a helix on one pole only, in which the forces developed under the two methods are in the ratio of 125 to 64.

(*d*). Prismatic armatures are attracted with a force proportioned to their extent of surface; but the *form* of the surface has a great influence, as the mean distance of the whole surface from the poles may vary considerably with the shape. Thus a cylindrical armature is attracted with much less force than one of prismatic form having the same amount of surface; the ratio of the forces being as 44 to 85.

(*e*). By reason of an analogous reaction to this last, the *lateral* attraction of electro-magnets, where the cores project a little way from the helix, is much less energetic than the attraction in the direction of the axis of the cores; the ratio being about as 18 to 33.

(*f*). Armatures made of permanent magnets aid the attraction only when used at a distance from the poles, and are kept parallel to a line joining the two poles. For other positions they are a disadvantage, as magnetic action, when exerted upon steel, is much less than upon iron.

(*g*). The attractive force due to the momentary closing of a circuit is, for any given distance of the armature, much greater than that arising from the constant action of the same current when a counter force is opposed to it. This fact may be referred to the effects of living forces, and to the polarization in the cell. The ratio of attractive forces is as 136 to 95.

(*h*). When the force of an electro-magnet is divided among several armatures, the total attractive force is augmented, but the force exerted upon any one armature is less in proportion as the number is greater. This increase of total force, however, is manifested only within certain limits, which are reached when the mass of the armatures is equal to that of the electro-magnets.

(*i*). The attractive force between an electro-magnet and an armature which has not been previously employed, is for a given current much greater than for the same magnet and armature, when they have been once subjected to the current. And to obtain a force nearly equal to that primarily produced it is necessary to reverse the current; but the maximum force exists only at the first closing of the circuit.

(*j*). The attraction of electro-magnets

at a distance is weakened when from any cause the first closing of the circuit has not been followed by the complete attraction of the armature. This is due, as in the preceding case, to the effect of residual magnetism.

(*k*). The repellant force exerted by electro-magnets upon magnetized armatures is far from corresponding with the attractive force which is produced by reversing the poles of the magnet. This fact, which was observed in the early experiments upon electro-magnetism, and which was for a long time discussed by Mussembroeck and Abbe Nollet, is easily explained as an example of induction by the magnet, which tends to develop upon the armature a polarity opposite to that which exists in the magnet. Now, in attracting, the action of the induction is to increase the dynamic effect, while in repulsion it diminishes it. (*Etudes du Magnetisme*, p. 103.)

(*l*). When the iron core of an electro-magnet extends beyond the helix the force is diminished ; but if the poles be fitted with extension pieces in the inter-polar space, the force is augmented, and the maximum effect is reached when the space separating the auxilliary pieces is about one-fourth of the entire interval. This increase follows from the increase in the attractive surface of the poles, which then correspond to an armature.

(*m*). According to the experiments of M. Dub, the best condition of electro-magnets relating to the dimensions of their armatures are obtained when the different parts (separate branches, armature and bottom bar) are equal.

(*n*). This conclusion is naturally subordinate to conditions of application; for it is certain that if we desire prompt action in the armature, that it must be light; but we can then supply the deficiency of weight by reinforcing the poles of the magnet as above described.

3d. Conditions depending on the mass of the iron core.—The conditions of magnetic force depending on the diameter of the magnet and the degree of saturation may, under certain circumstances, be quite antagonistic; for though we gain force by increasing the diameter of the core, we may be subjected to loss because, by reason of size, the core is not charged to saturation, and then the central inert portion becomes, by a secondary action, like another armature. It has been attempted to obviate this difficulty by employing tubular magnets, which allow of easy magnetizing and demagnetizing; but it was found that there was a loss of force by this system, and which led to their rejection for a time. The author in 1862 began to study the conditions of force in this kind of magnet, and after many experiments presented a memoir to the *Académie des Sciences* containing the following conclusions:

(*a*). The greater force of electro-magnets with solid cores is not in consequence of their greater weight, but depends chiefly upon the disposition of their polar surfaces with reference to their armatures.

(*b*). If the polar extremity of a tubular magnet is furnished on the inside with a plug, which may be very thin, the force is very nearly the same as when the core is solid. But no such result is realized if the pole be augmented by a ring of iron outside the tube; which proves that it is not so much the increased amount of polar surface that reacts upon the attractive force as the disposition of this surface.

(*c*). If we consider in the case of the ring, that the latter acts as an armature, and that such action is prejudicial to the attractive force by disseminating it, as in the case of the magnet with several armatures; and that in the case of the plug or stopper there is a concentration of magnetic forces from different sides of the tube we can see why there should be a considerable difference of energy in the two cases; and we can readily estimate the result of the magnetic concentration in the second case if we suppose the plug projected upon the outside of the tube at the moment the latter is magnetized.

(*d*). It results from these effects that, to employ tubular electro-magnets with good effect, we ought to furnish their polar extremities with plugs whose thickness is at least equal to that of the tube. We may obtain an increase of force which in telegraphic magnets will be in the ratio of 25 to 38.

The thickness of the tube should bear proper relation to the strength of the current which excites the magnet. The experiments of Hughes upon telegraphic instruments led him to the conclusion

that this thickness should be one-fourth the diameter. But the author's experiments led him to conclude that for electro-magnets of large size the thickness may be reduced below this proportion, and in a previous memoir the problem was treated at length,* where it was shown that

$$c'=c\sqrt[3]{\frac{x^3}{4(x-1)}},$$

in which c' represents the diameter of the tube; c the diameter of a solid iron core capable of saturation; x the divisor of c' to give the thickness. This value of x may without great inconvenience be made as high as 7.

(*e*). The inconvenience of solid magnets arises from residual magnetism and of magnetic condensation; but we can diminish considerably these effects by insulating magnetically the two branches from the bottom bar by copper discs, or by inserting in the bar itself a thickness of copper.

IX.—Relations which the Grouping of the Battery bear to the Circuit and to a given Electro-Magnet.

As it is a matter of importance that the electro-magnet should be properly adjusted to the current that charges it, it is, of course, equally important that the battery should be arranged with reference to producing the best effect upon a circuit containing a given magnet. Ohm has given a formula for such cases, which is found in all works on physics, but which is of limited application in practice—a fact which led the author in 1860 to seek for another method of analysis, based upon experimental grouping of the cells. The problem is to determine the most advantageous grouping of the elements of a battery to work against a known resistance. Thus, having a battery of 12 cells, it is required to know whether it will be best, in working against the resistance R, to arrange the cells in groups of 3 for quantity and 4 for tension, or in 6 groups of double elements, or 2 groups of sextuple elements, etc., etc.

Now, to determine the general formula, suppose the number of cells to be n. Let a be the number united for *tension* and b the number united for *quantity*. The expression for intensity of current, when r is the resistance in each cell and R the resistance of the circuit and E the electromotive force, is

$$I=\frac{E}{\frac{r}{b}+R}$$

for each group of b cups, and for the a groups united in tension

$$I'=\frac{aE}{\frac{ar}{b}+R}\text{ or }I'=\frac{abE}{ar+bR}.$$

Now, this formula, as well as that of Ohm, is susceptible of a maximum value, for as $ab=n$ and $b=\frac{n}{a}$ the equation becomes

$$I'=\frac{nE}{a+\frac{n}{a}R}=\frac{E}{\frac{ar}{n}+\frac{R}{a}}.$$

By considering a as the variable we find the maximum value corresponds to

$$\frac{r}{n}-\frac{R}{a^2}=0;\text{ whence }a=\sqrt{\frac{nR}{r}},$$

$$\text{and as }a=\frac{n}{b},\ b=\sqrt{\frac{nr}{R}},\text{ or simply }\frac{n}{a}.$$

This conclusion is easily reached without the aid of the differential calculus, as the author has previously shown in *Exposé des applicationes de l'Electricitie*, Vol. I.

Some important results are deduced from this condition of maximum value.

1st. When a battery is arranged for maximum value with reference to a given external resistance R; this resistance is equal to that of the battery, and from the equation

$$a=\sqrt{\frac{nR}{r}}\text{ we get }R=\frac{a}{b}r,$$

and the right hand member of this equation represents the resistance of the battery.

2d. The most advantageous grouping of a battery for a given intensity, I, may be also determined, provided the resistance R is not greater than nr, as under maximum conditions we have

$$\frac{nE}{ar+bR}=\frac{nE}{2ar}=\frac{nE}{2br}$$

whence we get

* Recherches sur les meilleures conditions de construction des Electro-Armants.

$$a=\frac{2IR}{E} \text{ and } b=\frac{2Ir}{E};$$

very convenient and simple formulas for use in applications of electricity.

In some papers presented by the author to the *Académie des Sciences* in June and August, 1860, and in September, 1869, he indicated the limits of external resistance against which it is advantageous to employ certain specified methods of grouping of the battery. These limits for b cells joined for quantity are $\frac{nr}{(b-1)b}$, and $\frac{nr}{(b+1)b}$; that is to say, for cells in pairs the limits of resistance R, will be $\frac{nr}{2}$ and $\frac{nr}{6}$. For triple grouping the limits are $\frac{nr}{6}$ and $\frac{nr}{12}$. For quadruple $\frac{nr}{12}$ and $\frac{nr}{20}$, etc., etc.; and it has also been shown that, as Ohm has proved that the resistances R corresponding to maxium effects for different arrangements of the battery, are represented by $\frac{nr}{b^2}$; that is to say by $\frac{nr}{4}$ for doubled cells, $\frac{nr}{9}$ for tripled, $\frac{nr}{16}$ for quadrupled, etc., etc. Finally it was demonstrated that the limits of resistance of the external circuit for which the grouping by twos, threes or fours compared with the joining for *intensity* and for *quantity*, furnishing the same current, correspond on the one hand to a half, third or fourth of the total resistance of the battery, and on the other to a half, third or fourth of the resistance of a single cell.*

Thus we know by the preceding calculation that for a Daniell battery of 20 cells the resistances of the external circuit, which would require an arrangement of the cells in pairs, lie between 10,000 meters and 3333 meters of telegraph wire. If the resistances are between 3333^m and 1666^m, the cells may be in threes, and if between 1666^m and 1000^m we may adopt the quadruple arrangement.

It is seen from the foregoing that there is no difficulty in determining the best arrangement of any given number of cells to work against any given resistance, since it will suffice to divide the total resistance of the n cells successively by 4, 9, 16, etc., and see which quotient most nearly corresponds to the external resistance. The divisor then represents the square of the number of cells in each group, and the number of groups is obtained by dividing n by the number of cells thus grouped for quantity.

All these calculations are exceedingly simple, and it seems astonishing that so much time and money is expended in useless researches and trials, when the best conditions for useful effect are so easily determined.

Some exacting people assert that these calculations are only rigorously true when we get whole numbers for results, as we cannot divide a battery cell; but in electrical applications we are satisfied with approximate results. It is true we cannot divide the cell of a battery, and so we cannot always realize the maximum conditions; but the calculation affords us a means of determining what numbers to adopt in grouping to obtain the best result. Thus in the example in Chapter V. we found the value of $a = 3.074$; now it is evident that 3 groups in intensity are indicated, and as 8 divided by 3 gives 2.7. there should be 3 cells in each row.

The formulas presented in the preceding articles enable us to compare the forces of different electric generators. There are, however, in these calculations certain considerations which, not having been viewed in the same light by different physicists, have led to disagreements that are to be regretted. Thus, according to Jacobi, we may compare two batteries, both of which are working under maximum conditions; but for this purpose the exterior and interior re-

* These different deductions are obtained successively by equating the values of I obtained by supposing the different groupings of the battery and obtaining from them different values of R. The conditions of maximum value of I may be thus obtained without employing the calculus; for supposing b and b' the cells joined for quantity in two consecutive groupings, we find in equating the two expressions:

$$ar+bR=a'r+b'R$$

or

$$R(b-b')=r\left(\frac{n}{b}-\frac{n}{b'}\right)$$

whence

$$R=\frac{nr}{bb'}.$$

This may be transformed into

$$=R\frac{nr}{(b-1 b} \text{ or } R=\frac{nr}{(b+1)b}$$

according as we compare the groupings. But if we make $b=b'$ then $R=\frac{nr}{b^2}$ and we arrive at the maximum values previously obtained.

sistances for each case must be equal. The intensity of each would then be $\frac{nE}{2ra}$ or $\frac{nE}{2bR}$. Now if for the battery taken as the standard of comparison, we assume $n=1$, which reduces the preceding formula to $\frac{E}{2r}$; we have if we assume the intensities equal:

$$\frac{nE}{2ar}=\frac{E'}{2r'} \text{ and } \frac{nE}{2br}=\frac{E'}{2r'};$$

equations which may be put under the form,

$$\frac{bE}{2r}=\frac{E'}{2r'}, \text{ and } \frac{aE}{2R}=\frac{E'}{2r'},$$

and we can deduce the values of a and b by supposing the resistance R to the resistance r' of the cell of comparison. Then we have

$$a=\frac{E'}{E} \text{ and } b=\frac{E'}{E}\times\frac{r}{r'};$$

and the total number of elements is found by multiplying a by b.

Proceeding on this plan, we find that 5 Bunsen cells represent 9 Daniell cells, each with a surface 11 times as great; which indicates that we should employ 20 Daniel cells, conveniently arranged, as an equivalent for one Bunsen cell. It has been similarly found that 3 Bunsen cells equal in intensity 4 cells of the mercury sulphate battery, each of which had a surface three and a third times that of the Bunsen cell; which indicates that it would require 5 of the mercury sulphate cells to be equal to one Bunsen.

When this method of Jacobi has been duly considered, it is seen that he desired to avoid giving the exterior resistance R a determinate value; but in fact this resistance is represented in the resistance of the battery which is employed as a unit of comparison, and in the preceding example it is equal to 153 meters of telegraph wire. Now, it is easy to see that in proportion as the resistance increases or diminishes, the above figures are singularly modified; for the value of R may be reduced more or less or become more or less preponderant in the value of the denominator of the expression for intensity of current. It is this fact which excites the criticisms of many physicists, We could perhaps take for a starting point a value of R which would be a mean of the resistance of the different cells, but we should not have gained much, and this system of comparison would apply only to short circuits. In such a case we might begin by taking I equal to the intensity of the battery chosen as a standard of comparison, and then obtain the values a and b by the formulas $a=\frac{2IR}{E}$ and $b=\frac{2Ir}{E}$, which in the case of the Bunsen and Daniell batteries will give for a resistance

$$R=153^m,\ I=36.55.$$

$$a=\frac{2\times36.55\times153}{5973}=1.87,$$

and $$b=\frac{2\times36.55\times931}{5973}=11.4.$$

This brings us back to the conclusion previously reached, as R is the resistance of the battery used as a standard of comparison. Suppose R $=$ 1000^m, then I becomes equal to 9.64, a = 3.2, and b = 3, and we see that instead of employing 20 Daniell cells it will be necessary to use only 9 to obtain the equivalent of a Bunsen cell.

X.—Tables for Use in Calculations Relating to Electro-Magnets.

The following tables are believed to be indispensable to a work treating of calculations in electro-magnetism. It should be mentioned that by reason of the omission in Ohm's formula, of a term dependent on polarization effects, the figures representing the resistances of battery cells are too large, and as they increase with the resistances of the external circuit,* there are given in the first table values both for short circuits and for resistent circuits.

The first values were calculated by M. Ed. Becquerel; the second by the author; so that to obtain the best results it will be necessary to use the author's figures when the external resistances are great and Becquerel's when they are small.

* In taking into account the effect of polarization, the formulas for E and r according to Ohm's method are:

$$E=\frac{II'(R'-R)+(Ie'-I'e)}{I-I'} \text{ and } r=\frac{(I'R'-IR)-(e-e')}{I-I'}$$

which differ from the applied formulas only in the second terms of the numerators, which do not appear in the latter. This omission causes the calculated values to be greater than the quantities R and R'.

TABLE No I.—VOLTAIC CONSTANTS.

Batteries in common use.	Electromotive force E.	Resistance r in meters: Du Moncel.	Resistance r in meters: Becquerel.	Ratio of electromotive force.
Daniell battery (French telegraph pattern)....	5978	931	180	1.00
Bunsen's cell (amalgamated zinc)..........	11123	153	57	1.86
Delaurier cell (chromic acid and salt solution)	12418	366	"	2.08
Chutaux cell (potassic bichromate)...	11400	600	"	1.91
Duchemin cell (ferric chloride and salt solution)	9640	942	"	1.61
Marie Davy cell (mercuric sulphate and water)...............	8192	550	"	1.37
Leclanché cell (large size)................	7529	400	"	1.26
De la Rues cell (fused silver chloride and zinc chloride; size, minute).....	5596	748	"	0.94
Prudhomme's cell (lead sulphate and water...	3301	880	225	0.55

The figures in Table No. 2, giving the values of the diameters of wire, with and without the silk covering, are not exactly the results obtained by direct measurement. These results are deduced from measurements with the rheostat upon manufactured electro-magnets, so that open spaces and other irregularities of winding are taken into account. This method is not very exact in one sense, but it is clear that it gives values much more serviceable for calculation, for it takes into account quantities which would otherwise be neglected.

These values are easily calculated from formula (5). When $a = 0.012$, $c = 0.012$ and $b = 0.12$

$$g^2 = f\sqrt{\frac{0.000000000289529}{H}}$$

and as $\frac{g}{f} = s$, s representing the diameter of the copper wire, measured directly without the silk,

$$f = \sqrt{\frac{\frac{0.0000000028952}{H}}{s^2}}$$

TABLE II.—DIMENSIONS OF WIRE USED FOR ELECTRO-MAGNETS.

	Gauge numbers.	Diam. naked wire.	Diam. covered wire.	Values of f.	f^2.	$\frac{s^2}{s'^2}$	g^2.
		m.	m.				
Common gauge, silk covered.	No. 32	0.00014	0.00023	1.6428	2.6988	816.3	0.0000000529
	" 28	0.00022	0.00033	1.5000	2.2500	330.6	0.0000001089
	" 24	0 00027	0.00040	1.4814	2.1984	219.5	0.0000001600
	" 20	0.00035	0.00048	1.3714	1.8796	130.6	0.0000002304
	" 16	0.00040	0.00055	1.3750	1.8906	100.0	0.0000003025
	" 12	0.00049	0.00065	1.3265	1.7582	66 6	0.0000004225
	" P	0.00058	0.00077	1.3275	1.7609	47.5	0.0000005929
Decimal gauge, cotton covered.	No. 1	0.00060	0.00122	2.0833	4.1331	44.4	0.000001488
	" 2	0.00070	0.00134	1.9143	3.6634	32.6	0.000001795
	" 3	0.00080	0.00146	1.8250	3.3306	25.0	0.000002132
	" 4	0.00090	0.00158	1.7555	3.0800	19.8	0.000002396
	" 5	0.00100	0.00172	1.7200	2.9584	16.0	0.000002958
	" 6	0.00110	0.00184	1.6727	2.7989	13.2	0.000003385
	" 7	0.00120	0.00196	1.6333	2.6687	11.1	0.000003842
	" 8	0.00130	0.00208	1.6000	2.5600	9.4	0.000004326
	" 9	0.00140	0.00220	1.5714	2.4670	8.2	0.000004840
	" 10	0.00150	0.00232	1.5466	2.3901	7.1	0.000005382
	" 11	0.00170	0.00254	1.4941	2.2320	5.5	0.000006451
	" 12	0.00180	0.00266	1.4777	2.1844	4.9	0.000007075
	" 13	0.00200	0.00288	1.4444	2.0851	4.0	0.000008294
	" 14	0.00210	0.00300	1.4285	2.0392	3.6	0 000009000
	" 15	0.00235	0.00327	1.3915	1.9849	2.9	0.000010690
	" 16	0.00260	0.00354	1.3615	1.8523	2.4	0.000012532

Table No. III.—Dimensions of Wire in Common Use.

Old Gauge	Diameter uncovered.	Meters per kilogram.	Diameter covered wire.	Meters per kilogram.	Resistance per kilogram.
No.	m	m	m		
12	0.00056	514	0.00054	515	—
14	0.00050	680	0.00048	678	—
16	0.00044	796	0.00044	780	22.30
18	0.00040	867	0.00040	929	29.60
20	0.00036	1238	0.00034	1390	31.47
22	0.00032	1439	0.00032	1420	—
24	0.00028	1925	0.00028	1872	43.75
26	0.00026	2389	0.00026	2257	133.23
28	0.00024	2854	0.00022	3230	174 94
29	0.00023	3054	0. —	—	185.77
30	0.00021	3822	0.00020	3920	228.46
31	0.00020	4042	0.00019	4390	471.51
32	0.000185	5875	0.00017	4810	528.57
33	0.00017	6130	0 00015	9400	967.56
34	0.00016	7750	0.00014	9580	—
38	0 000125	10550	0.000125	9650	1680
39	0 000115	11950	0.000115	10600	—
40	0.00010	13400	0.00009	16290	—
42	0.00008	24800	—	—	4228
43	0.000075	25600	0.00007	26310	—
44	0.00006	36500	0.00006	35400	17500

Table No. IV.—Dimensions of Wire in Common Use.

Decimal Gauge.	Meters per kilogram.	Diameters.	Decimal Gauge.	Meters per kilogram.	Diameters.
No.			No.		
P	670	0.0005	10	70	0.0015
1	418	0.0006	11	66	0.0016
2	330	0.0007	12	54	0.0018
3	208	0.0008	13	40	0.0020
4	180	0.0009	14	34	0.0022
5	160	0.0010	15	30	0.0024
6	137	0.0011	16	26	0.0027
7	100	0.0012	17	19	0.0030
8	85	0.0013	18	16	0.0034
9	75	0.0014	20	10	0.0044

Tables 3 and 4 have been prepared with much care by M. Bonis, a manufacturer of wire in Paris, who uses copper whose conductivity is 94 to 96.

The covered wire is smaller than the wire before covering, because of the tension to which they are subjected in the covering process, which causes a reduction in diameter averaging three hundredths of a millimeter. So that there is an increased length per kilogram for some of the sizes.

In the preceding tables the resistances are given in meters of telegraph wire. These are reduced to *ohms* by dividing by 100.

The values of electromotive force in Table 1 may be reduced to *volts* by multiplying by 1.079.

Table No. V.—Resistances.

Diameter millimeters.	Gauge.	Meters per kilogram.	Resistances Ohms. per kilogram.	Resistances. Ohms. per kilometer.
	Old gauge.			
0.02	—	355584	1803084	52317
0.10	40	14369	30377	2118
0.20	31	3614	1922	581
0.30	23	1607	378	235
0.40	18	902	119	133
	Decimal gauge.			
0.50	14	576	49	85
0.60	1	401	24	59
0.70	2	294	13	43
0.80	3	226	7.5	33
0.90	4	178	4.6	26
1.00	5	144	3.0	21
1.50	10	64	0.59	9.2
2.00	13	36	0 19	5.3
2.50	15	23	0.078	3.39
3.00	17	16	0.037	2.36
3.50	18	12	0 020	1.72
4.00	—	9	0.011	1.32
4.50	20	7	0.0074	1.04
5.00	—	5.76	0.0049	0.84
5.50	—	4.71	0.0033	0.70

At a recent meeting of the Physical Society Dr. Jas. Moser read a paper on "A General Method of Strengthening Telephonic Currents." This consists in forming a primary circuit of the telephone transmitter or derived circuit, a set of induction bobbins in derived circuit, and a charged secondary battery, the whole circuit having a very low resistance. Each primary bobbin has a secondary wound over it, and these secondaries are connected in quantity to the telephone line, which has at its remote end a set of telephones in derived circuit to the earth or return wire. In this way one line wire serves to supply a large number of separate telephones, a hundred being employed by Dr. Moser to transmit music from the Hippodrome in Paris to the Place Vendôme. The system is applicable to long lines; and the induction noises are reduced by subdivision among the separate telephones.

THE SUEZ CANAL AND THE EUPHRATES VALLEY RAILWAY.

"The Nautical Magazine."

Removing a neighbor's landmark is numbered among the nine special sins denounced in our very charitable Commination Service, and no few honest thinkers, puzzled to explain the existence of somewhat similar sentiments in the Bible itself, have gone so far as to assert that allusion is made to achievements like the Suez and Panama Canals, which certainly do alter the natural face of the land, and completely obliterate our neighbor's landmarks. There assuredly is something approaching the impious in the French suggestion to turn the lower levels of the desert into a vast inland sea, while the notion of converting South America into an island proves equally unseemly in certain quarters. But pessimists of this school evidently belong to a class akin to those "stay-where-you-are" politicians of a former day, who for similar reasons first denounced roads into an enemy's country, and subsequently raised a hue and cry against railways. Happily most likely for the world, science takes no notice of these perennial croakings, nor of the constant attempts made by bigotry to enslave her. Having grown accustomed to them from long acquaintance she heeds them not in the slightest, pursuing unabated the even tenor of her course towards perfection. Those who love progress and those who do not must accept matters as they are, and expect in the future still greater effacements of existing geographical distinctions. Nevertheless it must be conceded in fairness that each mundane alteration made in Dame Nature's map is the occasion for strife among the nations, and it is too much to hope that the Suez Canal will escape the hateful fate which for some occult reason has dogged the steps of science throughout all ages. M. de Lesseps' handiwork has become a new bone of contention which it seems likely may be fought for by the European nations with even more bitterness than Gibraltar ever was in the past. Already it has led to the shedding of blood, although by a happy diplomatic fiction we are not supposed to be at war with any one, and before the century closes it may, probably will, lead to fresh and important changes in the geograpy of Europe. Moreover, and what is equally consequential from our point of view, it may bring around considerable alterations in the respective commercial positions of the leading European Powers. It is this problem we desire first to consider.

Those States who have an outlet on the Mediterranean Sea are now several days nearer the East than ourselves, and by right of that newly-gained advantage possess a *natural* trade interest in India and China at least equal, if not superior, to our own. Since Count Ferdinand de Lesseps' waterway was opened, France, Italy, Austria, and Russia, have alike paid growing attention to Eastern affairs. One and all have started direct steamship communication with various ports in the Orient; each, it appears, is daily adding to its mercantile fleet; nor does it seem unreasonable to anticipate that as time grows older these great nations, and possibly Spain and Greece also, will increasingly supply their own shipping wants in the East, instead of largely employing British bottoms as now. In each country a revolution has commenced in favor of home service, and if the Suez Canal remains permanently open, we shall doubtless see constant large additions made to their Mercantile Marines with comparative stagnation at home. Not that the British shipping engaged in the Occidental and Oriental trades is likely to show any signs of diminution—far otherwise. Commerce is rapidly growing between England and India, and between England and Australia; nor is there any pressing danger of a decline in our trade with China. Our commerce with the Eastern world is bound to grow apace, so long as we maintain the integrity of our world-wide Empire; but whereas the annual augmentations to our mercantile fleet will be gradually progressive, the

additions made to those of foreign nations will, for some time to come at least, be by leaps and bounds. To otherwise put it—our excessive preponderance will steadily pass away. Any one who has closely followed the fortunes of the Messageries Maritimes, Rubattino and Austrian Lloyds lines during the past few years will know that these pessimist forebodings are not absolutely uncalled for. Even Russia, with a capital overland route to China, has found it worth her while to start a direct line of steamers between Odessa and Hankow, threatening our tea ships with the loss of a most lucrative branch of transport. Let optimists say what they like, there is, unhappily, only too much reason for our misgivings, and this every one may observe for himself by glancing over the files of that well-compiled Parisian shipping journal, *L'Indicateur Maritime Universel*, which gives particulars of the chief mercantile fleets of every nation engaged in the foreign trade. In its columns we can trace the progressive history of each company. Those who have not noted the doings of the Messageries Maritimes and Rubattino Companies will find therein ample matter for quiet reflection, possibly of a not over pleasant character. Until the Suez Canal was dug neither of these Companies had a vessel employed in the Eastern trade; to-day both possess fleets of considerable magnitude, to which additions are being made, so to say, daily. Can this revolution be deemed advantageous to England? We fear not, despite the glee evidently felt by certain daily journals at this country owning four-fifths of the shipping passing Suez. So far as we can see there is little significance in the fact, and that little, if either way, counts against us. It certainly affords no occasion for rejoicing, or for the singing of triumphal pæans, as until the opening of the Canal, and while commerce had to round the Cape, English steamers were alone seen in Eastern waters. Twelve years ago neither France, Italy, Austria, nor Russia ran a single steamer between the Mediterranean and the Indian Ocean; whereas now the vessels of their steam fleets are both numerous and powerful. Why this should be regarded as a triumph of British enterprise we confess our inability to see, for, had not the Suez route been opened, it is very doubtful whether a single foreign steamer would yet have found its way to Bombay, Point de Galle, Singapore, or Hong Kong.

Unquestionably the chief feature in the near future will consist in the revivification of French commerce with the East. Possessing a magnificent seaboard, with several good harbors along the Mediterranean, and an almost complete railway system, our enterprising neighbors are sure to avail themselves to the very utmost of their splendid good fortune. Being many days' passage nearer Port Said than the nearest English ports, namely, Plymouth or Southampton, and having as well at command unusual facilities for inland communication, they would be the next thing to insane to continue sending goods to England for transhipment, or to further employ British shipping in the traffic between Bombay and Port Said. Trade between France and the East is much larger than is generally supposed, incomparably larger than is revealed by any *prima facie* examination of trade statistics. From China alone the French import annually several million pounds worth of raw silk, but a portion only of this commerce is entered in their Board of Trade returns. Considerable quantities are still shipped in English bottoms, despite the fact that nine-tenths of the silk factories are located in the south, mostly around Lyons. Northern France still obtains via England a material proportion of such Oriental wares as she consumes—a negation of common sense and all economic laws of distribution that must find a speedy ending, now that French shipowners are learning to appreciate the general advantages accruing to Marseilles through its central position. Is it reasonable to suppose that Parisians will wait a week or more for goods to travel to England first, when a few hours' railway ride will carry them from Marseilles to Paris? The world is not quite so mad as all this, and Frenchmen above all people are qualified to take care of their own interests, whether commercial or political. That our trade has not been cut to pieces already is solely due to the intense selfishness; to the unutterable short sightedness and folly of the French railway companies, who have never afforded merchants the least encourage-

ment. To their prohibitory charges, more than to anything else, we owe the continuance of our maritime supremacy; but signs are not wanting to tell us that, although somewhat late in the day, these unpatriotic bodies are at length beginning to understand in what direction lies their best hope of future prosperity. By lowering freights they are now seeking to divert French commerce with the East to the Marseilles route, and according to a recent statement, their efforts are meeting with a fair measure of success.

Probably enough this change of policy on the part of the Great Southern of France, the Paris and Lyons, and other railways is due to no more exalted motives than self-protection. By the opening of the St. Gothard tunnel a new route has been opened for goods to reach western France, and it may be that this threatened competition has led the French companies to reconsider their position. The same cause is unquestionably diverting traffic to the Antwerp route, and is also likely enough to affect French railways in the direction suggested. Moreover, the Arlberg tunnel is rapidly drawing to completion. When it is opened (which will be at no very distant date) both the west and north of France are bound to be extensively supplied through Lombardy and Switzerland, and to counteract this new competition we may rely upon it that freights for merchandise and passengers will be again reduced on all the lines connecting Marseilles with Lyons, Dijon, and Paris.

Each reduction in charges made by these railway companies will lead to an increase in the Mercantile Marines of France, Austria, and Italy, and add to the importance of Port Said and Suez in the eyes of all European nations. Already, commerce between the East and West has been largely diverted from its ancient route round the Cape of Good Hope, and, through the rapid supercession of sailing vessels by the colossal high-speeded steamers of this era, seems destined to desert it almost entirely; especially, if it looks probable, railway freights on the Continent are generally lowered. Time is a factor that has to be allowed for when solving any trade problem, and granting that the companies reduce their tariffs so as to compete with English shipowners, and work in concert with the steamship lines managed by their countrymen, the world will witness another great trade revolution from which English shipping interests will seriously suffer.

In the event of M. de Lesseps' Canal being permanently kept open and maintaining its present position as England's road to India, there can be no doubt but that a large and increasing share of prosperity will fall to the lot of Marseilles, Genoa, Venice, Trieste, Brindisi, and other Mediterranean ports; indeed, one or more of them in time is likely to rival Liverpool or Glasgow in commercial grandeur; possibly may far surpass either of those great shipping centers. Having Western Europe, with its enormous population, as a market for the products of the Orient, and *vice versa* having the entire East to supply with the manufactures of Europe, there seems no positive counter reason why the more fortunate and flourishing amongst the southern outlets should not ultimately grow as rich, as well populated, and as important as Berlin, Brussels, or Paris; or, for the matter of that, as vast as London itself. At present, of course, it would be mere waste of energy to attempt to prophecy which towns will fall upon evil days and languish away into insignificance, or which will leave their indelible marks upon the pages of history, civilization and commerce. Such rises and such decadences are almost entirely dependent upon national enterprise and the idiosyncracies of trade; nevertheless, it requires no Merlin-like gifts to see that Venice and Genoa will again emulate their glorious past, or that Marseilles, Trieste, Brindisi, and Salonica, will be numbered among the great cities of the future.

One point, however, must not be overlooked in any inquiry into the commeror strategic influence of the Suez Canal. It is ever liable to be seized by some one of the Great Powers and closed to commerce for a time—perhaps for months or years should England and France again come to blows. Both have important interests in the East to defend, equally to be attacked; and as of old the battle for supremacy would be partly fought out in the Indian and Chinese Seas. In that

eventuality the nation obtaining the Suez Canal would reap an immeasurable advantage. Indeed, its possession would half win the battle. Neutralization is proposed under the joint guardianship of that sublimely impotent mental creation, the European Concert. But all efforts in this direction must prove futile, mere vanities of vanities, as in times of war treaties and conventions are very apt to go the way of all flesh. France and Italy, if not several other of the Powers, favor the neutrality proposition, probably foreseeing that sooner or later it may operate to embarrass England's military action, while strange to say not a few shipowners in this country are numbered among its adherents. However, it is very doubtful whether the latter have given any deep consideration to the problem, for if they had they would probably have found out by this time that the suggestion is chimerical to a degree. As the *Temps* very properly points out, there is something childish about the scheme from beginning to end. Seeing that in one day England seized the Canal along its whole length, while unprepared Europe looked on in amazement, it is mere waste of breath to talk about securing by a European Convention its free passage for ships of commerce. Moreover, it is impossible that English diplomacy can be beguiled by so flimsy a pretense. Every statesman in this country understands that the foreign cry for neutrality is only a fresh attempt on the part of Europe to stay the hand of England and cripple her future power for good or evil in Asia. Besides, if any such Convention was signed at all, reservations about *force majeure* would necessarily have to be inserted, and this plea can always be put forward to countenance military operations undertaken contrary to the spirit, if not to the letter, of international compacts. No belligerent can conscientiously carry out its engagements with other nations. Circumstances are always arising for which no previous provision was or could have been made, and, besides, when the war path is being trod military necessities are paramount to all other considerations. Let England be at war with France, Italy, Russia, or any Power, and no Convention would prevent them trying to aim a vital blow at her in the plains of the Egyptian Delta. The instrument would then become a mere piece of waste paper. So long as the waterway between Port Said and Suez continues as now to be England's highway to the East, so long will Egypt be her battle-field, and if in time of war she does not herself seize the Canal other nations will.

In the next place the Egyptian race is liable to "kick against the pricks;" in other words, to rebel against the European control, and at no time more likely than when we are involved in an imbroglio with one of the Mediterranean Powers. At any moment a popular movement might end in dethroning the reigning Khedive and in the seizure of the Canal. Treachery is always to be feared when dealing with Asiatic races, and unfortunately for us unfriendly counsels are often tendered to them by our disguised foes. If left undefended as now, the Canal could be seized and closed against us long before we received notice of any such intention, while supposing that at the same time our relations were greatly strained with France or Italy, a flying expedition emerging from Toulon or any other great naval dockyard on the Mediterranean, might cross unperceived to Port Said and succesfully bar our road. The only ways to guard against this prospective danger are to double our Mediterranean fleet, to convert Cyprus into a place of arms, and to employ a large number of cruisers in these waters; or to construct an alternative route, and render ourselves somewhat independent of the modern Gibraltar.

Neutralizing the Canal will not safeguard our interests in the least; it will only bring us into graver dangers and hasten the day when its possession will be fought for among the European lions. Nor do the patrons of the scheme seem themselves to have any distinct notion as to what the word means or how their plan is to be put into operation. In what possible way can the Canal be neutralized? It certainly is not proposed to prohibit the passage of ships of war during times of peace. If so, Europe might as well surrender Asia to the Asiatics. In these days of superlative weapons every European in Asia might be massacred long before aid could arrive round the Cape. No nation, least of all

England, would consent to forego the advantage of rapid communication between the different sections of its Empire, while in times of war it is quite certain the provisions of any agreement would be disregarded by the belligerents. If France was at war with England her military policy would be inevitably directed to blocking the Canal against us, while her emissaries stirred up insurrection in India. The only possible result of a European convention with regard to the passage of the Canal would be to lull us into a sleep of false security, out of which we might be rudely awakened when too late.

Sir William Andrew and many other Anglo-Indian authorities throw forward as a remedy the construction of a railway through the Euphrates Valley, from Alexandretta in the Bay of Iskenderoon, to Grain at the head of the Persian Gulf; and I take it as assured that among the more important outcomes of passing events in Egypt will be the construction of that railway under State guarantee, or something very much akin to a guarantee. The distance between the two places above mentioned is estimated at about 920 miles, and as the line would traverse a flat country devoid of any serious engineering difficulties the capital required would be comparatively small. Knowing at what price railways have been laid in America and India the estimate of £7,500 a mile does not seemingly err on the side of minimisation. At this rate a capital of about eight millions would suffice to construct the line, including stations and plant, and upon this sum dividend earning should not be impossible. In the worst case a guarantee of 4 per cent. interest would only cost Government the inconsiderable sum of £320,000 per annum, compared with which the political advantages to be obtained are immeasurably more consequential; indeed, cannot be weighed in the same balance. Besides which, the saving of seven days in the passage to India, would enable Government to effect several economies in administration, and in all probability to more than save the actual outlay. About the strategic advantage of a quick alternative route which would make us to some extent independent of the Canal there can be no two questions. It would enable us to govern India twice as efficiently and ten times more safely than at present, while it would do more than anything else to secure the peace of Europe. Egypt and the Suez Canal would then lose much of their political significance, and it might be possible for Continental nations—then no longer jealous of England—to come to look upon the Canal in the light of a commercial waterway only. Of course, we should take upon ourselves the material as well as the moral protection of Turkey in Asia, and this would mean fighting the Sultan's battles for him. This, however, might be to our advantage. It would secure the independence of the Turkish Empire and seriously check, if not put an end to Russian encroachments in that quarter.

The moment peace is assured in the Delta, and friendly relations are again set up with the wily Sultan, negotiations it is to be hoped will be opened up with a view to obtaining the indispensable concession from the sovereign ruler of Syria and Mesopotamia. If already in train they will have been commenced not one moment too soon. France has ever studied to exercise an influence in Syria, and if French capital or French enterprise is directed to this undertaking, we shall at some future day experience just the same trouble that we are now undergoing at Suez. Syria must not be allowed to pass under the protection of our ever sore and jealous neighbor, nor to become to all intents and purposes one of her outlying provinces. Lying almost in the direct road between Great Britain and India, our interests therein are too important to be neglected, even in times when weightier matters are pressing.

While on this point it may be pardonable to express the hope that negotiations will also be opened up with the Shah of Persia for the right to construct a line connecting Kurachee with Bassorah on the river Euphrates. English statesmanship should not overlook the fact that Russian money is rapidly pushing a system of railways through Persia to the head of the Persian Gulf. When these lines are completed, the dream of a British railway between the Mediterranean Sea and India will be plainly impossible.

This, however, is not the proper time or place to discuss these problems, nor is

it the place to try and unravel the equally interesting and still moot problem as to where the railway should start from. Alexandretta, Seleucia, Latakia, and a dozen other points on the Syrian coast have been named. Each possesses its separate commercial and political advantages, and each is subject to its particular disadvantages. Alexandretta has generally met with the preference; first, because of the natural shelter afforded to vessels through the magnificent Bay of Iskenderoon; and, next, because it lies in a direct line with the Bosphorus and Constantinople, to which city the framers of the scheme hope ultimately to continue the line. What we wish to consider mostly is the probable effect that its construction will exercise upon British shipping and how far it will affect the Suez Canal.

From a strategic aspect Suez would lose half or more of its present significance, and this could only result in a gain to commerce. With the railway in existence the Canal would no longer constitute our chief military road to India in time of trouble, troops being so much more readily and more expeditiously forwarded by rail; it would consequently cease in part to be a menace to the peace of Europe. The enmity of Europe would then center in Mesopotamia instead of the Delta as now; while the Canal to some extent at least would be surrendered to the cause of humanity, progress, and commerce. To neutralize it then would be no such impossible task, for the Euphrates railway would serve England's military needs in times both of war and peace, and those of other nations in times of peace alone. Having comparatively small interests in the East to serve, other nations might then consent to shut against their war ships the short cut to the Red Sea. That the railway would abstract any large share of the commerce now passing through the Canal is an argument finding no echo in the writer's mind. Some proportion of through traffic, especially light goods and those liable to spoil by long keeping would doubtless pass over the line; but it happens that economy is the chief *desideratum* which merchants have to consider, and the cost of double transhipment, together with the high freight charges entailed by a 900 miles railway journey, would more than eat up, I fear, any saving that might be effected by taking the shorter sea voyage up the Persian Gulf. Lesseps' Strait would convey just as much shipping between the Mediterranean and Red Seas, though the Company might lose some share of its prosperity in being compelled to lower its excessive tolls. This, however, would prove a general gain to the commercial world, not a loss, though the De Lesseps family might think differently.

Among other probable consequences there can be little doubt that Kurrachee would become the military port of India and in the course of years rival Bombay in magnificence; while regular steam communication would be established between those towns and Grain. Cyprus, of necessity, would be converted into our chief *place d'armes* in the Mediterranean, and upon it we should have to maintain a considerable army, supported by a fleet of powerful ironclads; while Alexandretta, or Latakia, or whatever other town the line started from, would leave its ineffaceable mark upon the history of commerce and might be expected in time to revive the glories of the Syrian cities of old.

Finally, there seems no cause to imagine that the construction of a line through the Euphrates Valley will exercise a detrimental effect upon our shipowning interests. It is more likely to impart a new stimulus to them. Fresh lines of steamers will have to be run between Grain and Kurrachee or Bombay; while a new and important port will be opened on the Mediterranean. Only good apparently can result to British commerce from the threatened competition with the Suez Canal Company; and, if that ill-disposed and selfish body experience some amount of harm, no one but its constituent members will have any occasion for moaning.

WATER MAINS IN LONDON.—There are now 828¾ miles of water mains for the supply of London which are constantly charged. Of these the New River Company has 214 miles; Lambeth, 136¾; Southwark and Vauxhall, 117; West Middlesex, 86½; Kent, 85; East London, 85; Chelsea, 67; and Grand Junction, 37½ miles.

THE TRAINING FOR STUDENTS IN CIVIL ENGINEERING.

By GEORGE L. VOSE.

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The remark has recently been made that, while civil engineering is a *business* of great antiquity, it has only recently become a *science*. Without stopping just now to ask whether engineering has in reality become a science, we may very properly inquire whether anything that can correctly be called engineering science has made the business any better, or has enabled us to produce any better practical results; and, if so, in what that science consists. I ask these questions, as they seem to me to lie at the foundation of a correct understanding of what we should teach, and how we should teach it in the engineering school. To know how to teach, we should know first of all what we are required to teach, and what results we are expected to produce. These we learn best by a careful examination of the methods followed and the works constructed by our leading engineers.

We find at the outset that civil engineering in the United States is somewhat different from that in Europe, and in many respects very properly so. To have attempted in this country to carry out the methods followed in Great Britain and on the continent, would have soon put a stop to all internal improvements in America. The rapid and enormous development of the United States is due not simply to the railroad system, but to the *American* railroad system. Our early self-taught engineers at once saw that with our very limited means, and our vast extent of territory, a very much cheaper kind of engineering would have to be adopted for this country than was followed in the densely populated parts of Europe, where money was plenty and the territory was comparatively small. If we look at the works of Smeaton and Brindley, of Rennie and Telford, of Brunel and the Stephensons, we find a kind of engineering eminently English, eminently substantial, eminently expensive. The docks of Liverpool and London, the splendid roads of Wales, the stone bridges and viaducts through the kingdom, almost without number, the harbors and breakwaters, the works for drainage and reclamation of land from the sea, the older canals, and more recently the enormous network of railways, with their elaborate structures of iron, brick and stone, all bear witness to the energy and the skill of English engineers, but all at the same time illustrate a very costly kind of engineering. If we look at the engineers of the earlier works in Great Britain, we shall find a very large proportion of them to have been self-taught men, men who arose from the ranks—masons, blacksmiths, bricklayers, carpenters, men with but little of what we call education, and little or perhaps nothing of what we call science, but men of indomitable, courage, infinite patience, well-trained judgment and unbounded common sense, who by long years of persistent toil put themselves at the head of the world's engineers.

If we look now at the early engineering of the United States, we find that the requirement here was quite different from that in Europe. We were very fortunate in this country in possessing good natural seaports, and were thus saved the expense of the artificial harbors which have made so noted a feature in foreign engineering. There was found in North America a land traversed by magnificent natural water-courses, furnishing ready-made means of communication, which for a long time were quite sufficient for the purposes of our interior commerce. Civil engineering in the United States may be said to have commenced with the building of the Erie Canal, the construction of which first called out our native talent, and produced our pioneers in the profession, Benjamin Wright, James Geddes and Canvass White. The rapid spread of the canal system into Pennsylvania and Ohio produced many other admirable engineers; but the advent of a new mode of transport soon put a stop to canal work, and gave us the fathers of railroad engineering in the United States, Gridley

Bryant, Jonathan Knight; Benjamin H. Latrobe, Charles Ellet, John B. Jervis, John Childe, William Gibbs McNeil and George Whistler. Many of the most noted among these early engineers were, like their English brothers, entirely self-taught men, who had to feel their way along cautiously through the untried field of engineering work; and all of them owed their success mainly to their own indefatigable exertions, aided by strong native talent, and not to anything that could properly be termed engineering science or engineering education. There has been no more admirable feature in American engineering than the manner in which these early engineers adapted themselves to the requirements peculiar to our country, and molded their practice to the conditions imposed upon them. The long range of the Appalachian Mountains, lying as a barrier between the great central basin and the Atlantic coast, made an enormous draft upon the energy, skill and patience of the fathers of the American railroad system, and well was that draft honored by the engineers of the Baltimore and Ohio, the Pennsylvania and the Erie railroads. Not only have the methods employed by the engineers of those early roads given to this country the backbones of our system of interior communication, but they have served for examples the world over, as models for railroad location.

We may stop here a moment to note the fact that all through the early engineering of both England and the United States there was no such thing as any school for training the civil engineer. There was no such thing as a science of civil engineering. Engineering was in some sort a craft, but not a recognized profession. The works of internal improvement were comparatively limited in extent, and the demand for engineers was not larger than could be supplied by men of strong natural powers, with enthusiastic love for their work; men who had the genius to originate, and the ability and the perseverence to find out, what they did not know.

To come down a little later, we find the construction of public works rapidly increasing, and a corresponding increase in the demand for engineers. Many young men, drawn to an occupation which is always attractive to persons of a practical turn of mind, found employment as assistants to surveyors and engineers, and gradually the system of apprenticeship arose, under which a young man, wishing to become an engineer, entered an office for a fixed time, and paid a premium for learning what he could. Of course, as a general thing no attempt was made to teach him, but he was expected, or at any rate allowed, to keep his eyes open, and to see what was going on. About this time, too, a few books upon engineering matters began to show themselves, and the shelves of the offices furnished such food as Pambour on the Locomotive, Wood on Railroads, Vicat on Cements, Parnell on Common Roads, and the like. These works made very little attempt to be scientific, but were largely descriptive of actual works, and were thus valuable.

Foremost among institutions designed especially for the training of engineers, was the Polytechnic School at Paris. In this school it was recognized that civil engineering was largely a mathematical business, and it seemed to be assumed at the start that if a little mathematics was good, more mathematics was better, and the most mathematics was the best; and many leading minds in that eminently mathematical nation set to work to reduce engineering to a mathematical science, and volume after volume, upon the location of roads, the stability of retaining walls, the transportation of earth, the application of descriptive geometry to the construction of masonry, and other like matters appeared; in which all the resources of the higher mathematics were exhausted, and which showed the authors to possess every accomplishment except, perhaps, a little common sense. In establishing the earlier schools in this country, it was quite natural to look to the pioneer school in Paris, and in many places a system was imported not at all adapted to any practical engineering in this country, if indeed it was to that in any country. The fact that under such a system good engineers have been produced means nothing, as the same may be said of no system at all. We are not to judge a system of instruction by a few brilliant exceptions, but by the general average. While we may justly admire the great works of foreign engineers, and

while we have much to learn from their practice, at the foundation our system is better for us than any other; and we shall do better to let the plant which is native to our soil grow naturally, than to graft foreign branches on to it. The attempt to transplant the French system of engineering instruction into the United States has not been a success. American engineering is the best engineering for this country. Our students are to be American engineers, and we must fit them accordingly.

But it may be asked, is not this rather a narrow view to take? Is not geometry the same the world over? Is not the strength of a piece of iron the same in the United States as elsewhere? Are not the fundamental principles of engineering the same in all countries? Most undoubtedly they are. But, we may reply, the varying conditions under which we are to apply the fundamental principles of engineering may make it advisable to do very differently in one place from what we would in another. I stood a good many years ago with an English engineer upon the site of the present Niagara suspension railroad bridge, and I told him that it was proposed to carry railway trains across that river. He replied that it could not be done, because a tubular girder of 800 feet span, if strong enough, would fall by its own weight, and that a suspension bridge could not be made stiff enough to carry a railway train. The geometry and the iron of the English engineer were the same as those of Mr. Roebling, but Mr. Roebling had the genius for meeting a new problem, which the latter had not. The whole progress of engineering in this country has been a perpetual illustration of the successful solution of new problems. The very facts which to one man are an impassable barrier, become in the hands of another the very means of success. The grades and curves which by foreign engineers were pronounced impractible, if not impossible, in the hands of Mr. Latrobe were made to perform one of the greatest feats of modern engineering.

In deciding what course we shall follow the best to train the young engineer, we see at once that we have to do two things—to make him an engineer and to make him a man. The more symmetrically we develop our man the better the result will be, even in a special direction. Far too little attention has been given in our technical schools to anything like general education. Just as a variety of food is best for physical health, so is a variety of study best for mental health; and just as we need a foundation of the best general physical health in order to train a man for physical exertion in any particular direction, so do we need a basis of good general mental health in order to train a man for mental exertion in any special line. "A thorough technical education," says a recent writer, "embracing all that science and art can bestow, is not enough to produce the best industrial results. There is need of the additional discipline which comes only from the study of letters. The man must be *formed* as well as *informed* before he is fully educated even for practical purposes." Many years ago the Bavarian Council for Roads and Bridges decided on admitting into the body of government engineers none but those who before entering the Polytechnic School had followed a complete classical course. The administration of mines had also constantly required the same qualification. A better illustration of the thoroughly and symmetrically trained engineer can nowhere be found than the late Baron Weber, perhaps the best railroad expert the world has ever seen. He commenced with a thorough classical training; next he passed through the polytechnic school at Dresden; he then received a practical training, first as a pupil and afterward as constructor in the locomotive works of Borsig at Berlin, at the same time attending university lectures in political economy and the natural sciences. Finally he commenced his practical railroad course, beginning as locomotive engineer and working up through all the technical and administrative positions to the office of the General Manager; and later, having traveled through all civilized countries on a tour of inspection of public works, became Councillor to the German Empire.

We see at once that in laying out any course of study we are to keep in view a double object. We are to drill the student, and to give him useful information. Some studies furnish excellent discipline, but are of little or no practical use;

others are useful, but do not afford much discipline; while others again at the same time accomplish both of the above results. "Every branch of study," said the late Dr. Wayland, "should be so taught as to not only increase our knowledge, but also confer valuable discipline; and it should not only confer valuable discipline, but also increase our knowledge. If it does not accomplish both these results, there is either some defect in our mode of teaching, or the study is imperfectly adapted to the purposes of education." Studies more especially for discipline should, as far as possible, come early in the course, while studies more for use should come later, as having already the discipline, the student makes better progress. As one of our prominent engineers has put it, when a student has learned how to learn, he can quickly learn anything.

The study of mathematics has always taken a prominent place in engineering courses, and very properly so; but it is after all the more simple and elementary part of mathematics that is of the most use to the engineer. Recent discussions upon the subject of the training for civil engineers have called out some very decided opinions from authorities who are certainly competent to express themselves in this matter. "Much time," says Mr. Thomas C. Clark, "is wasted in our colleges and technical schools over the higher mathematics. Every engineer will agree with me that the cases where the use of the higher calculus is indispensable in our practice are so few that its study is not worth the time expended on it; and we have the highest authority for saying that unless its use is constantly kept up we become too rusty to use it at all." "It is true," says Mr. Fuertes," himself an accomplished professor of civil engineering, "that there are very few cases in which the engineer needs the higher analysis; that not one in a hundred uses it at all; that it cannot possibly be applied unless we are quite familiar with it; that, like correct fingering on a musical instrument, it makes severe demands on our time, and that it is easily forgotten, because much of its machinery is dependent on forms or processes that must be memorized for instant choice when needed." "Practical engineers," says Mr. Charles Bender, himself an accomplished mathematician, and a graduate from a German University, "generally do not place much confidence in long formulæ; and if they once have studied mathematics thoroughly, they lose the taste for these studies after some time of practice, since they have convinced themselves of the futility of ultra-refined theoretical speculations," "In our experience of nearly half a century as an engineer," says Mr. Julius W. Adams, "we have very rarely found that engineers possessing this peculiar facility for minute mathematical analysis, with the consequent reliance upon its infallibility which usually accompanies it, were safe guides, either in the design or execution of novel projects."

Many persons seem to think that if we can put anything into mathematical language we have done all that is necessary; that the mathematical mechanism can take the place of actual facts; that the lever is enough without the applied power. "We must not," says a recent writer, "confound mathematical skill in the making and manipulating of formulæ with science. There is a vast difference between the misuse of the higher mathematics and the proper use of those mathematical principles which lie at the foundations of all engineering operations." Mathematical skill must be tempered with a good deal of judgment before it can be of any very great use. It has been said by those who admit that the higher calculus is of little actual service to the engineer, that while it may not be really necessary for use, it is very desirable for discipline. This sounds quite well, but it is very doubtful if the average student gets from the study of the higher calculus the discipline that he is supposed to. It is very easy to deceive ourselves into believing that the student *does* get what by our theories he *ought* to get. Not many years ago the custom was very general in our colleges to oblige all students to go through the differential and integral calculus. This was done purely on the ground of discipline, for no one ever claimed that it was to be of any use to the student. It was found, however, after many years, that, except in rare cases, the student utterly failed to get any return at all commensurate to the amount of time given to this study. For the great num-

ber it was merely an inducement to shirk duty, and a means for getting slovenly habits of study. It is now almost universally abandoned as a required study in colleges. The fact is now recognized that much better discipline is had by doing a more simple thing well than by doing a difficult thing badly. To a certain extent a person must have mathematical genius to do good work with the higher calculus. An old mathematician being asked by what rule he performed a certain operation, replied, "By the rule of Gumption;" and it is certainly the fact that the inventive faculty, nay, even the imagination, enters as a larger and larger factor in the mathematics as we climb to higher and higher planes in that science. It has been said of the poet that he is born, not made. It may be said of the mathematician that he must be both born and made. Mathematical talent of a high order is rare, and any system of instruction that is based upon the assumption that all students can profitably spend a large amount of time upon an extended course of advanced mathematical work, I believe to be a faulty system. On the ground that we should do the greatest good to the greatest number, I hold that we should make our course in engineering study useful rather than difficult. It is much more important to make the student very familiar with the more simple things that he is going to use, than to cram him with the more difficult things that he will never use. Our instruction should be adapted to the average of good students, and not to a few exceptionally bright ones, always endeavoring, of course, to raise the average, and always providing advanced work for those who can take it. The school should be like a good garden for hardy plants, and not like a hot-house for forcing an unnatural growth. Mathematics is called an exact science, and those who have but little experience are apt to think that any mathematical operation must of necessity produce an exact result. This over-reliance on mathematical processes is a sure sign of inexperience. Only when the mathematical mechanism is applied to the right facts, and with correct judgment, do we get reliable results.

It is well understood among engineers that all ordinary engineering problems, mathematically considered, are quite simple. Look at any of our great engineering works, and see how much mathematics was needed to carry it out. Take from civil engineering all that the higher mathematics has ever done for it, and see how much it would be damaged. See what one of our great works we should lose, if the calculus had never been invented. A short time since there was held at Washington an examination for the position of civil engineer in the army. Government wanted four civil engineers. The number of applicants was very large, for the position was a good one, and a permanent one. The candidates were subjected to a long and searching competitive examination, conducted by five expert civil engineers. There was not a question from beginning to end that involved anything beyond the most elementary knowledge of mathematics or mechanics. The examination was marked throughout by fairness and common sense, the desired result being to get not an engineering scholar, but a man who would be of the most use to the government as a sound, practical engineer.

It is certainly not advisable to consume too much of the student's time in mathematical discussions of engineering questions, for not only are such discussions of little or no practical use, but the pupil acquires a habit of regarding all engineering problems as capable of solutions far more exact than is possible, considering the defects inherent both in materials and workmanship. A grave defect in our mode of instruction is that instead of fitting the student to deal with engineering problems *as they are*, we fit him to deal with these problems *as mathematicians assume them to be*. A glance at any of the ordinary text-books will show how much more they are mathematical than practical. The market has been flooded during the past ten or twelve years with more or less mathematical works upon bridge building, but we shall look a good while in these books for any information of practical use that we did not possess before. Had we more books like Clark's description of the Quincy bridge, or Chanute's work on the Kansas City bridge, we should be a good deal better off than we are; but,

strangely enough, we have to go to Europe for the best published illustrations of our own engineering works. The tendency of many recent writers to indulge in mathematical recreations has produced works far more interesting to the authors than valuable to engineers. We can hardly wonder, from the general aspect of many of the papers in our professional magazines, at the remark of one of our oldest and best engineers, that science seems to consist in burying the simplest facts completely out of sight under heaps of mathematical rubbish; or the criticism of another of our professional veterans, that such methods seem very much like using a sledge hammer to break an egg.

The idea that has sometimes been expressed that an engineer must at any time be able to go to the foundation of any formula which he may have to use, shows simply an entire lack of appreciation of the work an engineer has to do. Indeed many of the formulæ are found upon examination to have no foundation on which any reliance can be placed. A very large part of the rules in the books have been made, not by engineers, but by mathematicians, or by mere engineering scholars; and however admirable they may be as specimens of mathematical reasoning, they are of little or no use in practice. Take the whole matter of stone arches, of retaining walls, of dams, of the pressure of earth work, the results of the higher analysis are for the most part of no practical value whatever, and serve only to confuse and disgust the student with what treated in a simple and practical way may be made both useful and interesting. One of the most distinguished engineers has said with a good deal of truth that "trying to apply the higher mathematics to engineering is like looking into the clouds with a telescope of high power in search of facts within our grasp on the surface of the earth."

"If," says the late Milnor Roberts, "it was required to designate the one essential requisite of an engineer, we would place *judgment* first. An engineer to be great or strong must have other qualifications, but lacking that essential element, judgment, he would be incapable of availing himself even of brilliant talents." This point is of so great importance that I may be permitted to quote a few lines from a very masterly address delivered by the late Professor Faraday, before the Royal Society of London, on the Education of the Judgment. "A great deficiency," says Mr. Faraday, "In the exercise of the mental powers in any direction, is deficiency of judgment. Failure to draw correct conclusions comes far oftener from error of judgment than from error of sense. I believe that the judgment may be educated to a very large extent. There is one point in this education of the judgment which is very important, and very difficult to deal with, because it involves an internal conflict. It consists in the tendency to deceive ourselves regarding all we wish for, and the necessity for resistance to all these desires. It is impossible for any one who has not been constrained by the course of his occupation and thoughts to a habit of continual self-correction to be aware of the amount of error in relation to judgment arising from this tendency. The force of the temptation which urges us to seek for such evidences and appearances as are in favor of our desires, and to disregard those which oppose them, is wonderfully great. The inclination we exhibit in respect of any report or opinion that harmonizes with our preconceived notions can only be compared in degree with the incredulity we entertain toward everything that opposes them. I believe that point of self-education which consists in teaching the mind to resist its desires and inclinations, until they are proved to be right is the most important of all. One exercise of the mind which largely influences the power and character of the judgment is the habit of forming clear and precise ideas, so that vivid and distinct impressions of the matter in hand may remain. In like manner, we should accustom ourselves to clear and definite language, giving to a word its true and full but measured meaning, that we may be able to convey our ideas clearly to the minds of others. When the data have been collected, and we have succeeded in forming a clear idea of each, the mind should be instructed to balance them one against the other, and should not be allowed carelessly to hasten to a conclusion. Again, we should drill ourselves to being

able to form a *proportionate* judgment. The mind naturally desires to settle on one thing or another, and that with a degree of absolutism which is irrational and improper. In drawing a conclusion it is very difficult, but not the less necessary, to make it *proportionate to the evidence;* and in some cases the exercise of the judgment ought to end in absolute reservation." In commending these words of Mr. Faraday to the student, we may add the not less important advice of a distinguished English geologist, Sir Henry La Bêche: "It can only be amid a thousand errors, and by a determination to abandon our preconceived opinions when shown to be untenable, not by pertinaciously adhering to them because we have once adopted them, that we can approximate toward the truth. By strictly advocating a particular theory, prominently displaying the facts only which appear to afford it support, we are in perpetual danger of deceiving ourselves and others. Facts of all kinds, whether in favor of or against our views, should be honestly brought forward, in order that those whose opinions are unprejudiced may fairly weigh the evidence adduced." The world has just lost a man who was remarkable for absolute perfection of judgment; and we as engineers may well learn from the greatest of modern philosophers, who during twenty years of intense labor persistently withheld his judgment, but patiently accumulated the facts upon which the doctrine of evolution now firmly stands.

In all our training of the engineering student, it is to be kept in mind that we are not only to give him the best standard information for his future use, but we are also to give him, as far as possible, the power of acquiring information, and of knowing how to use it when he gets it. Above all we must not narrow nor distort his mind by partial training, so that after his studies are completed he will have to spend half a dozen years in unwarping it. We must rather give his mind that healthy elasticity that shall enable it to maintain its true form under all conditions. We are not to furnish the student with blue glasses nor with green glasses, but with white glasses, through which he shall see things as they are. We should also use every means to make the pupil as he progresses rely more and more upon himself. It will, perhaps, be asked if we expect to give the student anything that will take the place of natural talent. We never can find anything to take the place of hard work; we never can find anything to take the place of devotion to duty; we never can find anything to take the place of honesty; but we can find something, which, as far as it goes, will take the place of natural talent, *i. e.*, artificial talent or education. The number of engineers possessing any very decided engineering genius is and always will be small. The great mass of the profession will always be, as it always has been, composed of men of average ability, who rely largely upon a thorough training for their business. The engineering works in this or in any other country which show brilliant genius are quite few. The greater part are not works of genius, but of good, practical, business capacity.

Suppose I should take a young man to give him private instruction in civil engineering. How would I go to work? Would I put Weisbach or Rankine into his hands, and tell him to read those books? Not at all. I would take him out with the transit and level. I would show him how the simplest things were done, and I would make him do them himself over and over again until he knew the practical detail thoroughly. Then I would show him the mathematical principles underlying that much practice; and little by little I would make him rely on himself for his methods of proceeding. Next I would show him some of the materials he is to use. I would take him to the quarry, to the brick-kiln, to the rolling mill and the foundry, and have him see the process of obtaining the materials of construction. I would show him where and how these materials are employed in engineering works. He should thus find what tension, compression, cross strain and shearing mean. He should see these materials broken under various conditions. He should learn by inspection what the fracture of various kinds of iron indicates. He should learn to judge of the quality of materials by actual contact with these things. Next, I would show him how these various materials are combined in engineering structures, with the appa-

ratus used in such work. I would have him note carefully all of the practical details of building, the mixing of mortars and cements, the laying of brick and stone. I would keep him ever on the watch for bad workmanship and faulty methods. I would be especially careful to call his attention to any defective structures, to any signs of failure, and make him find the reasons for such defects. I would have him at every point note carefully the difference between the methods of the workman and the methods of the books. He should find, what the workman rarely, if ever does, the *reasons* for his methods. At the time he is thus laying the foundation of the facts of engineering, I would step by step show to him the mechanical principles underlying those facts. I would not bother him with fine-spun theories in regard to the strength of materials, the theory of elasticity, or the stability of structures; but I would show him simple modes for doing simple things. I would never for a moment let him suppose that there was such a thing as a conflict between practice and theory. I would so train him that he should never regard science and art as two separate things, but rather as two parts of one and the same thing. After he had got steady on his legs, I would gradually bring him to see more complex and difficult problems. I would show him plainly that many problems are quite indeterminate, and that often a purely empirical method of solving a problem is not only sufficient, but is better than any other. Above all, I would train him in such a way that he should learn to know, as Lord Bacon has put it, "the relative value of knowledges." Finally, I would take every possible means to make him well poised and absolutely honest in judgment.

We cannot, of course, deal with large classes as we would with an individual, but we can certainly approach the method. At all events we need not go out of the way to invert the mode. Our courses of study assume all students to be alike. This is to a certain extent necessary; but as the pupil advances the work may be made more and more individual, to some extent: we may thus fall in with nature and work with her, instead of pushing against her. We must not only be sure that the right subject is presented in the proper manner and at the right time, but we must remember that to have the matter properly presented is only half the work. It must also be properly received. The great mass of students are always able to understand any ordinary subject when it is properly presented, and we must make sure as we go along that the pupil actually gets what we intend he should. No two students are alike; each has his own difficulties, each looks at a subject in his own way; and not only is it quite proper that he should, but the general progress of a class is far more satisfactory from this difference in its members than it would be were they all alike. The result of our course should be not so much to have made the student read certain books, as to make him able to do certain work. A man may have Weisbach and Rankine by heart, and yet know little or nothing of engineering; he may apparently have a perfect knowledge of all the books, and yet have no real knowledge of the subject.

We can in teaching make a subject repulsive or we can make it attractive. There is a vast difference between simply hearing lessons and giving instruction. The duty of an instructor is not merely to unload all the knowledge he has at the student's door, never waiting to see that he takes it in and uses it. We are not to regard the engineering student as a school boy, but we are to take him by the hand and make a companion of him. There should be always attraction between the teacher and the pupil, never repulsion. The study of civil engineering cannot be made easy, but it can be cleared of unnecessary obstructions, and I regard it as a prominent part of our duty to do this. We have got to work and to make the student work; but we must work along the line of least resistance. We want our labor to produce useful result, and not to be wasted in overcoming friction.

We often hear it stated that the business of the school is not to deal with details, but with what are somewhat vaguely termed general principles. I believe there never was a greater fallacy. No general principle can possibly be applied to engineering construction except by means of practical details; and in many cases the details are more important than the principles. I believe

that just as engineering practice preceded engineering science, so in our course of instruction we must have a soil of practical conceptions in which the theoretical plant can grow. To give a young man an exhaustive theoretical discussion in the school, and to tell him that by and by he will run across the practical details, is very much like setting out a plant upon a brick sidewalk, and trusting to luck to get some earth about its roots at some future time. We can stop in the school to study and discuss details with the student, but it is not the business of an employer in actual work to instruct his assistants, and he never does it. I don't mean to say that experience can be taught in a four years' course, but there are a great many results of experience that can be presented to the student which will save him a vast deal of time and trouble, and at the same time show him exactly what his work is like. There is nothing more important than that the student should be made to see very plainly that no mechanical philosopy whatever can be applied to practice which is not subject to the unavoidable imperfections of materials and of workmanship, and I hold that one of the most important things we have to do in the school is to show how far the contingencies of workmanship limit the applications of science. We should make the connection plain between the science of engineering and its practical applications. We should give the student that knowledge of practical details which shall enable him to modify his theoretical knowledge in accordance with the requirements of practice. For lack of this the student wastes his time, the practitioner blunders along without the aid he might have, and a fancied conflict appears between two things, neither of which can do its best without the aid of the other.

It has sometimes been said that the work of the school is not so much to enable a young man to be at once serviceable to his employer, or profitable to himself, as to lay the foundation on which he can build the highest professional character hereafter. This remark would seem to indicate that these two results are in some way discordant. If engineering is properly taught, there can be no possible conflict here. There is no reason why the same process should not produce both of these results. There is no possible reason why the instruction should not be given with a view to immediate usefulness and at the same be given in such a manner that it can be expanded to any desirable extent.

There is one point upon which I am very particular not to be misunderstood. I do not wish to be thought in favor of omitting the study of any part of the higher mathematics. I am not one of those who think that nothing should be taught unless we can see in it some immediate use. I make no objection to the time given in our engineering schools to Descriptive Geometry. It is of no practical use. Very few engineers know anything about it, and those who do never use it; but I regard it as one of the best studies we have for discipline, as it develops a faculty in the student which is reached in no other way. The modern languages again are not essential for the engineer. I know the theory is that he reads the French and German engineering works. I know equally well the fact that he does not. For all that I regard the study of the modern languages, or of any language, as of great importance to all students, and I should value it as a most useful part of the education of an engineer, though I knew that he would never use it in his life.

It is very necessary that the student should be taught to be careful about little things, and not to think because he may be all right in the matter of general principles that the details will look out for themselves. It would, indeed, be more correct, in many cases, to say: Look out for the details and the general principles will take care of themselves. On the other hand, the student should learn, what is not less important, when *not to be exact.* He should learn that a lack of method is sometimes preferable to too much method; that very often a purely empirical mode is better than any other. It is in many cases neither bad engineering nor bad logic to assume a solution for a problem and verify it afterwards. We might, perhaps, make a general rule for finding the position of a slope stake in a side-hill cutting, but we can put the stake in by the common mode of trial a good deal quicker than we could apply a general formula.

In laying out our work in the engineering school, it is very important to make the course comprehensive. We owe this not only to the students, but also to the school; for by so doing, we need not only deal more fairly with the young men, but we insure a more steady supply of students, as we make the school more independent of fluctuations of special departments of engineering business. Many of our schools seem to have assumed that the subjects of railroads and bridges cover the whole field of engineering, and ignore entirely the matters of hydraulics, water supply for cities, sanitary work, drainage and irrigation, river and harbor work, and other equally important branches. Especially do the schools appear to have neglected altogether any endeavor to make of the student a good business man. There can certainly be no more important exercise for the engineering pupil than the careful study in detail of well-made specifications, of contracts, and of the strictly business portion of engineering operations, and certainly nothing has been more completely neglected. We graduate young men who are more or less familiar with the mathematics, chemistry, physics, geology, astronomy, but quite unable to examine the simplest accounts of a contractor, or to judge in any way of the economy with which any work is being carried on. The power to handle large bodies of workmen, and to obtain the greatest useful result from a given expenditure of money, is no doubt very largely a natural gift; but a very useful amount of this power can certainly be gained by close study and careful attention to detail, and this point should at all times be kept before the student.

It was easier in old times to be a noted engineer, when the number engaged in the business was small, than at present, when the profession is large. We have now to supply a great body of engineers. The work of the school is to give a good education to the average student. The masters in the profession will still be few and far between, and either with or without the aid of the school will take the prominent places. We must expect now and then to see a genius arise superior to all schools, who sets all educational theories at defiance and strides out far in advance of the best graduates. Teach as we may, Nature will continue to do as she always has, graduate her favorite pupils in her own manner.

In deciding just what to teach in the engineering school we must remember that we cannot teach everything in four years. We should devote that time to the more elementary things and to the more important things, while the more advanced but less essential things should be made the subjects for a post-graduate course. In the latter might come the higher mechanics, the higher surveying, geodesy, practical astronomy, experimental physics, and the like, for such students as have a taste for these things. In this part of the course too, the individual tastes of the student may be allowed a certain degree of control in the selection of studies, which is not practicable, even if desirable, in the more elementary part of the course. I regard this as a solution of many difficulties which have developed under our present system of instruction, which seems to me defective in attempting to crowd too much and too difficult work into a four years' course for a class of students quite young and quite unused to higher technical studies.

In carrying out our course of training for the young engineer, we are never to lose sight of the fact that we are not only to teach our students to be engineers, but also to be men. The civil engineer has a connection with the public welfare that no other professional man has. Quackery is more fatal in this than in any other business. The hundreds of millions of passengers that annually travel over our railroads place their lives in the hands of the engineer. He is bound to provide for their safety. It is his privilege and his duty to do so. In the words of England's greatest engineer, "A man's ability is a debt he owes to the welfare of his fellow men." In all his labor the engineer should make self subordinate to the general good. It is not the business of an engineer to erect monuments to himself with his employer's money. That money is given to him in trust, and to waste it to gratify his own pride is violating that trust. To do the required work in a proper manner for the least money is what is re-

quired of the engineer. The student cannot too carefully study the lives of the older engineers, Smeaton and Brindley, and Rennie and Telford, and the Stephensons in England; Gauthey, Navier, Perronet, and Rondelet, in France, and Bryant and Baldwin, Jervis and Childe, Knight and Latrobe, Geddes and Wright, McNeil and Whistler, and a host of others in this country. Let him carefully study the lives, the character and the works of these men, and above all, let him take to heart these words of one of the foremost living members of the profession, a man of whom American engineers are justly proud: "What should be our highest aim as engineers? Should it be to stand at the head of the profession, or to scrupulously discharge the duties of the positions in which we are placed? If the former, then we have many chances of failure to one of success, which will so often depend upon circumstances entirely beyond our control. If the latter, then success depends upon ourselves, for it is assured by a simple and constant attention to the requirements of each occasion as it arises." Engineering is no less a business than a science, and the most successful engineer will be as much a business man as a scientific man. Our roads and railroads, our locks and canals, our bridges and tunnels, our sanitary and hydraulic works throughout the country have been the result of very simple science, but of a great deal of practical skill and of good sound business talent.

As a science, engineering is a very simple one; as a business it is often exceedingly complex. To train our young engineers so that they may produce the greatest useful result with the least expenditure of money, it must be kept in mind that to a thorough knowledge of the more elementary science must be added the most careful and faithful attention to the practical details of engineering business.

THE INDUCTIVE AND CONDUCTIVE CIRCUITS.

By JOHN T. SPRAGUE.

From "The Electrician."

It is generally considered that static electricity and dynamic electricity require distinct treatment. The result is that few people acquire clear conceptions as to both branches. and the formulæ and laws of the one are apt to prove an incumbrance when studying the other. The real distinction between the two is, however, nearly unknown, and not pointed out in any of the books. That distinction is that there is no such thing as a *quantity of static electricity*. It is quite true that we have two definitions cf such a quantity, but those definitions themselves disprove its existence when studied.

The coulomb appears to be a unit of quantity, and it really is so as to dynamic electricity. The microfarad appears to be a receiver capable of comfortably holding a quantity of electricity like a pint bottle would a quantity of air, and that in ratio of the pressures put upon it.

The coulomb is, however, a quantity of electric *action*, not necessarily a *quantity of electricity*, even of dynamic electricity, because its truth and utility are equal, whether we consider electricity as an entity, a fluid, or as merely a function of molecular or ethereal motions and changes. Its value is fully definable as a material, actual quantity, because it releases a definite weight of hydrogen or other substance. This gives it a quantitative material value. A micro coulomb is one-millionth, and we can bottle this up in a microfarad and take it out again. Is that quite sure? Is it *electricity* we have put into our bottle?

What is the microfarad? The only essential part of it is a *dielectric*. The coatings only serve as the neck of the bottle, a pathway—electrodes. What, then, is a dielectric? It is a substance in which a certain state of *stress* can be produced, developing a *field of force;* that field of force is of such a nature that it is produced by what we call electricity, and will give us back the energy stored up in the stress as electricity again. Then it may be energy stored under par-

ticular conditions, not electricity, just as in secondary batteries we can store energy in the form of chemical affinity ready to reproduce electricity.

The other unit of electro-static quantity is that quantity which, placed at unit distance, repels an equal similar quantity with unit force. But the very definition tells us that this is a quantity of a force; it is simply the reaction of the "field of force" existing between the two electrodes.

Returning to our bottle of air, we find in it two things, a "quantity," the air itself—a "pressure" exerted by the air. Are not these the two things we find in electricity—a something definable as a "quantity" in dynamic electricity, which something exerts a "static pressure," related to the energy charged upon the "field of force" generated in that air? In this case, however, the air itself plays the double part; in electricity, whether there be a quantity of it having existence or not, it needs another substance, the dielectric, to store the energy, which, however, it may be said, corresponds to the material of the containing bottle.

The distinction drawn does not depend on the analogy, for this always fails at some point. The point itself is that the phenomena of static electricity are related, not to quantities of electricity, but to energy operating in a field of force; whenever "quantity" comes into play, as in discharge, we are no longer dealing with static, but with dynamic electricity. To make this clear it is necessary to consider some of the actions which occur.

According to the mathematical theory of electricity, current, &c., discharge, &c., are due simply to *difference of potential*, the actual position of the two points on what we may call the "scale of potential" having no influence. This is, of course, analogous to currents generated in a column of water, which may be regarded as a "scale of potential;" such a current issues from any point in the column as is due to the head of water above that point, however high up in the scale it may be itself.

The same effect will arise between two points in an electric circuit, whether those points are at + 15 and + 5 potential as regards zero, and though both of them have free + charges, as would occur between two points at + 5 and — 5, with equal free + and — charges. The facts are, of course, true, and for some of the purposes of calculation, the theory has its advantages. But the theory is not true as a fact of nature. The water analogy itself disproves it; the column of water below the point at which current passes is inert, *it is not in the circuit*. The same is the case with the electric circuit. It is true that we can have two Leyden jars charged to such differences, with their external coatings connected together, as will represent the scale of potential, the external coatings being the + and —. But then we may connect the junction of the external coatings to the theoretical zero of potential, the earth, and no current will pass. The analogy to the opening in the column of water fails. But why? Simply because we have two distinct inductive circuits. Each jar is complete in itself, just as a battery cell is. We can discharge the jars singly, just as we can take current singly from two cells joined in series, or we can discharge them in series with the effect due to the sum of the potentials, just as we can use the cells.

In fact, each jar, *i. e.*, each inductive circuit, has its own scale of potential, of which the — is at zero. The theoretical, or earth zero, is a mere artificial invention for purposes of calculation; it is precisely the same with the column of water. The total height of the column has no actual relation to the result, and the zero is not the earth's level (in fact, if anywhere, it would be at the earth's center); but for each current the real zero is the point at which it issues.

To meet the fact that no discharge will occur at the common junction to "earth," although that junction is at + potential, the other artificial doctrine of *bound* electricity is invented. The free charge on the inner coatings dissimulates an equal quantity on the corresponding outer coating; but what is the use of this complication when the case is absolutely analogous to that of the two galvanic cells? As long as the cells are open, they are under identical conditions with the charged jars; in both there is a break of connection—an inductive circuit between the poles, which the force cannot break down. Under these circumstances

we can insulate the jars, either connected or separate, and we can give them a *free charge*. But what is this but adding a third inductive circuit? We can even charge the external coatings + or —, but in so doing we simply make the outside surfaces the one coating to the air of the room to which the other coating is the surrounding surface. We can even put opposite free charges on the two jars in the same manner, and we can make these charges either the same or the opposite of the charges which their inner surfaces bear as regards the inner coatings.

All this becomes perfectly clear when we examine each separate inductive circuit as we should the circuits of currents, which every one knows may be absolutely distinct though they all unite at a common point, or even traverse some part of a common conductor.

We may now contrast and compare the two orders of electric circuits.

The *conductive circuit* consists of matter which allows electrical action to take place in the form of current, or we may say it permits electricity to pass. It does so in quantities related to the electromotive force, to the conducting capacity of the material substance, and in producing current the electricity expends energy.

The *inductive circuit* consists of matter which does not transmit electricity, but admits of the formation of a field of force manifested as *charge* at its opposite sides. Such "charge" is proportional to the potential, to the inductive capacity of the material called the dielectric, and in producing charge the electricity expends energy.

It will be better now to present [the relations in parallel columns:

Static.	*Electricity.*	*Dynamic.*
Energy *potential;* stored in a field of force, as stress in a dielectric	means	energy *kinetic;* acting in a conducting path along molecular chains.
Inductive.	*Capacity.*	*Conductive.*
Area of dielectric. Spec. ind. capacity of dielectric.	varies as	area of conductor. Specific conductivity of material.
Thickness.	inversely as	length.
Inductive.	*Resistance.*	*Conductive.*
Reciprocal of conductive capacity.	is the	reciprocal of conducting capacity.
Charge.		*Current.*
Potential in Volts.	is as	electromotive force in volts.
Resistance.	inversely as	resistance.
Inversely as the several resistances.	divides among several circuits.	inversely as the several resistances.
Lines of forces under stress.	is related to and dependent on	lines of equivalent molecules forming chains.
	Energy, or Work.	
Square of charge in unit or equal resistance.	is as	square of current in unit or equal resistance.

FORMULÆ.

Charge $\frac{E}{R}=Q$ Current $\frac{E}{R}=C$

Potential $Q\times R=E$ Electromotive force $C\times R=E$

Resistance $\frac{E}{C}=R$ Resistance $\frac{E}{C}=R$.

As to the capacity, the specific inductive capacity is measured in its own terms, by the charge received under unit potential by a unit dimension, as, for instance, a square foot of 1 mil. in thickness of the material. Specific conducting power is usually and most conveniently measured as a resistance of a unit dimension, such as a wire 1 foot long and 1 circular mil. in area. Resistance and capacity being reciprocals of each other, it is indifferent under which term it is measured.

The influence of thickness of dielectric is taken here under its simplest form, as a plate whose area vastly exceeds its thickness, which is practically in the form of concentric spheres of very large radius. This is correct, not only because it is simple, but because the comparison is really to be made between the areas which would transmit a unit of current or take up the same unit as charge, and this latter area is many million fold in the dielectric what it is in the conductor.

RETAINING WALLS.

By E. SHERMAN GOULD.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE title of this paper may draw a smile—or perhaps a sigh—from the readers of VAN NOSTRAND'S MAGAZINE, as their eye falls upon the familiar words. And indeed it might well seem that but little is left to be said upon what has become one of the threadbare topics of engineering literature. Still, in spite of all that has been written upon the subject, if we were asked to mention an author to whom one could go for full and intelligible information respecting the calculation and general rationale of retaining walls, what name should we give?

Without further expanding our preamble, we will at once state that a very excellent work has recently appeared from the press of I. Baudry, in which the matter is treated with such marked ability, that we feel it almost a duty to consign some of its most useful teachings to the pages of this Magazine, as the best means of making them known to American engineers.

This work is entitled "*Etudes théoriques et pratiques sur les murs de soutènement et les ponts et viaducs en maçonnerie*, par J. Dubosque."

The author establishes, and we here repeat for convenience of reference, the following well-known fundamental formulæ:

$$Q=\frac{1}{2}\delta h^2 tg^2\frac{a}{2}$$

$$M_Q=\frac{1}{6}\delta h^3 tg^2\frac{a}{2}$$

$$M_K=\frac{\pi h x^2}{2}$$

in which Q represents the thrust of the earth; δ the unit weight of the earth; h the height of the wall and sustained bank; a the angle which the natural slope makes with the vertical, and M_Q the overturning moment of Q. M_K represents the resisting moment of a vertical wall, of which x is the uniform thickness and π the unit weight of the material of which it is composed. He then puts

$$M_K=2M_Q,$$

using a safety factor of 2. From this last relation, he draws

$$x=hgt\frac{a}{2}\sqrt{\frac{2\delta}{3\pi}}.$$

He simplifies this formula by giving to the Greek letters their approved approximate values. He gives, first, to δ the value of 106 lbs. per cu. ft., and to π, the value of 137 lbs. per cu. ft.; this gives $\frac{\delta}{\pi}=0.773$. Assuming, first, $a=45°$ (slope of 1 to 1) he finds, $x=0.3h$.

Next, assuming $a=56°18'30''$ (slope of 1.5 to 1) and taking the corresponding practical value of $\delta=112$ lbs. per cu. ft. which gives $\frac{\delta}{\pi}=0.82$ (nearly) he finds

$$x=0.4h,$$

averaging $x=0.35h$.

This, Mr. Dubosque gives as the proper thickness, expressed in terms of height, for ordinary vertical walls without surcharge. We may venture a little further in the path of simplification, and write his general formula for such walls

$$x=\frac{h}{3}.$$

Further on, he solves the three fundamental formulæ given above, in terms of h. Thus, using values for a and δ corresponding to a slope of 1.5 to 1, he finds

$$Q=16h^2,$$

$$M_Q=5.4h^3,$$

and, using the maximum value of $x=0\ 4h$,

$$M_K=176h^3\ \ 11h^3.$$

Dividing one by the other,

$$\frac{11}{5.4}=2+,$$

he recovers his proposed factor, 2, of safety.

This vertical wall, with uniform thickness equal to $\frac{4}{10}$th of its height, and sus-

taining a bank of earth of equal height, having a natural slope of 1.5 horizontal to 1 vertical, the weight of the earth being 82 per cent. of that of the masonry, and including a factor of safety of 2, he calls the "typical wall." Any other wall, of whatever shape, having a resisting moment equivalent to that of this type, will fulfil the requirements of safety, barring, of course, exceptional densities and slopes.

This forms the key to Mr. Dubosque's method. Any required dimension of a wall of given section is obtained, by putting the moment of such wall of the above (or any given) thickness, in such form that the required dimension shall be the only unknown quantity.

Thus, let it be required to design a wall having an exterior batter, of given height, and given top thickness. The unknown and required value is then that of the bottom thickness. Assume a vertical wall, of equal height, with an uniform thickness equal to E, offering the necessary resisting moment. Let B represent the required bottom thickness of proposed wall, and b its given top thickness. The readiest way to obtain the resisting moment of proposed wall is probably to calculate it as if vertical, with uniform thickness equal to B, and then deduct moment of triangle having a height equal to h, and a base equal to $B-b$. The moment of the equivalent, given, vertical wall of thickness E being

$\frac{E^2h}{2}$, we have $\frac{B^2h}{2}-\frac{(B-b)^2}{6}\times h=\frac{E^2h}{2}$,

whence

$$B=-\frac{b}{2}+\frac{\sqrt{3}}{2}\sqrt{b^2+2E^2},$$

or $$B=-\frac{b}{2}+0.866\sqrt{b^2+2E^2}.$$

A numerical example is given, in which it is required to transform a vertical wall 5 meters high, with uniform thickness of $1^m.35$, into one of equal height and equal stability having a top thickness of $0^m.60$, vertical back, and battering face. These data give $B=1^m.43$. It will be observed that this transformation of section, while leaving the stability unimpaired, occasions an economy of 25 per cent. in material.

The same principle is used in investigating the subject of counterparts, or buttresses. It is known that a considerable economy of material may be effected by building a light wall reinforced by buttresses, which may be placed either against the face or back of the wall. The former is the more advantageous position for them, as they there oppose most efficaciously the overturning moment of the earth pressure; placed against the back, they have the advantage of not encroaching upon available space, and of cutting up the earth prism resting against the wall. One of the principle objections to placing them at the back of the wall is, that the earth pressure tends to tear the rest of the wall away from them, whereas in the case of an exterior position, the wall is pressed against, instead of from them.

The required data in regard to counterforts are, their distance apart, their thickness, the thickness of the intervening wall or "mask" (as Mr. Dubosque terms it) and the amount of their projection beyond the mask. Of these, all but the last are determined from the successful practice of the best constructors. Mr. Dubosque gives: distance between two adjacent counterforts, in the clear, 3 meters (say 10 feet); thickness of each counterfort, 1 meter (say 3′ 4″); thickness of mask, from $\frac{1}{4}$ to $\frac{1}{6}$ of the height. We have now only to determine x, the projection of the counterfort.

Referring to the figure we will consider the section of wall contained between the planes AB and CD, situated 4 meters apart. And first, to calculate the moment of the portion of mask $abcd$. The height of the wall will be always represented by h, and the thickness of the mask by $\frac{h}{c}$. The weight of the mask $abcd$ is

$$P(abcd)=\frac{4h}{c}\times\pi h.$$

Its lever arm is $\frac{h}{2c}+x$. Its moment is therefore

$$M_K(abcd)=\frac{4h}{c}\times\pi h\times\left(\frac{h}{2c}+x\right)$$
$$=\frac{4h^2}{c}\times\pi\left(\frac{h}{2c}+x\right).$$

The weight of the counterfort $efgh$ is

$$P(efgh)=\pi hx.$$

Its lever arm is $\frac{x}{2}$, and its moment

$$M_K(efgh)=\pi hx\times\frac{x}{2}=\pi h\frac{x^2}{2}.$$

The sum of these two moments is, for a section 4 meters in length,

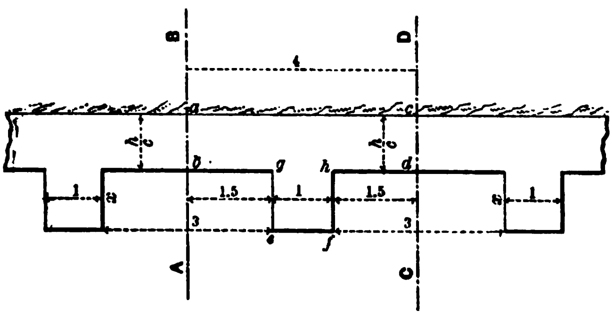
$$\frac{4h^2}{c}\times\pi\left(\frac{h}{2c}+x\right)+\pi h\frac{x^2}{2}$$

which, reduced to the unit of one meter, becomes

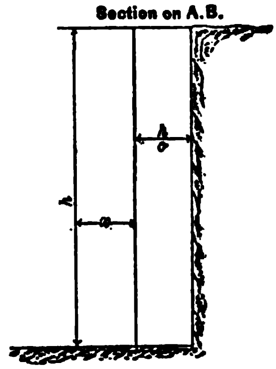
$$\frac{h^2}{c}\times\pi\left(\frac{h}{2c}+x\right)+\pi h\frac{x^2}{8}.$$

Making this equal to the resisting moment of the typical wall, we obtain

$$\frac{h^2}{c}\times\pi\left(\frac{h}{2c}+x\right)+\pi h\frac{x^2}{8}=\pi\frac{0.4^2}{2}h^3.$$



Section on A.B.



This equality takes the shape of the following equation of the 2d degree:

$$x^2+\frac{8h}{c}x+\left(\frac{4h^2}{c^2}-0.4^2\times4h^2\right)=0,$$

whence

$$x=-\frac{4h}{c}\pm\sqrt{\left(\frac{4h}{c}\right)^2-\left(\frac{4h^2}{c^2}-0.4^2\times4h^2\right)},$$

which becomes, after reduction and simplification,

$$x=\frac{h}{c}\left(-4\pm\sqrt{12+0.64c^2}\right).$$

This equation shows that as c increases the thickness of the mask diminishes, the value of x increases, and the total cube of masonry diminishes. There is therefore an economic advantage in increasing c as far as possible. As we have just seen, experience gives a value to this factor of from 5 to 6. That is, the thickness of the mask may be diminished to $\frac{1}{5}$ or $\frac{1}{6}$ of the height of the wall. Using 5 as the value of c, and considering only the positive sign of the radical, we obtain the simplified value of x,

$$x=\frac{h}{4}.$$

If we adopt a slope of 1 to 1 and take the corresponding value of δ, we obtain, calculating as before, but using the minimum value of $0.3h$ for the thickness of the equivalent vertical wall, we get, in round numbers,

$$x=\frac{h}{8}.$$

Calculations for interior counterforts are made in a similar manner. The same figures will answer for this case if we consider the earth placed on the opposite side. The value of the symbol remaining the same, we have, in the case of a slope of 1.5 to 1, and a typical wall of thickness $0.4h$, a value for x,

$$x=\frac{h}{c}\left(-1\pm\sqrt{0.64c^2-3}\right),$$

whence, in round numbers, $x=\frac{h}{2}$.

For a natural slope of earth of .1 to 1, using the same data as for the similar case with exterior counterforts, we get, in round numbers,

$$x=\frac{h}{3}.$$

As regards sliding, the familiar assurance is repeated that a wall secure against overthrow is thereby secure against slid-

ing. Thus the force provocative of sliding is $Q=16h^2$; the resistance to same is the weight of the wall multiplied by the coefficient 0.76 of friction. The resistance to sliding of the typical wall is therefore $41.6h^2$. The factor of safety is therefore given by the relation

$$\frac{41.6}{16}=2.60.$$

There is besides, the cohesion of the mortar, intentionally neglected.

It may be well to mention that in this paper we have given the numerical values of Q, M_Q and M_K, in pounds, per running foot of wall or bank, whereas in the original they are naturally in kilogrammes per running meter. For purposes of comparison it might be convenient to reduce their numerical coefficients to a simpler, imaginary unit, which we can do by dividing them all by 5.4. This would give very nearly the following relations:

Thrust or horizontal component of earth pressure,

$$Q=3h^2.$$

Overturning moment of same,

$$M_Q=1h^3.$$

Resisting moment of typical wall,

$$M_K=2h^3.$$

The above is a rapid sketch of a few noteworthy features of the admirable work of Mr. Dubosque, in so far as it relates to a certain class of retaining walls. The sections devoted to the different varieties of surcharging to which a wall may be subjected, the practical observations upon the construction of retaining walls, and the entire second part of the volume which relates to arches, piers and abutments, are left, necessarily, unnoticed.

THE PANAMA CANAL.

From the "Nautical Magazine."

In the midst of the grave crisis which has arisen in connection with Egyptian affairs, the Panama Canal question seems to have quite lost its hold on the public attention. That such should be the case is perfectly natural. At the present time the difficulties in which we find ourselves involved in the East are not to be compared with the more problematical dangers that are looming in the West. In the one case they are only too definite and real; in the other they exist merely on paper; and, while one question is being decided by the sword, the other still forms a subject for the diplomatist's pen. Yet, in spite of the small importance which the Panama Canal affair seems now to possess, in comparison with the Egyptian difficulty, it is by no means unlikely that the day will arrive—and that at no distant date—when the case will be otherwise, and when the former will become a question of the utmost gravity.

As regards the possibility of constructing the Canal there seems now to be no room for reasonable doubt. It appears to be simply a question of money, and, looking to the facility with which the means for carrying out large undertakings of this class are raised at the present day, it becomes tolerably clear that the project is by no means such a chimera as some of its objectors have supposed. The indomitable M. de Lesseps is fully convinced of the practicability of the scheme, and he has thus far experienced no difficulty in imparting a share of his own confidence to those who can provide the sinews of war. Ample means are forthcoming for the preliminary operations, and these are being actively pushed forward. Contracts of considerable magnitude have been entered into for constructing the Colon end of the Canal, a large stock of contractor's plant is being accumulated at various points, and M. de Lesseps assures the shareholders that within the next seven years his scheme will have become an accomplished fact. He contends that the physical difficulties of the undertaking have been greatly exaggerated, that much of what was supposed to be hard rock is found to be soft earth, that the climate is not unhealthy, and that the tradition which places the bones of a Chinamen beneath every sleeper on the

Panama Railway is all idle nonsense. Of course, some allowance must be made for the naturally roseate views and statements of those who have to induce capitalists to advance the sum of £24,000,000 which the Canal is to cost; but when this has been done there seems to be no ground for regarding the scheme as other than feasibie, or for supposing that the completion of the work is a question too remote to call for serious consideration at the present time.

The practicability of M. de Lesseps' scheme is not, however, the question with which this country is immediately concerned. A more serious matter for consideration is to be found in the extraordinary pretensions which have been put forward by the Government of the United States with regard to the control of the Canal when the work has been completed. Certainly these have assumed a somewhat more moderate tone during the last twelve months; but, so far as we can judge from the correspondence which has thus far been made public, the subject is one on which the views of the two Governments are still widely divergent. As a diplomatist Mr. Frelinghuysen is considerably in advance of Mr. Blaine. The tone of this gentleman's remarkable dispatch of June, 1881, can hardly be termed conciliatory, or be regarded as likely to lead to a satisfactory solution of a difficult and debatable question. To open the ball by talking in grandiose style of the possessions of the United States on the Pacific coast "Imperial in extent and extraordinary in growth"—to completely ignore the Clayton-Bulwer Treaty of 1850, which gave England a voice in the question of an interoceanic canal, and to say that for her to attempt to supplement the treaty made in 1846 between the United States and Colombia (formerly New Granada) would necessarily be regarded as "an uncalled-for intrusion," and that any step taken by the European Powers for the purpose of guaranteeing the neutrality of the Canal would be deemed to possess "the nature of an alliance against the United States," was perhaps not the best way of laying the foundation for a mutual and harmonious undertaking. At the time of composing this dispatch Mr. Blaine seems to have been unaware of the existence of the Clayton-Bulwer Treaty. He subsequently became better informed, and both he and his successor in office have addressed themselves to the task of showing the advisability—or rather of pointing out, from the American point of view, the necessity of setting the provisions of this Convention on one side. In November last, Mr. Blaine stated that his Government would not "consent to perpetuate any treaty impeaching its rightful and long-established claim to priority on the American Continent," and that they regarded "the canal question as solely an American one." Mr. Frelinghuysen has adopted a somewhat less defiant tone, and has endeavored to show that the United States are warranted, by principles of justice as well as by those of expediency, in insisting on the abrogation of the Clayton-Bulwer compact. He contends that England has infringed the contract in question by colonizing Belize in opposition to the agreement that neither party should colonize or fortify in the locality and that, since England has seen fit to set this condition on one side, the United States are fully warranted in regarding the Treaty as altogether void. He further maintains that the arrangement in which England took part in 1850 was made with a view to the possible construction of a Canal by the Nicaragua route, and not by way of Panama, whilst the United States, by virtue of the Treaty with Colombia, guaranteed the route of the Panama Railway from sea to sea. Mr. Frelinghuysen states that,

"Should Her Majesty's Government, after obtaining the consent thereto of the States of Colombia, claim under the Clayton-Bulwer Treaty the right to join the States in the protection of the existing Panama Railway, or any future Panama Canal, the United States would submit that experience has shown that no such joint protectorate is requisite, and that the Clayton-Bulwer Treaty is subject to the provisions of the Treaty of 1846 with New Granada while it exists, which Treaty obliges the United States to afford, and secures to it the sole protectorate of any transit by the Panama route. If Great Britain still claimed the right to join in the protectorate, the United States would then determine whether the Treaty stipulations

proposed by Great Britain regulating that joint protectorate, were just; and if so, whether the length of time during which Great Britain has recognized the protectorate of the States in Panama under the Treaty with New Granada, has, or has not, relieved the United States from any obligation to accept a proposal from that Government to join in a guarantee."

* * * * * * *

"The United States esteem themselves competent to refuse to afford their protection jointly with Great Britain, to any other Company than that possessing the original grant from Nicaragua; and they hold themselves free hereafter to protect any interoceanic communication in which they or their citizens may become interested, in such a way as the treaties with local Sovereign Powers may warrant and their interests may require."

There can be no question that Mr. Frelinghuysen is conducting his country's case with much greater skill than was displayed by his predecessor; but the gist of his argument, like that of Mr. Blaine's, amounts simply to the contention that the United States regard the Panama Canal question as one exclusively their own, and that, whatever past Treaties may say on the subject, they do not intend to allow any European Power to have a voice in its future control. The United States cannot think of trusting to any general guarantee of neutrality, but a guarantee by themselves and Colombia is, we are told, an entirely different affair.

That the Clayton-Bulwer Treaty gives England a direct claim to share in the protectorate of the Canal must be plain to any man who understands the English language. But England's claim to consideration in the matter does not rest solely on the wording of the Clayton-Bulwer convention. The value of most treaties is much more apparent than real. They usually retain their efficacy only so long as both the contracting parties are willing to abide by them, and it would seem that the case in point forms no exception to the general rule. However, in discussing the issue now at stake, the English Government can afford to leave past arrangements out of the question, if necessary, and base their claim, as the United States seem inclined to do, on the ground of present right and expediency. The States Government appear to think that the extent of their possessions on the Pacific coast warrants them in claiming the entire control of the Panama route, but England is not altogether unjustified in urging considerations of this kind as reasons why she should have a voice in the matter. She too has possessions of considerable importance at various points on the shores of the Pacific and Indian Oceans, and, what is of less consequence, so far as the Panama route is concerned, it is quite certain that if the Canal were completed, the proportion of British to United States tonnage which would pass through would be about three or four to one.

There are several reasons why the United States should wish to have the complete management of the Panama Canal. As a matter of fact the whole scheme seems to be viewed with a certain amount of disfavor by the Eastern and more influential States of the Union. They are naturally unwilling to encourage the construction of a work that threatens to divert a considerable portion of the traffic now passing through their midst, and at the same time to increase the importance of the Western States; and if the completion of the Canal depended on their good wishes, the world would probably have to wait a long time before the American Continent would be cut in two. Of course, these are considerations which cannot be openly avowed, but as there are occasionally wheels within wheels in American politics, it may be that there exists a party who are doing their best to swamp the entire scheme by political embarrassment. This, however, is a phase of the question which we need not pause to discuss. The arguments the English Foreign Office have to meet are of a totally different class. The United States Government contend that England's maritime supremacy would render her mistress of the situation, and they wish to correct this by taking prior possession of what they think would be a point of great strategic importance in time of war. They contend, moreover, that, even in time of peace, the possession of the Canal would justify them in keeping their naval armament upon a much less extensive scale than would be requisite under a general guarantee of neutrality,

and they do not wish to see created any pretext under which European navies could assemble in American waters. The people of the United States seem inclined to cherish the belief that their right to decide all questions of any political importance relating to any portion of the American Continent is plain and indefeasible, and in the claims put forward with respect to the Panama Canal this belief has taken a precise and definite form. It remains to be seen to what extent this impression can be removed, but it must be admitted that, at the present time, the aspect of affairs is somewhat foreboding.

On the grounds of reason and justice the European Powers may reasonably urge that the question is one in which they may ask to be heard. As a matter of right the United States would be as much justified in attempting to control the traffic around Cape Horn as in taking upon themselves the sole management of the Panama route. Their commercial interests in the latter are not involved more deeply than are those of England. Indeed, it would not be difficult to show that the case is exactly the reverse. The commercial and direct value to any nation of a waterway through the Isthmus would clearly be in proportion to its maritime carrying trade, and in this respect England may fairly claim priority. And, even on political grounds, her stake in the question is not less important than that of the United States. England also has possessions—to use Mr. Blaine's words—"Imperial in extent," if not extraordinary in growth; and to some of these the Panama Canal would be a direct route. Why, therefore, is she to be debarred from taking any part in the matter? If the United States are unwilling to trust any portion of their political interests to a general guarantee of neutrality, surely England is justified in declining to place her interests in the sole keeping of the United States.

It is difficult to reconcile the claims recently put forward by the present representatives of the American Government, with the language adopted by those of their predecessors who took any part in dealing with the Panama question. The latter always maintained that the United States by no means wished to usurp any undue advantage with regard to the proposed waterway, and that their main object was to secure similar and equitable treatment for all. Now this has all been changed and, if we may judge from the tone of the published dispatches, it seems as though the States have fully determined to take possesion of the Canal when made, and to set at nought all claims for consideration coming from this side of the Atlantic. The assertion of the rights of the European Powers will, of course, devolve almost entirely upon this country, and although the American dispatches upon the subject have been addressed to all the principal European States, they are really intended for England only. The results of the Conference which the United States Government proposed should be held between their own representatives, and those of the Central and South American Republics have not yet been made known, but no doubt they will prove quite satisfactory at Washington—much more so, indeed, than they will probably be found in London and Paris. It is perfectly natural that the people of the United States should feel a deep interest in the question of the Panama Canal, but they can hardly expect England to sit quietly by while her stake in the matter is being settled by their Government in conference with the agents of two or three insignificant American Republics. Nor can it be allowed that they have any sufficient reason for objecting to a guarantee of neutrality in which the European Powers generally would take part. We imagine that, if three-fourths of the tonnage passing through the Suez Canal belonged to the United States, they would wish to take at least some part in any guarantee that might be formed with respect to its neutrality. If England possessed no mercantile navy of any of any importance, they would naturally and reasonably object to a proposal that she should shut them quite out of the question, and make an Anglo-Egyptian treaty, which would permit her to seize the Canal and fortify its banks. And this is precisely the position in which we are placed with respect to the Panama route. England asks for no pre-eminence in the matter. All she requires is equitable treatment, and unless the American Government have fully determined to set considerations of right and justice totally on one side, it is difficult to see how this can be refused.

DYNAMO-ELECTRIC MACHINERY.

By PROFESSOR SILVANUS P. THOMPSON, B. A., D. Sc., M. S. T. E.

From the "Journal of the Society of Arts."

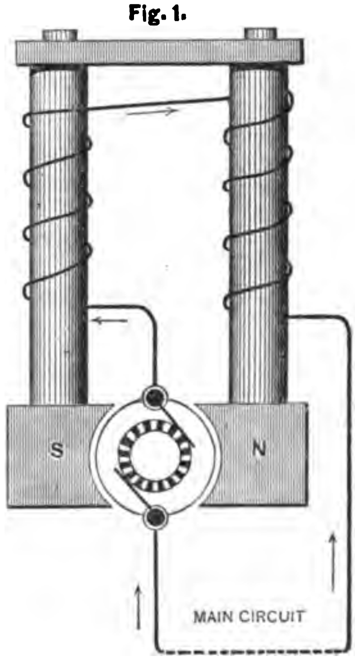
I.

The Dynamo in Theory.

By "Dynamo-electric Machinery," in the most geneneral etymological sense of the term, is meant machinery for converting the energy of mechanical motion into the energy of electric currents, or *vice versa*. From this wide defininition must be excepted machines like the well-known statical induction-machine of Holtz, whose action is purely electrostatic. Those machines only are included in the definition whose action is dependent on the principle of electro-magnetic induction, discovered by Faraday, in 1831. It is, however, not quite easy to decide what machines shall be called dynamo-electric machines, because the sense in which the term is used commonly is narrow, and restricted in a manner not quite logical.

The name dynamo-electric machine appears to have been first employed by Dr. Werner Siemens, in his communication of January 17th, 1867, to the Berlin Academy, in which he described a machine for generating electric currents by the application of mechanical power, the currents being induced in the coils of a rotating armature by the action of electro-magnets, which were themselves excited by the currents so generated. The machine was, in fact, a self-exciting dynamo, with the field magnets and armature united "in series" to the external circuit, or what we now call a "series-dynamo," a diagramatic representation of which is given in Fig. 1. But the term dynamo-electric machine, then introduced into electric technology, has not remained thus restrained to its narrowest meaning. It was next applied to machines of kindred nature, in which, though self-excited, only a portion of the entire current generated by the rotating armature was applied to excite the field-magnets (see Fig. 2). This principle of working (now known as that of the "shunt-dynamo"), first introduced by Wheatstone, is but a variation of the former arrangement in detail, and no violence is done to the original term to apply it to both cases. In fact, the name was welcomed as being convenient in practice for distinguishing such machines from those which were not self-excited—those in which either steel magnets or separately excited electro-magnets were used to produce the magnetic field.

Fig. 1.

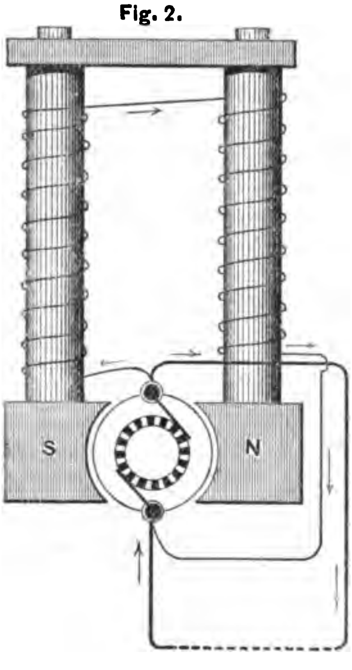


THE SERIES DYNAMO.

But since the great development of electric lighting took place, it has been found convenient to use generating machines in certain combinations, in which the self-exciting principle is abandoned. Some systems of electric lights require alternating currents, produced in machines which cannot excite their own magnets with a continuous magnetization: and there are other systems where

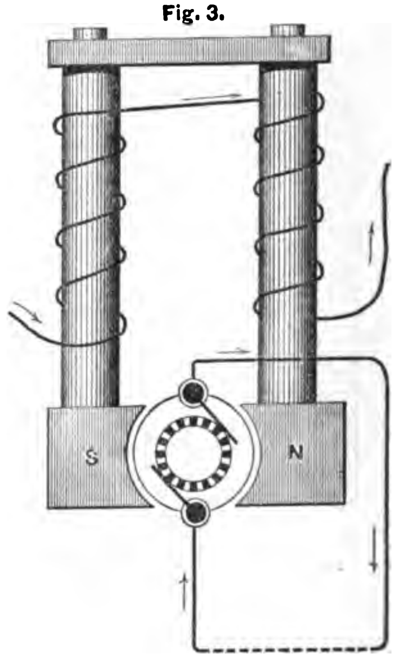
continuous currents are employed, in which also practice has shown that the currents are better regulated when the magnets of the dynamos are separately excited by currents derived from an external source. There is, then, a third class of dynamo-electric machine, the "separately-excited dynamo" (Fig. 3), which was indeed earlier than either of the preceding, having been brought out by Wilde in 1866. A dynamo is a dynamo in fact, whether its magnets be excited by the whole of its own current, or by a part of its own current or by a

Fig. 2.



THE SHUNT DYNAMO.

Fig. 3.



THE SEPARATELY-EXCITED DYNAMO.

current from an independent source. The source of the magnetizing power is indifferent, provided a magnetic "field" of sufficient intensity be produced wherein the generating coils can be rotated. Now, as it does not matter where the magnetizing power comes from, it is clear that we must include amongst possible sources the power of permanent steel magnets. In short, the arbitrary distinction between so-called magneto-electric mechines (see Fig. 4) and dynamo-electric machines fails when examined carefully. In all these machines a magnet, whether permanently excited, independently excited, or self-excited, is employed to provide a field of magnetic force. And in all of them dynamical power is employed to do the work of rotating the coils of the armature, in order to generate the electric currents.

The true and comprehensive definition of a dynamo-electric machine, is then, the following: A dynamo-electric machine is a machine for converting energy in the form of dynamical power into energy in the form of electric currents, by the operation of setting conductors (usually in the form of coils of copper wire) to rotate in a magnetic field.

Inasmuch, however, as every dynamo-electric machine, in the most general sense of the term as now laid down, will work as a motor, and becomes a source of mechanical power when supplied with electric currents, it is possible to discuss dynamo-electric machinery from the opposite points of view in serving the two converse functions, in short, to treat the dynamo on the one hand as a generator, on the other hand as a motor.

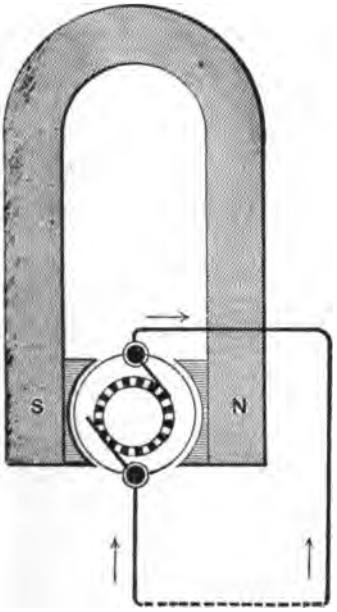
It is as a generator only that the action

of the dynamo will be considered in the first two of these lectures, reserving considerations respecting its functions as a motor for the third lecture of the present course.

The Dynamo in Theory.

To treat exhaustively the theory of dynamo-electric machines, would require not one lecture, but many. It would be necessary, moreover, to enter upon lengthy mathematical and geometrical discussions, which would be out of place in the present course, which is expected to deal with subjects from an industrial, rather than from a purely scientific aspect. The mathematical theory of the dynamo is, indeed, very complex, and takes different forms for its expression in the four classes of machine now included under the one name of "dynamo." For every different variety in each of these classes, there is a fresh variety of mathematical symbols. The theory of alternate current machines is entirely different from that of machines which are to furnish continuous and constant currents. Every form of armature and coils requires its own specific treatment in symbols; and the simple consideration of putting iron cores into the coils, when treated mathematically, introduces such complex expressions as to yield no hopes of a satisfactory general solution.

Fig. 4.



THE MAGNETO DYNAMO.

The theory of the dynamo, then, which will be developed in the present lecture, will not be a general mathematical theory. The aim will be to deal with physical and experimental rather than mathematical ideas. A physical theory of the dynamo is not new, though none of great completeness has yet been given,* most of such explanations being devoted to single machines of some particular type.

It will here be my aim to develop a general physical theory, applicable to all the varied types of dynamo-electric machines, and to trace it out into a number of corollaries, bearing upon the construction of such machines. Having recited these consequences, which we shall deduce from theory, it will then remain to see how these consequences are verified and embodied in the various forms assumed by the dynamo in practice.

Physical Theory of Dynamo-Electric Machines.

All dynamos are based upon the discovery made by Faraday in 1831, that electric currents are generated in conductors by moving them in a magnetic field. Faraday's principle may be enunciated as follows: When a conductor is moved in a field of magnetic force in any way so as to cut the lines of force, there is an electromotive force produced in the conductor, in a direction at right angles to the direction of the motion,† and at right

* See Du Moncel ("Exposé des Applications de l'Electricité," vol. ii.); Niaudet, "Machines Electriques;" Schellem, "Die Magnet-und Dynamo-elektrischen Maschinen;" Antoine Breguet (see "Annales de Chimie et de Physique," 1879). See also some theoretical papers by Clerk Maxwell in "Philosophical Magazine," 1867, and by MM. Marcel Deprez, Cabanellas, de Gourant, and others in the "Comptes Rendus;" also sundry papers by Frölich, Werner Siemens, Hagenbach, and others in the "Elektrotechnische Zeitschrift" and other German publications. Of more general scope is the physical theory advanced by the late M. Antoine Breguet, which deals with dyamos of the Gramme and Siemens type, and of which a detailed summary is given in "Electric Illumination." In an introductory chapter of the same work I have endeavored to lay the foundation of a more general physical throry. An admirable series of papers on the mathematical theory of dynamo-electric machines, from the pen of a well-known professor of physics, is at present appearing in the columns of *The Electrician*.

† Or, more strictly, to the resolved part of the motion in a plane perpendicular to the lines of force.

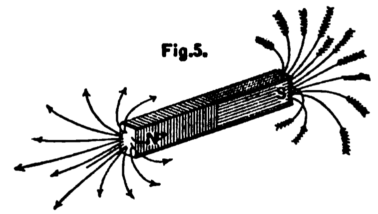
angles also to the direction of the lines of force, and to the right of the lines of force, as viewed from the point from which the motion originates.*

This induced electromotive force is, as Faraday showed, proportional to the number of lines of magnetic force cut per second; and is, therefore, proportional to the intensity of the magnetic "field," and to the length and velocity of the moving conductor. For steady currents, the flow of electricity in the conductor is, by Ohm's well-known law, directly proportional to this electromotive force, and inversely proportional to the resistance of the conductor. For sudden currents, or currents whose strength is varying rapidly, this is no longer true. And it is one of the most important matters, though one too often overlooked in the construction of dynamo-electric machinery, that the "resistance" of a coil of wire, or of a circuit, is by no means the only obstacle offered to the generation of a momentary current in that coil or circuit; but that, on the contrary, the self-induction exercised by one part of a coil or circuit upon another part or parts of the same, is, in many cases, quite as important a consideration, and in some cases a more important consideration, than the resistance.

To understand clearly Faraday's principle—that is to say, how it is that the act of moving a wire so as to cut magnetic lines of force can generate a current of electricity in that wire—let us inquire what a current of electricity is.

A wire through which a current of electricity is flowing looks no different from any other wire. No man has ever yet seen the electricity running along in a wire, or knows precisely what is happening there. Indeed, it is still a disputed point which way the electricity flows, or whether or not there are two currents flowing simultaneously in opposite directions. Until we know absolutely certainly what electricity is, we cannot expect to know precisely what a current of electricity is. But no electrician is in any doubt as to one most vital matter, namely, that when an electric current flows through a wire, the magnetic forces with which that wire is thereby, for the time, endowed, reside not in the wire at all, but in the space surrounding it. Every one knows that the space or "field" surrounding a magnet is full of magnetic "lines of force," and that these lines run

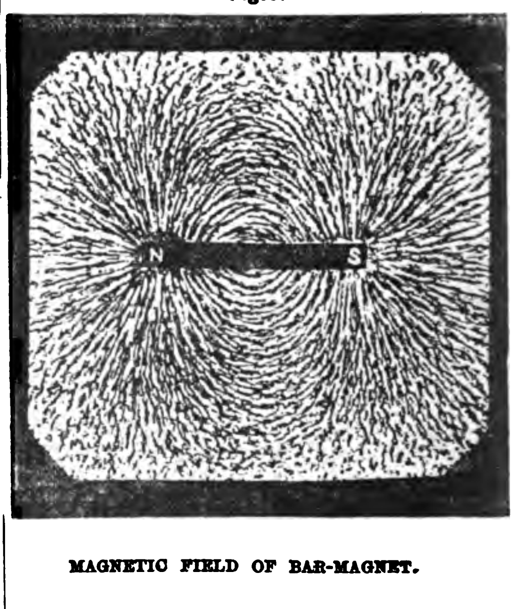
Fig. 5.



LINES OF FORCE OF BAR-MAGNET.

in tufts (Fig. 5) from the N-pointing pole to the S-pointing pole of the magnet, invisible until revealed by dusting iron filings into the field, whereby their presence is made known, though they

Fig. 6.



MAGNETIC FIELD OF BAR-MAGNET.

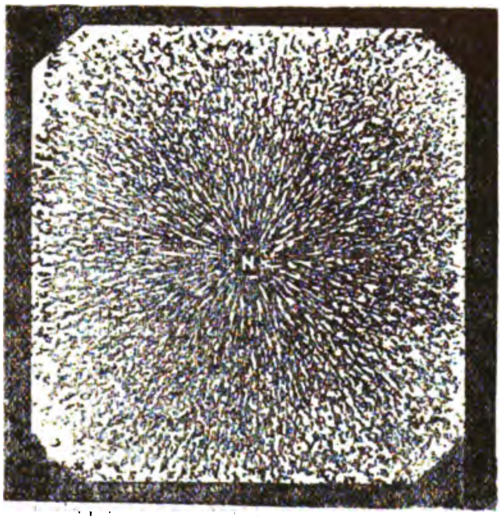
are always in reality there (Fig. 6). A view of the magnetic field at the pole of a bar-magnet, as seen end-on, would of course exhibit merely radial lines, as in Fig. 7.

Now, every electric current (so-called) is surrounded by a magnetic field, the

* A more usual rule for remembering the direction of the induced currents is the following adaptation from Ampère's well-known rule: Supposing a figure swimming in any conductor to turn so as to look along the (positive direction of the) lines of force. Then, if he and the conductor be moved towards his right hand, he will be swimming with the current induced by this motion.

lines of which can be similarly revealed. To observe them, a hole is bored through a card or a piece of glass, and the wire

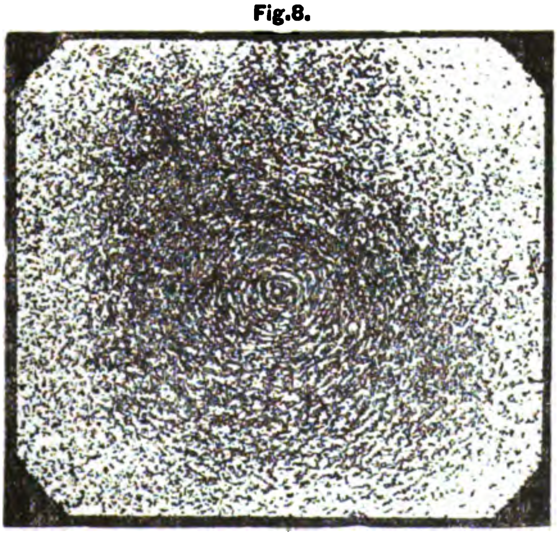
Fig.7.



MAGNETIC FIELD ROUND ONE POLE, END-ON.

which carries the current must be passed up through the hole. When iron filings are dusted into the field they assume the

Fig.8.



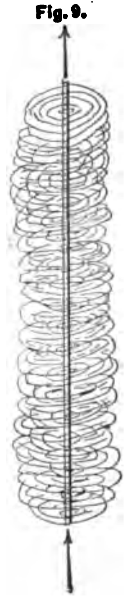
MAGNETIC FIELD SURROUNDING CURRENT. THE CONDUCTING WIRE SEEN END-ON.

form of concentric circles (Fig. 8), showing that the lines of force run completely round the wire, and do not stand out in tufts. In fact, every conducting wire is surrounded by a sort of magnetic whirl, like that shown in Fig. 9. A great part of the energy of the so-called electric current in the wire consists in these external magnetic whirls. To set them up requires an expenditure of energy; and to maintain them requires also a constant expenditure of energy. It is these mag-

netic whirls which act on magnets, and cause them to set as galvanometer needles do, at right angles to the conducting wire.

Now, Faraday's principle is nothing more nor less than this: That by moving a wire near a magnet, across a space in which there are magnetic lines, the motion of the wire, as it cuts across those magnetic lines, sets up magnetic whirls round the moving wire, or, in other language, generates a so-called current of electricity in that wire. Poking a magnet-pole into a loop or circuit of wire also necessarily generates a momentary current in the wire loop, because it momentarily sets up magnetic whirls. In Faraday's language, this action increases the number of magnetic lines of force intercepted by the circuit.

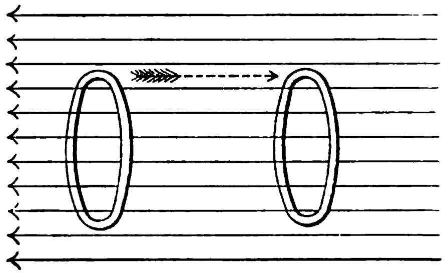
Fig. 9.



MAGNETIC WHIRL SURROUNDING WIRE CARRYING CURRENT.

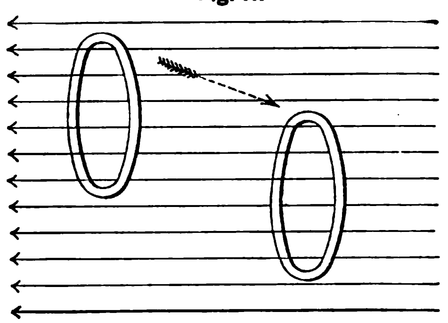
It is, however, necessary that the moving conductor should, in its motion, so cut the lines of force as to alter the number of lines of force that pass through the circuit of which the moving conductor forms part. If a conducting circuit —a wire ring or single coil, for example —be moved along in a uniform magnetic field, as indicated in Fig. 10, so that only the same lines of force pass through it, no current will be generated. Or, if again, as in Fig. 11, the coil be moved, by a motion of translation, to another part of the uniform field, as many lines of force will be left behind as are gained in advancing from its first to its second position, and there will be no current generated in the coil. If the coil be merely rotated on itself round a central axis, like the rim of a fly-wheel, it will not cut any more lines of force than before, and this motion will generate no current. But if, as in Fig. 12, the coil be tilted in its motion across the uniform field, or rotated round any axis in its own plane, then the number of lines of force that traverse it will be altered, and currents will be generated. These currents will flow round the ring-coil in the positive* sense (as viewed

Fig. 10.



CIRCUIT MOVED WITHOUT CUTTING LINES OF FORCE OF UNIFORM MAGNETIC FIELD.

Fig. 11.



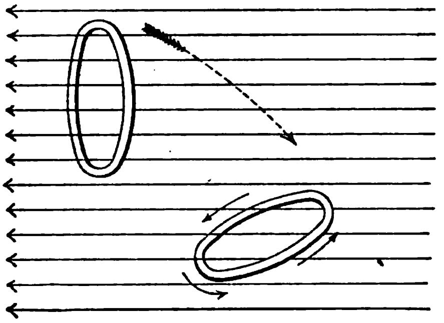
CIRCUIT MOVED WITHOUT CUTTING ANY MORE LINES OF FORCE.

* The positive sense of motion round a circle is that opposite to the sense in which the hands of a clock go round.

from the point toward which the lines of force run), if the effect of the movement is to diminish the number of lines of force that cross the coil; they will flow round in the opposite sense, if the effect of the movement is to increase the number of intercepted lines of force.

If the field of force be not a uniform one, then the effect of taking the coil, by a simple motion of translation, from a place where the lines of force are dense to a place where they are less dense, as from position 1 to position 2 in Fig. 13, will be to generate currents. Or, if the motion be to a place where the lines of force run in the reverse direction, the effect will be the same, but even more powerful.

Fig. 12.



CIRCUIT MOVED SO AS TO ALTER NUMBER OF LINES OF FORCE THROUGH IT.

We may now summarize the points under consideration, and some of their immediate consequences, in the following manner:

(1.) A part, at least, of the energy of an electric current exists in the form of magnetic whirls in the space surrounding the conductor.

(2.) Currents can be generated in conductors by setting up magnetic whirls round them.

(3.) We can set up magnetic whirls in conductors by moving magnets near them, or moving them near magnets.

(4.) To set up and maintain such magnetic whirls uses up a continuous expenditure of energy, or, in other words, consumes power.

(5.) To induce currents in a conductor, there must be relative motion between conductor and magnet of such a kind as to alter the number of lines of force embraced in the circuit.

(6.) Increase in the number of lines of force embraced by the circuit produces a current in the opposite sense to decrease.

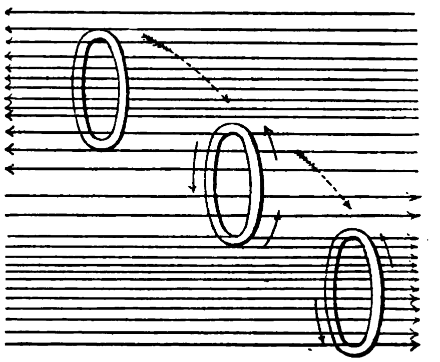
(7.) Approach induces an electromotive force in the opposite direction to that induced by recession.

(8.) The more powerful the magnet pole or magnetic field, the stronger will be the current generated (other things being equal).

(9.) The more rapid the motion, the stronger will be the currents.

(10.) The greater the length of the moving conductor thus employed in cutting lines of force (*i e.*, the longer the bars, or the more numerous the turns of the coil), the stronger will be the currents generated.

Fig. 13.



MOTION OF CIRCUIT IN NON-UNIFORM MAGNETIC FIELD.

(11.) The shorter the length of those parts of the conductor not so employed, the stronger will be the current.

(12.) Approach being a finite process, the method of approach and recession (of a coil towards and from a magnet-pole) must necessarily yield currents alternating in direction.

(13.) By using a suitable commutator, all the currents, direct or inverse, produced during recession or approach, can be turned into the same direction in the wire that goes to supply currents to the external circuits, thereby yielding an almost uniform current.

(14.) In a circuit where the flow of

currents is steady* it makes no difference what kind of magnets are used to procure the requisite magnetic field, whether permanent steel magnets or electro-magnets, self-excited or otherwise.

(15.) Hence the current of the generator may be itself utilized to excite the magnetism of the field-magnets, by being caused, wholly or partially, to flow round the field magnet coils.

A very large number of dynamo-electric machines have been constructed upon the foregoing principles. The variety is indeed so great, that classification is not altogether easy. Some have attempted to classify dynamos according to certain constructional points, such as whether the machine did or did not contain iron in its moving parts (which is a mere accident of manufacture, since almost all dynamos will work either with or without iron in their armatures, though not equally well); or whether the currents generated were direct and continuous, or alternating (which is in many cases a mere question of arrangement of parts of the commutator or collectors); or what was the form of the rotating armature (which is, again, a matter of choice in construction, rather than of fundamental principle). The classification which I shall adopt, is one which I have found more satisfactory and fundamental than any other. I distinguish three genera or main classes of dynamos.

Class I.—Dynamos in which there is rotation of a coil or coils in a uniform† field of force, such rotation being effected (as in the manner indicated in Fig. 12), round an axis in the plane of the coil, or one parallel to such an axis.

Examples.—Gramme, Siemens (Alteneck), Edison, Lontin, Bürgin, Fein, Schückert, Jürgensen (Thomson's Mousemill-dynamo), [Brush].

Class II.—Dynamos in which there is translation* of coils to different parts of a complex field of varying strength, or of opposite sign. Most, but by no means all, of the machines of this class furnish alternate currents.

Examples.—Pixii, Clarke, Niaudet, Wallace-Farmer, Wilde (alternate), Siemens (alternate), Hopkinson and Muirhead, Thomson (alternate), Gordon (alternate), Siemens-Alteneck (Disk Dynamo), Edison (Disk Dynamo), De Meritens.

Class III.—Dynamos having a conductor rotating so as to produce a continuous increase in the number of lines of force cut, by the device of sliding one part of the conductor on or round the magnet, or on some other part of the circuit.

Examples.—Faraday's Disk-machine, Siemens's ("Unipolar" Dynamo), Voice's Dynamo.

One machine, and one only, am I aware of which does not fall exactly within any of these classes,† and that is the extraordinary tentative dynamo of Edison, in which the coils are waved to and fro at the ends of a gigantic tuning-fork, instead of being rotated on a spindle.

Suppose, then, it were determined to construct a dynamo upon any one of these plans—say the first—a very slight acquaintance with Faraday's principle and its corollaries would suggest that, to obtain powerful electric currents, the machine must be constructed upon the following guiding lines:—

(*a.*) The field-magnets should be as strong as possible, and their poles as near together as possible.

(*b.*) The armature should have the greatest possible length of wire upon its coils.

* For currents that are not steady, there are other considerations to be taken into account, as will be shown hereafter.

† Or approximately uniform. A Gramme ring, or a Siemens drum armature, will work in a by no means uniform field, but is adapted to work in a field in which the lines of force run uniformly from one side to the other. But in such a field, a multipolar armature of many coils, such as that of Wilde, or such as is used in the Gramme alternate-current, or in the Siemens alternate-current machine, is useless and out of place. Indeed, the classification almost amounts to saying that in machines of Class I. there is one field of force, while in machines of Class II. there are many fields of force, or the whole field of force is complex.

* The motion by which the individual coils are carried round on such armatures as those of Niaudet, Wallace-Farmer, Siemens (alternate), &c., is, of course, not a pure translation. It may be regarded, however, as a combination of a motion of translation of the coil round the circumference of a circle, with a rotation of the coil round its own axis, which, as we have seen above, has *no* electrical effect. It is, of course, the translation of the coil to different parts of the field which is the effective motion.

† There are a few dynamos, including the Elphinstone Vincent, and the four pole Gülcher, which, though really belonging to the first class, are not named above, because they are, in reality, compound machines. The Gülcher, with its doubled field magnets and four collecting brushes, is really a double machine, though it has but one rotating ring. The same is the case with an octagonal pattern Gramme, which has four brushes. The Elphinstone Vincent machine, a remarkable one in many respects, is a triple machine, having six brushes; and may, indeed, be used as three machines, to feed three separate circuits.

(*c.*) The wire of the armature coils should be as thick as possible, so as to offer little resistance.

(*d.*) A very powerful steam-engine should be used to turn the armature, because,

(*e.*) The speed of rotation should be as great as possible.

Unfortunately, it is impossible to realize all these conditions at once, as they are incompatible with one another; and, moreover, there are a great many additional conditions to be observed in the construction of a successful dynamo. We will deal with the various matters in order, beginning with the speed of the machine.

Relation of Speed to Power.

Theory shows that, if the intensity of the magnetic field be constant, the electromotive force should be proportioned to the speed of the machine. Numerous experiments, by many different workers, have shown that this is true, within certain limits, for those machines in which the field magnets are independent of the main circuit, that is to say, for magneto and separately excited dynamos. It is not, however, quite exact unless the resistance of the circuit be increased proportionately to the speed, because the current in the coils itself re-acts on the magnetic field, and alters the distribution of the lines of force. The consequence of this reaction is that, firstly, the position of the "diameter of commutation" is altered; and, secondly, the effective number of lines of force is reduced. So that, with a constant resistance in circuit, the electromotive force, and therefore the current, are slightly less at high speeds than the proportion of the velocities would lead one to expect. Since the product of current into electromotive force gives a number proportional to the electric work of the machine, it follows that, for "independently excited" machines, the electric work done in given time is nearly proportional to the square of the speed, and the work drawn from the steam-engine will be similarly proportional to the square of the speed.

In self-exciting machines, whether "series" or "shunt" in their arrangements, a wholly different law obtains. If the iron of the field-magnets be not magnetized near to saturation, then, since the increase of current consequent on increase of speed produces a nearly proportional increase in the strength of the magnetic field, this increase will re-act on the electromotive force, and cause it to be proportional more nearly to the square of the velocity, which again will cause the current to increase in like proportion. But since the magnetization of the iron is, even when far from saturation point, not exactly proportional to the magnetizing force, but something less, it is in practice found that the electric work of the machine is not proportional to the fourth power of the speed, is not even proportional to the third power of the speed, but to something slightly less than the latter.

As mechanical considerations forbid too high a velocity in the moving parts, it is clear that, if there be a limiting speed at which it is safe to run any given armature, the greatest amount of work will be done at that speed by using the most powerful magnets possible, namely, electro-magnets rather than magnets of steel.

Marcel Deprez has shown* that it is convenient to study the action of dynamos, by constructing a curve which he calls a "characteristic," showing, for any given speed, the relation between electromotive force and strength of current generated in a circuit of given resistance. Amongst other conclusions, M. Deprez has found that for every dynamo there is a certain speed which we may call the "critical speed," at which, no matter what the current is which circulates in the coils of the field magnets, the electromotive force is proportional to the strength of that current; and he bases upon the discovery of this critical speed two methods of utmost importance in practice, for obtaining, automatically, either a constant electromotive force or a constant current at will, in a circuit in which the resistances are varied to any degree.

In all these combinations, however, everything depends upon the condition that the driving speed shall be uniform. For all the best kind of electric work, a gas-engine is wholly out of the question as a source of power, because of its extreme inequalities in speed. Even with

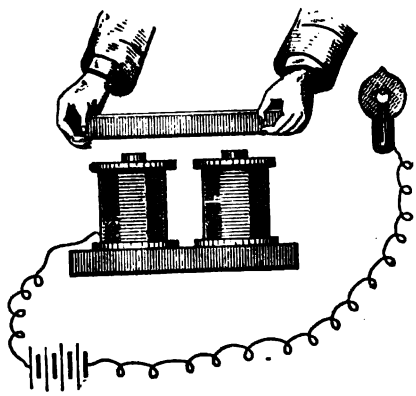
* *Comptes Rendus*, 1881, and elsewhere. For an excellent summary of the work of M. Deprez, see *La Lumière Electrique*, February 18, 1882.

the best steam-engines, a specially sensitive valve is required; and probably such valves will, in the future, be operated electrically by self-acting electro-magnet gearing. In any case, where the driving is at all liable to be uneven, the obvious and simple precaution should be taken of placing a heavy fly-wheel on the axis of the dynamo. It is, indeed, singular that this is not more generally done.

Field-magnets

The coils of the field-magnets of a dynamo cannot be constructed of no resistance. They, therefore, always waste some of the energy of the currents in heat. It has, therefore, been argued that it cannot be economical to use electro-magnets in comparison with permanent magnets of steel, which have only to be magnetized once for all. Nevertheless, there are certain considerations which tell in favor of electro-magnets. For equal power, their prime cost is less than that of steel magnets, which, moreover, are not permanent, but require remagnetizing at intervals. Moreover, as we have seen, from the fact that there is a limiting velocity at which it is safe to run a machine, it is important, in order not to have machines of needlessly great size, to use the most powerful field-magnets possible. But if we do not get our magnetism for nothing, and find it more convenient to spend part of our current upon the electro-magnets, economy dictates that we should so construct them that their magnetism may cost us as little as possible. To magnetize a piece of iron requires the expenditure of energy; but when once it is magnetized, it requires no further expenditure of energy (save the slight loss by heating in the coils, which may be reduced by making the resistance of the coils as little as possible) to keep it so magnetized, provided the magnet is doing no work. Even if it be doing no work, if the current flowing round it be not steady, there will be loss. If it do work, say in attracting a piece of iron to it, then there is an immediate and corresponding call upon the strength of the current in the coils, to provide the needful energy. This point may be illustrated by the following experiment:—Let a current from a steady source (see Fig. 14) pass through an incandescent lamp, and also through an electro-magnet, whose cores it magnetizes. When once established, the current is perfectly steady, and none of its energy is wasted on the magnet (save the negligible trifle due to the resistance of the coils). But if now the magnet is allowed to do work in attracting an iron bar towards itself, the light of the lamp is seen momentarily to fade. When the iron bar is snatched away, the light exhibits a momentary increase; in each case resuming its original intensity when the motion ceases. Now, in a dynamo where, in many cases there are revolving parts containing iron, it is

Fig. 14.



REACTION OF MOVING MASS OF IRON ON AN ELECTRO-MAGNET.

of importance that the approach of a recession of the iron parts should not produce such reactions as these in the magnetism of the magnet. Large, slow-acting field-magnets are therefore advisable. The following points embody the conditions for attaining the end desired.

(*a*.) The body of the field-magnets should be solid. Even in the iron itself currents are induced, and circulate round and round whenever the strength of the magnetism is altered. These self-induced currents tend to retard all changes in the degree of magnetization. They are stronger in proportion to the square of the diameter of the magnet, if cylindrical or to its area of cross-section. A thick magnet will, therefore, be a slow-acting one, and will steady the current induced in its field.

(*b*.) Use magnets having in them plenty of iron. It is important to have a suffi-

cient mass, that saturation may not be too soon attained.

(c.) Use the softest possible iron for field-magnets, not because soft iron magnetizes and demagnetizes quicker than other iron (that is here no advantage); but because soft iron has a higher magnetic susceptibility than other iron—is not so soon saturated.

(d.) Use long magnets. Again, the use of long magnets is to steady the magnetism, and therefore to steady the current. A long magnet takes a longer time than a short magnet to magnetize and demagnetize. It costs more than a short magnet, it is true, and requires more copper wire in the exterior coil; but the copper wire may be made thicker in proportion, and will offer less resistance. The magnetism so obtained should be utilized as directly as possible, therefore

(e.) Place the field-magnets or their pole-pieces as close to the rotating armature as is compatible with safety in running.

(f.) Avoid edges and corners on the magnets and pole-pieces if you want a uniform field. The laws of distribution of the magnetic lines of force round a pole are strikingly akin to those of the distribution of electrification over a conductor. We avoid edges and points in the latter case, and ought to do in the former. If the field-magnets or their pole-pieces have sharp edges, the field cannot be uniform, and some of the lines of force will run uselessly through the space outside the armature instead of going through it. Theoretically, the very best form to give externally to a magnet is that of the curves of the magnetic lines of force.

(g.) Reinforce the magnetic field by placing iron, or better still, electro-magnets, within the rotating armature. In many cases this is done by giving the armature coils iron cores which rotate with them; in other cases, the iron cores or internal masses are stationary. In the former case there is loss by heating; in the latter, there are structural difficulties to be overcome. Siemens has employed a stationary mass within his rotating drum-armature. Internal electro-magnets serving the function of concentrating the magnetism of the field, and of so providing a continuous magnetic circuit, have been used by Lord Elphinstone and Mr. Vincent. A similar device obtains in Sir. W. Thomson's "mouse-mill" dynamo, and in Jürgensen's dynamo.

(h.) In cases where a uniform magnetic-field is not desired, but where, as in dynamos of the second class, the field must have varying intensity at different points, it may be advisable specially to use field-magnets with edges or points, so as to concentrate the field at certain regions.

Pole-pieces.

(i.) The pole-pieces should be heavy, with plenty of iron in them, for reasons similar to those urged above.

(j.) The pole-pieces should be of shapes really adapted to their functions. If intended to form a single approximately uniform field, they should not extend too far on each side. The distribution of the electromotive force in the various sections of the coils on the armature depends very greatly on the shape of the pole-pieces.

(k.) Pole-pieces should be constructed so as to avoid, if possible, the generation in them of useless Foucault currents. The only way of diminishing loss from this source is to construct them of laminæ, built up so that the mass of iron is divided by planes in a direction perpendicular to the direction of the currents, or of the electromotive forces tending to start such currents.

(l.) If the bed-plates of dynamos are of cast-iron, care should be taken that these bed-plates do not short-circuit the magnetic lines of force from pole to pole of the field-magnets. Masses of brass, zinc, or other non-magnetic metal may be interposed; but are at best a poor resource. In a well-designed dynamo, there should be no need of such devices.

Field-magnet Coils.

(m.) In order to be of the greatest possible service, the coils of the field-magnets should be wound on most thickly at the middle of the magnet, not distributed uniformly along its length, nor yet crowded about its poles. The reason for this is two-fold. Inspection of Fig. 6, will show that many of the lines of force of a magnet "leak out" from the sides of the magnet before reaching its poles, where they should all emerge if the mass

of the magnet were perfectly equally magnetized throughout its whole length. Internally, the magnetization of the magnet is greatest at its center. At, or near the center, therefore, place the magnetizing coils, that the lines of force due to them may run through as much iron as possible. The second reason for not placing the coils at the end is this: any external influence which may disturb the magnetism of a magnet, or affect the distribution of its lines of force, affects the lines of force in the neighborhood of the pole far more than those in any other region. It is for this reason that in Bell's telephones, where it is desired to make a magnet most sensitive to variations in intensity, the coils are fixed on at the pole. In the field-magnet of a dynamo, on the contrary, where the magnet is wanted to be as steady and constant as possible in its magnetic power, the coils should not be placed on the poles.

(*n.*) The proper resistances to give to the field-magnet coils of dynamos have been calculated by Sir Wm. Thomson,* who has given the following results:

For "Series Dynamo," make the resistance of the field magnets a little less than that of the armature. Both of them should be small, compared with the resistance of the external circuit. The ratio of the waste by heating in the machine, to the total electric work of the machine, will be:

$$\frac{\text{waste:}}{\text{total work}}=\frac{R_M+R_A}{R_M+R_A+R_X}$$

and
$$\frac{\text{useful work}}{\text{total work}}=\frac{R_X}{R_M+R_A+R_X}$$

where

R_M is the resistance of the magnets,
R_A " " " armature,
R_X " " " external circuit.

For a "Shunt Dynamo," the rule is different. The best proportions are when such that

$$R_X=\sqrt{R_M R_A},$$

or that

$$R_M=\frac{R_X^2}{R_A};$$

also the ratio of useful work is:

* British Association Report, 1881, p. 528.

$$\frac{\text{useful work}}{\text{total work}}=\frac{1}{1+2\sqrt{\frac{R_A}{R_M}}}.$$

An example of the latter may be of advantage. Suppose it was wished that the waste should not be more than 10 per cent. of the useful work, the ratio of the formula must equal $\frac{10}{11}$, or $1\div1\frac{1}{10}$. Hence $\sqrt{\frac{R_A}{R_M}}$ must equal $\frac{1}{20}$; or R_M the resistance of the field magnets must be 400 times R_A that of the armature.

Armature Cores.

(*o.*) Theory dictates that if iron is employed in armatures, it must be slit or laminated, so as to prevent the generation of Foucault currents. Such iron cores should be structurally divided in planes normal to the circuits round which electromotive force is induced; or should be divided in planes parallel to the lines of force and to the direction of the motion. Cores built up of varnished iron wire, or of thin disks of sheet-iron separated by varnish, asbestos paper, or mica, partially realize the required condition.

(*p.*) Armature cores should be so arranged that the direction of polarity of their magnetization is never abruptly reversed during their rotation. If this precaution is neglected, the cores will be heated.

Armature Coils.

(*q.*) All needless resistance should be avoided in armature coils, as hurtful to the efficiency of the machine. The wires should therefore be as short and as thick as is consistent with obtaining the requisite electromotive force, without requiring an undue speed of driving.

(*r.*) The wire should be of the very best electric conductivity. The conductivity of good copper is so nearly equal to that of silver (over 96 per cent.), that it is not worth while to use silver wires in the armature coils of dynamos.

(*s.*) In cases where rods or strips of copper are used instead of mere wires, care must be taken to avoid Foucault currents by laminating such conductors, or slitting them in planes parallel to the electromotive force, that is to say, in planes per-

pendicular to the lines of force, and to the direction of the rotation.*

(*t.*) In dynamos of the first class, when used to generate currents in one direction, since the currents generated in the coils are doing half their motion inverse to those generated during the other half of their motion, a commutator or a collector of some kind must be used. In any single coil without a commutator, the alternate currents would be generated in successive revolutions, if the coil were destitute of self-induction currents, whose variations may be graphically expressed by a recurring sinusoidal curve, such as Fig. 15. But if by the addition of a sim-

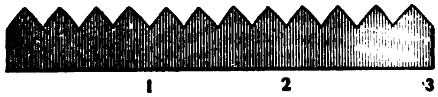
Fig. 15.

ple split-tube commutator the alternate halves of these currents are reversed, so as to rectify their direction through the rest of the circuit, the resultant currents will not be continuous, but will be of one sign only, as shown in Fig. 16, there being two currents generated during each revolution of the coil. If two coils are used, at right angles to each others' planes, so that one comes into the position of best action. while the other is in the position of least action (one being normal to the lines of force, when the other is parallel to them), and their actions be superposed, the result will be, as shown

Fig. 16.

in Fig. 17, to give a current which is continuous, but not steady, having four

Fig. 17.



slight undulations per revolution. If any number of separate coils are used, and their effects, occurring at regular intervals, be superposed, a similar curve will be obtained, but with summits proportionately more numerous and less elevated. When the number of coils used is very great, and the overlappings of the curves still more complete, the row of summits will form practically a straight line, or the whole current will be practically constant.

(*u.*) The rotating armature coils ought therefore to be divided into a large number of sections, each coming in regular succession into the position of best action.

(*v.*) If these sections, or coils, are independent of each other, each coil, or diametral pair of coils, must have its own commutator. If they are not independent, but are wound on in continuous connection all round the armature, a collector is needed, consisting of parallel metallic bars as numerous as the sections each bar communicating with the end of one section and the beginning of the next.

(*w.*) In any case, the connections of such sections and of the commutators, or collectors, should be symmetrical round the axis; for if not symmetrical, the induction will be unequal in the parts that successively occupy the same positions with respect to the field-magnets, giving rise to inequalities in the electromotive force, sparking at the commutator, or collector, and other irregularities.

(*x.*) In the case where the coils are working in series, it is advantageous to arrange the commutator to cut out the coil that is in the position of least action, as the circuit is thereby relieved of the resistance of an idle coil. But no such coil should be short-circuited to cut it out. In the case where the coils are working in parallel, cutting out an idle coil increases the resistance, but may be advisable to prevent heating from waste currents traversing it from the active coils.

(*y.*) In the case of pole-armatures, the coils should be wound on the poles rather than on the middles of the projecting cores; since the variations in the induced magnetism are most effective at or near the poles. (See § *m.*)

(*z.*) Since it is impossible to reduce the resistance of the armature coils to zero, it is impossible to prevent heat be-

* It will be observed that the rule for eliminating Foucault currents is different, in the three cases, for magnets and their pole-pieces, for moving iron armature cores, and for moving conductors in the armature.

ing developed in those coils during their rotation; hence it is advisable that the coils should be wound with air spaces in some way between them, that they may be cooled by ventilation.

(*aa.*) The insulation of the armature coils should be ensured with particular care, and should be carried out as far as possible with mica and asbestos, or other materials not liable to be melted, if the armature coils become heated.

Commutators, Collectors, and Brushes.

(*ab.*) Commutators and collectors being liable to be heated through imperfect contract, and liable to be corroded by sparking, should be made of very substantial pieces of copper.

(*ac.*) In the case of a collector made of parallel bars of copper, ranged upon the periphery of a cylinder, the separate bars should be capable of being removable singly, to admit of repairs and examination.

(*ad.*) The brushes should touch the commutator or the collector at the two points, the potentials of which are respectively the highest and the lowest of all the circumference. In a properly and symmetrically built dynamo, these points will be at opposite ends of a diameter.

(*ae.*) In consequence of the armature itself, when traversed by the currents, acting as a magnet, the magnetic lines of force of the field will not run straight across, from pole to pole, of the field magnets, but will take, on the whole, an angular position, being twisted a considerable number of degrees in the direction of the rotation. Hence the diameter of commutation (which is at right-angles to the resultant lines of force in machines of the Siemens and Gramme type, and parallel to the resultant lines of force in machines of the Brush type), will be shifted forward. In other words, the brushes will have a certain angular lead. The amount of this lead depends upon the relation between the intensity of the magnetic field and the strength of the current in the armature. This relation varies in the four different types of field magnets. In the series dynamo, where the one depends directly on the other, the angle of lead is nearly constant, whatever the external resistance. In other forms of dynamo, the lead will not be the same, because the variations of resistance in the external circuit do not produce a proportionate variation between the two variables which determine the angle of lead.

(*af.*) Hence in all dynamos it is advisable to have an adjustment, enabling the brushes to be rotated round the commutator or collector, to the position of the diameter of commutation for the time being. Otherwise there will be sparking at the brushes, and in part of the coils at least the current will be wasting itself by running against an opposing electromotive force.

(*ag.*) The arrangements of the collector, or commutator, should be such that, as the brushes slip from one part to the next, no coil or section in which there is an electromotive force should be short circuited, otherwise work will be lost in heating that coil. For this reason, it is well so to arrange the pole places that the several sections or coils on either side of the neutral position should differ but very slightly in potential from one another.

(*ah.*) The number of contact points between the brushes and the collector, or commutator, should be as numerous as possible, for by increasing the number of contacts, the energy wasted in sparks will be diminished inversely as the square of that number. The brushes might, with advantage, be laminated, or made of parallel loose strips of copper, each bearing edgeways on the collector.

Relation of Size to Efficiency.

The efficiency of a dynamo-electric machine is the ratio of the useful electrical work done by the machine to the total mechanical work applied in driving it. Every circumstance which contributes to wasting the energy of the current reduces the efficiency of the machine. In the preceding paragraphs it has been shown what the chief electric sources of waste are, and how they may be avoided. The precautions needful to obviate Foucault currents, to avoid reversals of magnetization, to get rid of needless resistance, to obviate opposing electro motive forces, have been detailed. Mechanical friction of the moving parts can be minimized also by due mechanical arrangements. But one thing cannot be entirely obviated, because even the best conductors employed have a certain resistance. We cannot prevent the heating of the conducting

coils; and the more powerful the current generated by the machine, the more important does this source of waste become. There is but one way to reduce this, and that is by increasing the size of the machines. For some three years or so I have been the advocate of large dynamo machines, not because I have any admiration for mere bigness, but because, as in steam engines so in dynamos, the larger machines may be made more efficient than the small, in proportion to their cost. In discussing the relation of size to efficiency, I shall assume, for the sake of argument, that the size of any machine can be increased n times in every dimension, and that though the dimensions are increased, the velocity of rotation remains the same, and that the intensity of the magnetic field, per square centimeter, remains also constant. If the linear dimensions be n times as great in the larger machine as in the smaller, the area it stands on will be increased n^2 times, and its volume and weight n^3 times. The cost will be less than n^3 times but greater than n times. If the same increase of dimensions in the coils be observed (the number of layers and of turns remaining the same as before), there will be in the armature coils a length n times as great, and the area of cross-section of the wire will be n^2 times as great as before. The resistance of these coils will, therefore, be but $\frac{1}{n}$ part of the original resistance of the smaller machine. If the field-magnet coils are increased similarly they will offer only $\frac{1}{n}$ of the resistance of those of the smaller machine. Moreover, seeing that while the speed of the machine is the same, the area cut through by the rotating coils is increased n^2 times, these coils will in the same time cut n^2 times as many lines of force, or the electromotive force will be increased n^2 times. Supposing the whole of the circuit to be similarly magnified, its resistance will also be but $\frac{1}{n}$ of the previous value.

If the machine is a "series"-wound dynamo, an electromotive force, n^2, working through $\frac{1}{n}$ resistance will give a current n^3 times as great as before. Such a current will, as a matter of fact, much more than suffice to bring the magnetic field to the required strength, viz., n^2 times the area of surface magnetized to the same average intensity per square centimeter, as stipulated; for the mass of iron being n^3 times as great, it need not be so much saturated as before to give the required field. Here an economy may be effected, therefore, by further reducing the number of coils, and therefore the wasteful resistance of the field-magnet coils, in the proportion of n^3 to n^2, or to $\frac{1}{n}$ of its already diminished value. Even if this were not done, by the formula given above for the electrical efficiency of a "series" dynamo, the waste, when working through a constant external resistance, will be n-fold less than with the smaller machine. Now, if the current be increased n^3 times, and the electromotive force n^2 times, the total electric work which is the product of these will be n^5 times greater than in the small machine, and it will consume n^5 times as much power to drive it.* It is clearly an important economy, if a machine costing less than n^3 times as much, will do n^5 times as much work (to say nothing of the increased ratio of efficiency). A machine doubled in all its linear dimensions will not cost eight times as much, and will be electrically thirty-two times as powerful a machine.

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* This calculation agrees with the result deduced on entirely different principles by M. Marcel Deprez. M. Deprez considers the mutual reaction, dF, between two elements, ds and ds', of a system of conductors which, by Ampère's principle, is

$$dF = C^2 \frac{ds\, ds'}{r^2} f(a)$$

where C is the current, r the distance of the elements apart, and $f(a)$ a certain function of the machine, independent of its size. Writing a for the area, we have

$$dF = \frac{C^2}{a^2} \cdot \frac{a ds \,.\, a ds'}{r^2} \cdot f(a)$$
$$= \frac{C^2}{a^2} \cdot \frac{dv \,.\, dv'}{r^2} \cdot f(a)$$

which, if the linear dimensions be increased n times, becomes

$$dF =' \frac{C^2}{a^2} \cdot \frac{n^3 dv \,.\, n^3 dv'}{n^2 r^2} \cdot f(a)$$
$$= n^4 dF$$

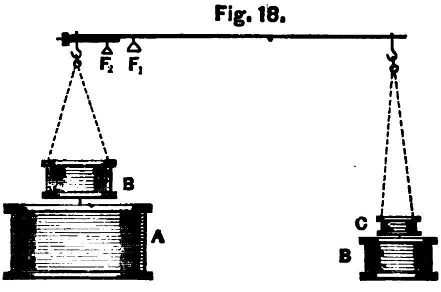
whence, since this is true for all elements of the circuits, $\frac{F'}{F} = n^4$, which is Deprez's so-called "law of similars," which asserts that for similar machines the "statical effort" increase as the fourth power of the linear dimensions. But work W=F × distance, and, in the similar machine whose dimensions are increased n times, the available distance through which the force F' can act also n times grealer.

Hence $\frac{W'}{W} n^5$, as above.

Suppose the machine to be "shunt" wound, then to produce the field of force of n^2 times as many square centimeters area, will require (if the electromotive force be n^3 times as great) that the absolute strength of the current remain the same as before in the field-magnet coils. This can be done by using the same sized wire as before, and increasing its length n^2 times, to allow for n times as many turns, of n times as great a diameter each, in the same number of layers of coils as before. In this case the work done in the shunt being equal to the product of the n^2-fold electromotive force into the unaltered current will be only n^2 times as great, while the whole work of the machine is augmented n^5 times. Now, if while augmenting the total work n^5 times, we have increased the waste work not to n^5 times but only n^2 times, it is clear that the ratio of waste to the total effect is diminished n^3-fold. There is therefore, every reason to construct large machines, from the advantage of economy, both in relative prime cost and relative efficiency.

Being desirous of testing the correctness of the deduction that the working capacity of a machine of n-fold linear dimensions is n^4 times as great, I constructed a little instrument, of which a drawing is given in Fig. 18. In this

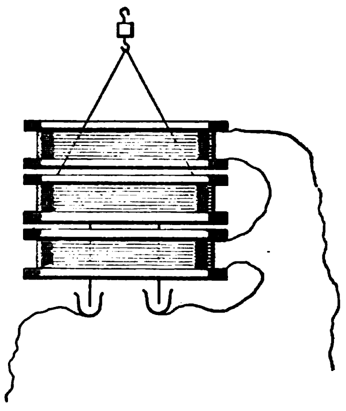
Fig. 18.



S. P. THOMPSON'S EXPERIMENTAL BALANCE.

instrument there are two pairs of coils, that on the left being, in every way, the counterpart of that on the right, but of double linear dimensions. When all four coils were traversed by the same current, the point of equilibrium was $\frac{1}{17}$ of the length of the beam from the extremity; or the attraction of the larger system was sixteen times that of the smaller. Now, it is clear that the larger force can be exerted through double the distance, or that the work-power is 32-fold, and 32 is 2^5, as theory requires. After I had constructed my apparatus, I learned that M. Marcel Deprez had made a very similar arrangement, but without the beam, to prove that the statical forces of similar machines are (see foot-note *ante*) proportional to the fourth power of the linear dimensions. M. Deprez's instrument consisted of a modified Mascart's electro-dynamometer, having a coil suspended from a balance, and acted upon by two others, placed axially above and below it (see Fig. 19).

Fig. 19.



MARCEL DEPREZ'S ELECTRO-DYNAMIC BALANCE.

The force was measured by directly balancing it against weights. Two such arrangements were made, one double the size of the other, and when equal currents were sent through them, M. Deprez found the forces to be as 5.600 to 0.355 kilos., or almost exactly 16 to 1.

Methods of Exciting the Field Magnetism.

It only remains to develop certain theoretical considerations respecting the method of exciting the magnetism of the field, in which the armatures are to revolve. The four main methods have already been alluded to at the outset of this lecture; but nothing has been said about the advantages or disadvantages of the four systems.

Magneto-Dynamos.—The magnet-dynamos (Fig. 4.) have the advantage in theory at least, that their electromotive force is very nearly exactly proportional to the velocity of rotation; though, of course, the variable difference of potential between the terminals of the circuit will depend on the relation of the resistance of the external circuit to the internal resistance of the armature coils. They possess the disadvantage that, since steel cannot be permanently magnetized to the same degree as that which soft iron can temporarily attain, they are not so powerful as other dynamos of equal size.

Separately-Excited Dynamos. — The separately excited dynamo (Fig. 3,) has the same advantage as the magneto-machine, in electromotive force being independent of its accidental changes of resistance in the working circuit, but is more powerful. It has, moreover, the further advantage that the strength of the field is under control. For by varying either the electromotive force or the resistance in the exciting circuit, the strength of the magnetic field is varied at will. It has the disadvantage of requiring a separate exciting machine.

Series Dynamos.—The ordinary, or series dymano (Fig. 1), is usually a cheaper machine, for equal power, than any of the other forms, as its coils are simpler to make than those of a shunt machine, and it wants no auxiliary exciter. It has the disadvantages of not starting action until a certain speed has been attained, or unless the resistance of the circuit is below a certain minimum. It is also liable to become reversed in polarity, a serious disadvantage when this machine is applied for electro-plating or for charging accumulators.

From its arrangements, it is clear that any increase of the resistance in the circuit lessens its power by diminishing the strength of its magnetic field. Hence it is better adapted for use with lamps arranged in parallel arc than for lamps arranged in series. An additional lamp switched in, in series, adds to the resistance of the circuit, and diminishes the power of the machine to supply current. While on the other hand, an additional lamp in parallel reduces the total resistance offered by the network of the circuit, and adds to the power of the machine to provide the needed current. It is easy to regulate the currents given by a series dynamo, by introducing a shunt of variable resistance across the field-magnet, thus altering the magnetizing influence of the current.

Shunt Dynamos.—The shunt dynamo (Fig. 2,) has several advantages over forms. It is less liable to reverse its polarity than the series dynamo, and it is commonly considered as providing the magnetizing power to the magnets with less waste of current. Moreover, for a set of lamps in series, its power to supply the needful current increases with the demands of the circuit, since any added resistance sends additional current round the shunt in which the field-magnets are placed, and so makes the magnetic field more intense. On the other hand, there is a greater sensitiveness to inequalities of driving in consequence of the great self-induction in the shunt. As previously pointed out, when there are sudden changes in the electromotive force acting in a complex circuit, the momentary currents thus set up do not distribute themselves in the various parts of the circuit in the simple inverse ratio of the resistances, for their distribution depends also, and in some cases chiefly, upon the self-induction in the various parts. It is more difficult to set up a sudden current in a circuit whose self-induction is great (or which, for example, consists of many turns wound closely together, so that they exercise great inductive action on each other, especially if they be wound about an iron core) than in one in which the self-induction is small. We cannot here follow further the mathematical law of the action of self-induction on momentary changes of electromotive force. But the application to the shunt-wound dynamo is too important to be passed over.

Any of these systems may be applied either in direct current or in alternate-current machines. Each of these four systems of exciting the field magnetism has its own merits for special cases, but none of them is perfect. Not one of these methods will insure that, with a uniform speed of driving, either the electromotive force or the current shall be constant, however the resistances of the circuit are altered.

But, though theory tells us that none of these systems is perfect, theory does not

leave us without a guide. Thanks to the genius of M. Marcel Deprez, we have been taught how to combine these methods so as to secure in practice a machine which shall, when driven at a constant speed, give either a constant electromotive force or a constant resistance. A consideration of the various methods of combining the four systems of exciting the field-magnetism is reserved for the lecture on the dynamo in practice.

Meantime, it may safely be said that there is no such thing yet as a best dynamo. As with the different kinds of voltaic batteries, some of which are used for telegraph work, others for electric bells, and others for blasting, so is it also with dynamos. One form is best for one purpose and another for another. One gives steadier currents, another is less liable to heat, a third is more compact, a fourth is cheaper, a fifth is less likely to reverse its currents, a sixth gives a greater volume of current, while a seventh evokes a higher electromotive force. Indeed, in the present transitional state of our knowledge with respect to dynamo-electric machinery, it is safe to assert that, for a long time to come, there will be no finality attained to. As with the steam-engine, however, so with the dynamo-machine, there will probably be a constant and progressive evolution, finally settling down upon two or three typical forms, which will survive the innumerable comparatively crude machines which, as yet, have taken shape and come into active service.

ELECTRIC LIGHTING IN MILLS.

Abstract of a Report by C. J. H. WOODBURY, Inspector of the Boston Manufacturers' Mutual Fire Insurance Company.

From Transactions of the New England Cotton Manufacturers' Association.

Expense.

The cost of maintenance of a system of lighting bears little relation to its intrinsic worth. The item of cost of lighting is a small fraction of the whole operating expense; and what is desired is to light a mill so well that there will be no difference in the character of day and night work, either in quantity or quality. Any expenditure beyond that is unwarrantable.

The question of the cost of lighting by electricity is subject to many legitimate variations, of which the question of power is most variable. In a steam-mill, where the dynamo is driven by the same engine that runs the mill, it is generally charged with only its share of fuel; but not with any other expense of power, wherever it does not introduce any new expenditures in the way of plant, repairs, or labor in the engine-room.

In following this plan for the sake of uniformity, with other estimates of the cost of electric light, I do not wish to be considered as endorsing this method of estimating. There is no reason why the electric apparatus should be supplied with power free of cost for transmission, interest, and supervision, which does not apply with equal force to every other machine in a mill, exempting it from these necessary items of cost. This paper refers only to the use of the electric light in mills, and not for circuit lighting, where the prime cost is much greater than in mills.

Some mills have departments which are only run by daylight, where work is thrown off at sundown, and so compensates for the power required by the dynamo. For example, in one mill using electric lights, the power used in the nepping-room is slightly more than is required for the dynamo; so, when the machinery in that room is stopped, the dynamo can be started without bringing any extra load on the engine. Most factories being driven by water power, with supplementary steam power during low water in the summer months, the electric lights would be required during the shorter days of the year at a time when there is usually an abundance of water; and the extra power can be used by the dynamo by the use of more water, without requiring any additional expense.

It is difficult to make a just comparison between various methods of illumi-

nation, because a change of light is always made an excuse for more light.

The majority of mills are lighted with gas made by the destructive distillation of petroleum, and of about eighty-candle power, which is generally reduced to sixty-candle power by mixing air with it, and burned through one-foot (nominal) burners, which consume about one and a quarter feet per hour, at the pressure generally used.

The annual cost of oil-gas per burner is from seventy-five cents to one dollar. In all these estimates, interest at six per cent. forms one item in cost. One large corporation, with exceptional privileges, makes its coal-gas at an annual cost of sixty-nine cents per burner. Another corporation, inland, makes its coal-gas at $1.25 per thousand cubic feet, at an annual cost of $1.79 per burner, each burner consuming 1,433 cubic feet annually.

Of two large mills in the same city, manufacturing similar goods, the more modern one makes oil-gas at an annual cost of seventy-nine cents per burner, while the older one buys coal-gas at $2.65 per burner.

Sometimes, when the gas-making apparatus is not managed with skill, the goods are damaged from soot which settles on them.

The following shows the cost of lighting a woolen mill with high-test kerosene oil, including wicks, chimneys, and matches:

Year	Lamps used.	Consumption of oil.		Cost.	
		Gallons Total.	Gallons per lamp	Total.	Per lamp.
1877.	125	167	1.33	$49.39	$0:40
1878.	140	387	2 75	84.63	.61
1879.	140	437	3.12	75.12	.54
1880.	140	408	2.94	89.34	.64
1881.	190	473	2.50	99.19	·52

The longer time light is required, the less the average cost, because, with the addition of operating expenses, the interest on plant, being a fixed amount, becomes a smaller proportion of the whole cost. In electric lighting, the cost of plant is so much that interest is an important item; and when a mill is run nights the relative cost of electric lighting is materially diminished. A white cotton mill, running sixty hours a week, generally uses light three hundred to three hundred and fifty hours a year; where they run sixty-six hours a week, lights are required four hundred to four hundred and fifty hours a year.

A dark mill requires about twice the number of lights that is sufficient in a white mill, and uses light about a hundred hours a year more than a white mill.

An arc light, as generally used in mills, requires about one-horse power. Mr. James Renfrew, Jr., at Adams, Mass., has found, by test, that the forty-light Brush dynamos in his mills each require 36.6-horse power. The lights were running in a satisfactory manner, but no photometric tests were made.

The cost of arc lights in several steam-mills, running four hundred hours per year, is 6½ cents per hour; of which one cent and a half is for carbons, and five cents for attendance, coal, depreciation, and interest. When a mill runs nights, the hourly cost is diminished.

The ratio of substituting electric lights for gas is quite variable, being one arc light to from ten to twenty gas-burners. In one mill lighted by kerosene the ratio was one arc lamp to eight kerosene lamps.

In a colored mill one arc light will light the looms on seven hundred to fourteen hundred square feet of floor; but in a white mill the same light will be sufficient for looms on a thousand to two thousand square feetof floor.

The reflected light from white walls and ceilings adds very materially to the diffusion. A card-room forty-eight by a hundred feet, containing sixty-four cards, was satisfactorily lighted by one arc light. The end of the room was extended about forty feet; and the light was not satisfactory toward that end of the room, where the light was formerly ample, because the removal of the end wall took away what served as a reflector.

It is advisable to place the conducting wires of lighting systems so that the different circuits will light similarly shadowed portions of the mill; one circuit reaching the darker places, another lighter parts, so that each of the various dynamo-machines can be used at their full capacity, as the total light in the mill is being increased or diminished.

It is convenient to compare the cost

of electric lighting with the expense of gas in the same place; although it must be remembered that gas does not furnish as much or as good light, and is therefore not so valuable where quantity or quality of light is of importance.

In a weave-room, on very fine work, twenty-four arc lights replaced 292 six-foot burners, which consume (292×6) 1,752 feet per hour, so one arc light represents the consumption of $(1,752 \div 24)$ seventy-three feet of gas per hour. A careful estimate shows these arc lights to be costing 6½ cents an hour; so this arc lighting system represents gas at eighty-nine cents per thousand. A similar estimate in another mill gives the annual cost of gas $2,188, and electricity at $1,125, or equal to gas at ninety cents a thousand. The annual saving to that mill in lighting expenses by the use of electricity makes a profit of $1,063, which represents six per cent. on $17,716, without making mention of any improvement in work or production due to that light. In both of these establishments the lights were used about 450 hours per year. Other estimates give the cost of arc lighting equal to gas at from sixty-five cents upward per thousand. In the case of incandescent lighting, the cost is more difficult to estimate, because they are run at all degrees of brilliancy, affecting both the power and the life of the lamp.

The duration of the carbon filament is the uncertain factor in estimating the cost of incandescent lighting. I do not know of any instance where all the incandescent lamps have become worn out by the ordinary use to which they are subjected, so as to reach an average of their ultimate duration.

The application of incandescent lighting is new, and the minimum guaranty of both companies may be considered as representing two years' ordinary use in a mill; and my experience in the matter has shown that this is largely exceeded.

The following records give the data of the longest time that I have at hand of the use of incandescent lamps; yet they are far from satisfactory, because they do not represent the average life of the carbons; because they are not all broken, no true average can yet be given.

Both the Edison and the Maxim lamps are guaranteed to average six hundred hours; yet in the New York Post-office the average record of the Maxim lamps is stated to be 2,200 hours up to Nov. 28, and four lamps had already burned 4,820 hours.

The ferry-boat "Jersey City," belonging to the Pennsylvania Railroad, is lighted by the Maxim lights. I have a copy of the official record of these lamps, dated Aug. 29, 1882, showing that at that time the average was 1,609 hours.

The data for the above were taken with lamps in use, and do not represent their ultimate endurance.

Mr. Timothy Merrick, of Holyoke, authorizes me to give the facts respecting his experience with the Edison system in the Merrick Thread Company's mill, No. 3. This mill runs all night, five nights in the week, for fifty-one weeks per year, using light 2,869 hours per annum. It was lighted by ninety-five burners with city gas, costing $2.13 net, which amounted to $225 per month. Ninety-five Edison B lamps (eight-candle power) were substituted for the gas. In the first thousand hours five lamp carbons had broken; and Oct. 20 they had been in use 1,278 hours, and eleven had broken.

Allowing that the lamps average six months' use, the annual cost of lighting is made up as follows:

Two renewals of 95 lamps equals 190 lamps at $1.00	$190 00
Interest and depreciation	153 50
Six-horse power at $10	60 00
Annual cost Edison light	$403 50
Monthly cost Edison light	33 62
Monthly cost gas	225 00

The results from these lamps are very satisfactory, and certainly in excess of what would have been obtained if the lamps had been forced beyond their normal capacity.

The Holyoke Water Power Company furnish water power very cheaply; and the result may be interesting if we hold the Edison Company to their minimum guaranty, and also charge the dynamo with four pounds of coal per hourly horse power.

$4\frac{78}{100}$ renewals of 95 lamps equals 454 lamps at \$1.00....	\$454 00
Interest and depreciation............	153 50
$30\frac{74}{100}$ tons coal at \$5.75..............	176 81
Annual cost Edison light............	\$784 81
Monthly cost Edison light...........	65 36

which is equal to gas at sixty-five cents per thousand.

The mill is situated at the base of a high bank, and is only eleven feet six inches between floors, so it is very hot in summer; and Mr. Merrick informed me that it would have been impossible to run the mill nights during the extremely hot season last summer, if the help had been subjected to the heat and vitiated air from the burning gas.

It must be kept in mind that an instance of a mill running day and night is an extreme one in favor of the electric lights; but the data are given, and the matter can be estimated to suit other times of operation.

If these electric plants were charged the proportionate cost of power, besides coal, the cost would be estimated greater than stated above.

Experience with Electric Light in Mills.

The general opinion of a great number using electric light in a practical way is of more weight than the conclusions of any single investigator.

In the pursuance of my occupation as inspector, it has been my duty to examine nearly every textile mill and many other establishments in New England, New York. New Jersey, and Pennsylvania, where electric lights are used.

I do not recall a single instance where the *quality* of the light was unsatisfactory.

A few extracts from my correspondence on the subject will show how it is regarded by those who deal with electric light solely in regard to its merits in practical application.

As none of these letters were written for publication, I do not feel authorized to give the names of the various writers; but all are either the executive or financial heads of mills insured in the mutual companies. It is but a matter of justice to the confidential nature of my occupation as an inspector, that this anonymous use of letters has been agreed to by their writers.

The letters from woolen mills will first be read. The proprietor of a fourteen-set woolen mill writes:

> We have had forty-four Fuller arc lights in operation six months without any repairs In the weave-room two hundred and eighty kerosene lamps were replaced by eighteen electric lights.
>
> Electric light costs more than kerosene; but I am quite willing to stand the extra expense, as there is as much improvement in the light as the extra expense would represent. The air is not contaminated, and no overheated rooms, as is the case when using so many kerosene lamps. The help all like them, and their work is more perfect. Our warps are all black; therefore, more light is needed than on light work.

The treasurer of another woolen mill using the Brush arc light writes:

> On account of the flood of light furnished the weavers, thus enabling them to make perfect work, as well as on account of the purity of the atmosphere, we feel that the production is increased to such an extent that we think that we cannot afford to do without it.

The superintendent of a woolen mill manufacturing dark goods, where the Edison incandescent light is used, writes:

> We find the running cost of the Edison light to be one-fourth that of gasolene vapor, formerly used in our mill. Regarding the advantages of the light, it is better, safer, and cheaper, and devoid of smell or heat.

The president of a woolen mill, a portion of which is lighted with Weston arc lights, writes:

> The dyamo-machines were put in ten months ago, and have not yet cost us anything for repairs. We cannot well estimate the actual cost of the light. We have an abundance of power. The cost of carbons thus far has been \$3.52 per lamp for ten months.
>
> It is used in finishing, burling, and repair departments, and is well adapted for our carding and spinning rooms.
>
> So far as we have had experience with them, we approve of the light.

The manager of a woolen mill formerly lighted by petroleum gas, which has been replaced by Brush arc lights, states:

> Cost is greater, of course; but we like the light, and don't mind the expense.

It should be added in this connection that these Brush lights furnish much more light than would be possible to obtain from oil-gas.

The superintendent of a woolen mill,

where the Edison incandescent light is in use, writes:

The light is certainly superior to gas-light, and is much liked by the workmen, particularly the dressers, who require a very good light; and where warps are dressed in different colors it is highly satisfactory in distinguishing the same, which is difficult and sometimes impossible to do by gas-light.

From a few mills on colored cotton goods I select the following. In one mill where the Weston arc light is used, the agent expresses his opinion that—

The excellence of this electric light consists in its brilliancy, steadiness, and freedom from tendency to vitiate the air. For our particular purpose it also has the advantage of being unaffected by the current of air from the dresser fans.

From a gingham mill lighted by Brush and by Weston arc lights we obtain the following information:

We have a thousand looms lighted with fifty lamps, giving ample light, more than we obtained with a gas-jet at each loom. I believe that we could light more than twenty looms to a light; but with these we get a splendid light, which is the same to us as gas at sixty-five cents per thousand feet. We would not change back to gas

The manager of a print-works using the Weston arc light states:

We have experienced no trouble whatever, and can only speak of the light in its most favorable terms.

The agent of a fine cotton mill lighted by the Brush arc light, gives this opinion:

Our experience leads us to the belief that a system of electricity is to be the artificial light of the future, especially in buildings of any magnitude.

The superintendent of a cotton mill lighted partially by the Maxim incandescent, and partially by the Weston arc lights, sums the result of three years' experience:

We have always considered it a success as compared with gas, as being a better and more economical light.

The experience of the manager of a silk mill with the Thomson-Houston arc light leads him to state:

We have twenty arc lights over seventy-five looms on silk and tapestry weaving. We consider the light far superior for our purpose to anything that we have ever used. It is especially useful in distinguishing colors. The power necessary to run the machines has, we think, been under-estimated, taking a full horse power to each light. The cost of maintaining the light has been very small; the item of repairs not yet coming in, as none have been necessary. We have an abundance of water power.

The following information is furnished by the officers of two cordage mills, one states:

We run four Weston machines of ten arc lights each, making forty lights in all. It requires $32\frac{8}{10}$-horse power to run the whole. The cost of running is about $1.75 per hour; this includes expense of carbons, man's time attending and trimming the lamps, wear and tear, and interest on original plant.

The lights have been used from one to four hours daily for five months in each of the past two years, without repairs; and we are so well pleased with their working that we are putting them up in our new mill.

Although these lights are reported as perfectly satisfactory, they could not have been regulated to the standard brilliancy. On long circuits, arc lights require about one and a quarter to one and a half horse power each; the power being measured at the cylinder of the engine.

The superintendent of a similar mill, where the Arnoux & Hochhausen arc light has been used for some time states:

We have expended nothing upon them for repairs, with the exception of brushes and other minor articles. I consider the electric light to be far superior to any that I have ever used, in point of brilliancy and steadiness.

The list might be continued to greater length; but enough has been given to show the favorable nature of the experience of the industries represented in this association, with the practical application of the various types of electric lighting apparatus in general use.

Of the arc and incandescent lights, both have zealous advocates. While each has merits which fit it for special work, there is a broad middle ground where both are used with success; and the decision as to their relative merits is indeed a difficult task, and the final result must often be one of personal judgment.

Electric arc lights are used in various large buildings, as foundries, rolling-mills, forges, boiler-shops, and dye-houses, with great success.

In no other place does the arc light show such a marked superiority over

other methods of illumination as in these industries. In one instance incan descent lights were unsatisfactory in the erecting department of a large locomotive shop, while arc lights have been used with success in similar places.

The incandescent light is the better adapted for places where the light must be localized, and would be obstructed by objects which would throw shadows if the light came from some remote source, as in the case of an arc light.

A weave-room two hundred and thirty-five by fifty feet, and ten feet high, contained two hundred and twenty-four yard-wide cotton-flannel looms, which were lighted by a hundred and fifteen gas-burners.

Thirteen Brush arc lights were placed in this room, the light being seven feet from the floor, requiring one light to seventeen looms. Twelve of these identical lamps were romoved to another mill in the same yard, and one hung in the engine-room; but the dynamo was not moved, nor its speed changed.

The new weave-room measured two hundred and nineteen by ninety-five feet, and was fifteen feet in height; the light being twelve feet from the floor. Owing to the greater height of the room, these arc lights illuminated three hundred and eighty-four looms, or thirty-two looms to a lamp, in place of seventeen in the former mill.

In the first instance there were nine hundred and four square feet of floor to a light, and in the second seventeen hundred and thirty-four square feet of floor to a light.

This is given as an instance showing how much surrounding conditions have to do in each case. In the first mill incandescent lights would have been cheaper, and in the second this would have been reversed.

Other instances need not be cited, as it is not our object to deal with applications of electric light other than for mills.

Safeguards of Electric Lighting.

The experience of the insurance companies in regard to electric lighting has constituted the subject a factor in underwriting. It is difficult to estimate the amount of hazard to which property is subjected by its use, because the elements of danger are diminished by suitable precautions. The hazards attending the use of the electric light have been overestimated; not in numbers or magnitude, but because too little account has been given to the preventable nature of such occurrences. If these precautions are disregarded, only good luck will avert disaster.

It is sometimes assumed by those ignorant of the facts, that electric lighting apparatus cannot set a fire. Electricity is no exception to other forms of energy. All power can be converted into heat. Your mills are equally liable to hot bearings, whether the motive power is derived from the fires under the boilers, or to the head of water in the mill-pond.

Whenever any one states, as a principle, that the electricity used for lighting cannot set fire to anything, he is not only in error, but is uttering a fallacy which will lead to the destruction of property, if carried into effect in any electric lighting system.

It is better to meet the issue fairly, and the interests of all will be advanced by the consideration of its dangers; for in no other manner can suitable measures for protection be reached.

In the Mill Mutual Insurance Companies there were sixty-one establishments lighted by electricity up to last May.

With few exceptions, the lights had not been in use previous to the autumn of 1881; and many had been started early in the spring.

In these sixty-one establishments I know of twenty-two fires due to electric lighting and assignable to the following causes: Eight were from globules of melted copper or particles of hot carbon falling out from the bottom of the globes. The actual number of fires from this cause was probably many times this number. That class of fires will not continue to happen, as all makers now set their lamp-globes in a tight stand with a ridge around the edge. A flat plate will not answer the purpose, as there was one instance where drops of melted copper rolled off, and set a fire.

Four fires were due to leaking water or washing floors; and two more were caused by water in a dye-house, condensing on the building to which uninsulated

wires were fastened. In most of these instances a grounded circuit formed one of the two connections necessary to divert the electricity from the wires. Many of the lower carbons fell from lamps, and five fires were caused where they fell upon combustible material.

Three fires were caused by cross arcs from one wire to another, where uninsulated wires were fastened against conductors. In one instance the conductor was formed by dust settling upon uninsulated wires; and on a damp day it absorbed enough moisture to form a path for the formation of a cross arc, which started a slight fire.

In another instance the wires were fastened to a damp beam, which was decayed, and was burned nearly in two by the smouldering fire. And in the third instance damp brickwork in a tunnel was a sufficient conductor to establish an arc which did not do any material damage there, but injured the dynamo. Reference has been made to other fires produced by cross arcs started by water, forming a connection between two wires.

In my connection with electric lighting matters for the Boston Fire Underwriters' Union, I know of two fires caused by improper switches; two by water reaching the wires of a circuit already grounded; and one from wires coming in contact with a building, so that their insulation was worn away. There have been many fires from electric apparatus in districts outside of those where I have business, and I do not feel called upon to make any reference to those instances in this connection.

No reference is made to accidents which did not set any fires, although fires would have ensued from many accidents if combustible matter had been present.

I believe that all these fires should be classed as avoidable fires, because the use of well-known precautions would have anticipated their possibility.

The precautions are known only as a matter of experience, because there was no source of information stating the results from electric lighting currents under certain circumstances.

The damage from these fires was in each instance small, as would be expected. It is the experience of the Boston Manufacturers' Mutual Fire Insurance Company, that in mills three-fourths of the fires are in the daytime, and three-fourths of the losses are in the night; so the chance of loss in the night is nine times as great as in the day. During the last two years the introduction of automatic sprinklers has reduced the damage from night fires one-half.

As the electric lights are used during working hours, these accidents come under the head of day fires. When they have happened, there has always been a sufficient number of employes engaged on the premsises to attend to the matter at once. When electricity is diverted from the system, the lights are correspondingly diminished, and general attention directed to the difficulty.

Eyre M. Shaw, Captain of the Metropolitan Fire Brigade of London, when in Boston, during his recent visit to America, stated to me, that, since the introduction of the electric light, there had been about one hundred fires in London from this cause.

Electricity forms the safest method of illumination when the following precautions are observed: The system insulated throughont, so that there is no electricial communication with the earth, or from one part of the apparatus to another, except through the proper conductors, even if the wires should be exposed to water. All switches made with a lapping connection, so that no arc can be formed. Arc lamps provided with globes closed underneath, and the frame so arranged that the lower carbon could not fall out even if the clamp failed to hold it securely.

The wires of incandescent systems provided with a sufficient number of fusible links to secure the system against any damage from an excess of current.

The carrying out of these principles in every detail requires oareful work and constant watchfulness.

Other features in an electric lighting system are advisable to assure the most satisfactory operation of the apparatus and protection to persons, but they may not be essential to secure safety against fire.

The insulation of a system can be assured only by frequent tests. The best

instrument for ordinary use is a magneto which generates an alternating current, and rings a bell like the ordinary telephone "calls." It is used by connecting one wire to the ground and the other to the system. The presence of a fault is indicated by the ringing of the bell when the magneto is put in operation. Means for the systematic trial of the insulation are relatively as important as the use of gauge cocks on boilers.

If an electric lighting system is sufficiently insulated when first arranged, there is no assurance that it will remain so, on account of the numerous changes, blunders, and accidents to which it is subjected. Unfortunately we are not forewarned of any lurking disarrangement of electric apparatus by means of any of the senses, in the same manner that leaking gas appeals to the sense of smell, or as leaking steam produces sound and vapor.

When electric lights become dim in rainy weather, it is conclusive evidence of ground connections, which divert the electricity from the system, causing both commercial loss of electricity and great danger of fire. Two contacts are necessary to divert electricity from an electric lighting system. If one contact already exists, and connects it with the earth, only one more contact is necessary to conduct a portion of the electricity from the system.

In such an event, if the electricity overcomes a conductor of sufficiently high resistance, the electricity is converted into heat sufficient to burn any combustible substance which is present.

Underwriters have two methods of defence against a special hazard—the one to advance the rate to an amount deemed commensurate with the increased danger; and the other method consists in the removal of the source of danger.

With electric lighting, the hazard, not an inseparable part of the system, can be obviated by the measures which have been referred to, and which are applicable to all systems of electric lighting.

In preparing the regulations for the Fire Underwriters' Union, I did not ask for any precaution not found by the experience of accidents and actual fires to be essential to protect either person or property.*

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* REGULATIONS OF THE BOSTON FIRE UNDERWRITERS UNION FOR THE USE OF ELECTRIC LIGHTING APPARATUS.

Wires.—Conducting wires over buildings must be seven feet above roofs, and also high enough to avoid ladders of the fire department.

Whenever the electric light wires are in proximity to other wires, dead guard wires must be placed so as to prevent any possibility of contact in case of accident to the wires or their supports. Conducting wires must be secured to insulated fastenings, and covered with an insulation which is water-proof on the outside, and not easily worn by abrasion. Whenever wires pass through walls, roofs, floors, or partitions, or there is liability to abrasion, or exposure to rats and mice, the insulation must be protected with lead, rubber, stoneware, or some other satisfactory material. Wires entering buildings must be wrapped so that water cannot enter through the tubes.

For inside use loops of wire must be avoided, and the insulated fastenings arranged to keep the wires free from contact with the building.

Joints in wires to be securely made and wrapped. Soldered joints are desirable, but not essential. Wires conducting electricity for arc lights must not approach each other nearer than one foot; and for incandescent lamps the main wires must not be less than two and half inches apart.

Care must be taken that the wires are not placed one above another, in such a manner that water could make a cross connection.

A cut-out which can be operated by the firemen or police must be placed in the circuit in a well-protected and accessible place.

Lamps.—For arc lamps the frames and other exposed parts of the lamps must be insulated from the circuit. Each lamp must be provided with a separate hand switch, and also with an automatic switch which will close the circuit, and put out the light whenever the carbons do not approach each other, or the resistance of the lamp becomes excessive from any cause. The lamps must be provided with some arrangement or device to prevent the lower carbons from falling out, in case the clamp should not hold them securely.

For inside use the light must be surrounded by a globe, which must rest in a tight stand, so that no particles of melted copper or heated carbon can escape; and, when near combustible material, this globe must be protected by a wire netting. Broken or cracked globes must be replaced immediately. Unless a very high globe is used, which closes in as far as possible at the top, it must be covered by some protector reaching to a safe distance above the light.

For incandescent lamps the conducting wires leading to each building and to each important branch circuit must be provided with an automatic switch or cut-off, or its equivalent, capable of protecting the system from any injury due to an excessive current of electricity.

The small wires leading to each lamp from the main wires must be very thoroughly insulated, and, if separated or broken, no attempt made to join them while the current is in the main wires.

Dynamo-Machines.—Dynamo-machines must be located in dry places, not exposed to flyings or easily combustible material, and insulated upon wood foundations. They must be provided with devices capable of controlling any changes in the quantity of the current; and, if these governors are not automatic, a competent person must be in attendance near the machine whenever it is in operation.

Each machine must be used with complete wire circuit; and connection of wires with pipes, or the use of ground circuits in any other method, is absolutely prohibited.

The whole system must be kept insulated, and tested every day for ground connections, at ample time before lighting to remedy faults of insulation if they are discovered.

Preference is given for switches constructed with a lapping connection, so that no electric arc can be formed at the switch when it is changed; otherwise the stands of switches, where powerful currents are used, must be made of stoneware, glass, slate, or some incombustible substance which will withstand the heat of the arc when the switch is changed.

The effects of an electric lighting current upon the person bear no relation to the injury which that current can do to property. Electricity has the two properties of quantity and tension, which are independent of each other. The heating effects are due to the quantity, and the results upon the person are proportional to the tension, of the electricity.

However, the greater the tension, the more liable is the electricity to force its way through bad conductors. The arc light is produced by a current of high tension and small quantity; the incandescent light is formed by a current of low tension and great quantity.

Therefore, in the arc light system the most secure insulation is essential; but with incandescent lights, if the insulation were ineffective, there would be more liability of fire. In incandescent lighting systems the whole reliance for safety is not placed upon the insulation; but small fusible links are placed at various points in the wires, so that, if the quantity of the current exceeds a certain amount, the fusible link will be melted, and cut off the electricity before other damage could ensue.

Although these safety catches were originally devised for the purpose of protecting the carbon filaments of incandescent lamps from destruction by extra currents, yet they are essential to assure safety against fire; and it is the use of these safety catches, and not from the fact that the electricial portions of the system can be handled with impunity, that has given the insurance interest somewhat of a bias in favor of incandescent lighting.

Electric lighting should be encouraged on account of its inherent qualities of safefy. Any system that is in conformity to the insurance regulations is also in its best condition electrically.

Electricity, like all forms of energy, is dangerous to the extent that it is not held in control. The same is true of steam in boilers, or water in millponds. Like fire, they are all "good servants, but poor masters."

DISCUSSION.

Mr. EDWARD KILBURN.—I think, after the exhaustive remarks of Mr. Woodbury, it would be folly for me to attempt to say any thing more than to state that we have just adopted the Edison incandescent system of lighting in our No. 6 mill, Wamsutta Mill, New Bedford, and we have three of the Edison K dynamos, equal to 360 A lights each of sixteen-candle power; and we are running in the mill regularly seven hundred and twenty lights. On Saturday last we weighed the power required to produce these lights, and we found it to be 63.7 horse power, or 8.6 lights to a horse power; and we light four looms to a light, and so proportionately throughout the mill. The mill has some 51,000 spindles in it; and (we have not had it long enough to determine very definitely about it, but so far as we can see now) we believe it to be as cheap as gas at fifty cents a thousand. We are unable to see it in any other way. I don't know that I have any thing more to say on the subject. Mr. Woodbury has said all that is necessary on the other part, much better than I could. The lights prove with us very satisfactory. It is certainly a beautiful light, and it is free from heat and smoke, etc. Our help like the lights very much indeed, and are very anxious to get from the other mills into this mill, and assign as a reason the electric lights. And we have help come from other mills; and we are overrun with help to-day, and a great many assign as a reason for their coming the electric lights. We run the lights from the engine which drives the mill.

Mr. C. W. LIPPITT.—I certainly agree with Mr. Kilburn, and commend to the fullest extent the excellent address on electric lighting, which I have had the pleasure of listening to from Mr. Woodbury. I feel there is very little now for me to say. There is one point I would like to refer to before giving some figures, however, which I hope will not be without interest. That is, when I received the notice to appear here to-day in connection with electric lighting, I felt compelled to notify the Secretary, as I admit, that it would be impossible for me to consider the scientific part of this system. My connection with the electric light has been for a year or two enly, and that entirely of a commercial or manufacturing nature. The particular matter concerning which I suppose you will expect to hear from me to-day consists of a portion of the experience of the Social Manufacturing Com-

pany with electric lighting in the Globe Mill, belonging to that corporation, at Woonsocket, R. I. In the fall of 1879, I think, a twenty arc light Brush machine was purchased and put in operation in the mill. It answered the purpose very well, cutting out a large number of lights, and giving satisfaction for a year or two. Since then that machine has been changed; and we are now using two ten-light Weston arc machines, They also have given us good satisfaction. Our experience with the arc light was of such a character that it tended to increase the inclination, felt by several members of the corporation, to try in a cotton manufacturing establishment an incandescent light. Several attempts were made to get a plant of this character; but we were obliged to wait some little time before we could get one which promised to be satisfactory. The machines which we are now using were furnished by the United States Electric Lighting Company of New York. At the time that I applied to them for this plant, about a year ago, they were just on the point of bringing out their present system of electric lighting, and were very much averse to letting me have the machines that we are now using. The reason was that the present system that they are now introducing is far superior to the old system which we have now in use. It was only after a good deal of urging that I obtained permission to put the machines in. It was with the distinct understanding, that, in case at any time any of the results that were obtained from our present plant should be used or published in any way, a careful statement should be made calling attention to the fact that they were old machines, and that they did not produce as good results as can be produced by the new machines. For instance, the most important point perhaps in connection with these machines that we are now using, in which they are inferior to those that are at present being sold, is that it requires one-horse power to produce between three and four lights; whereas the machines that are now being sold will produce from seven to eight lights to a horse power. We have two dynamos, and in connection with them there is a regulator which it is necessary to use with these old machines. The plant, including the cost of the dynamos and the regulators, the foundation, belting, the shafting, the labor necessary to put it up, and everything complete ready to run, cost $2,552.90. An important part of the expense of running electric lighting is in the horse power. That I have tried to get at as carefully as possible. The power is taken from the main engine of the mill, that runs about fifty revolutions a minute. It is a double Corliss engine. The expense of the horse power—including interest on the cost of the engine, foundations, the building, the boilers, and so on, taking forty-five thousand dollars to cover those items, also including the coal, the labor, and every other expense that could be fairly charged against the power, whether used for electric lighting or for the production of yarn in the mill—figures down to $\frac{92}{100}$ of a cent per horse power per hour. That is a little over thirty dollars per horse power per year. As this was so near to one per cent. per horse power, and as that is the ordinary price, I believe, for calculating the power for electric lighting, I have taken in these calculations one cent per horse power per hour as the cost of the power. Following, then, the calculation, I find that eight per cent. interest on the electric plant gives me $204.23. It was also found, by carefully testing the engines when they were running the electric lighting machines and nothing else, that they were developing for these two incandescent machines thirty-horse power. Thirty-horse power, at one cent per horse power per hour, figures thirty cents per hour for the horse power. The lights ran during the winter of 1881 and 1882 three hundred and sixty-seven hours. That would give us $110.10 for the cost of the power. The breakage is the next important item. The lights are now guaranteed to burn six hundred hours. Although the lights only ran three hundred and sixty-seven hours, I have taken the actual breakage. Of course that breakage would cost us nothing, but still I have figured it in. The actual breakage of these lights for three hundred and sixty-seven hours was twenty-seven. They were renewed at an expense of fifty cents each, which gives us $13.50 for the breakage. The dynamos and the regulators were taken care of by the engineer who had charge of the main engine, with-

out any extra expense, and consequently nothing is to be charged to the cost of the light for that care. The amount of repairs was practically nothing. After the lights had been running for a few days, some difficulty was found in managing them, and a man came on from New York to remedy the trouble. There was a small bill for his time; but that of course was no part of the repairs, and I have not included it. It was about twenty dollars. The total cost of burning one hundred and seventeen incandescent lights for three hundred and sixty-seven hours was $327.83. We burn one light to four looms. That enabled us to cut out two hundred and twenty gas-lights that were called four feet burners each. Two hundred and twenty gas lights burning four feet per hour, with gas at $2.20, burning three hundred and sixty-seven hours, cost us $710.60. In estimating the actual cost of the gas, it is necessary to include interest on the piping that is used to carry the gas about the mill, in the same way as we have considered interest on the wires and the dynamos that carry the electricity about the mill. I have allowed six hundred dollars for the cost of the piping, which would be on the basis of thirty-five hundred dollars to pipe the mill carrying twelve hundred ordinary gas-lights, which I understand is a fair estimate for that expense. Eight per cent. on this sum gives us forty-eight dollars; making the total cost of the gas that would have been used in place of the electric light $758.60. That makes a saving in the use of the electric light of $430.77, or a percentage of fifty-seven per cent. The result with these old style electric machines, producing only three and a quarter incandescent lights per each horse power, burning them for less than four hundred hours, was an actual saving on the cost of gas of fifty-seven per cent. in favor of the electric light.

At one of our other mills we have been using an oil gas. That of course, as cotton manufacturers understand, is considerably cheaper than coal gas. I have made some figures in connection with this oil-gas, which, as near as I can ascertain, are accurate. The cost of the oil gas plant, for a mill using from eleven to twelve hundred burners, is $8,423.29. Estimating the cost of the piping for such a mill at $3,500, it gives us $11,923.29 as the cost of the plant for oil gas. Interest upon this sum at eight per cent. is $953.86. The cost of the oil labor, running expenses, and everything connected with the production of the gas for a winter, taking a year in which the cost of oil was, as I understand, just about what it is to-day, from eight to eight and one-half cents, the total of all these expenses, including the oil, was $941.01; making a total cost for the oil gas that season of $1,894.87. The cubic feet of gas produced by this expenditure was 483,900 feet, making a cost per thousand of $3.91. The burners in the mill where this gas is used for illumination are called one and a half foot burners. If we burn less than that, the figuring would be a little different; but that is practically correct. The cost of two hundred and twenty burners burning one and a half feet of oil-gas per hour, and running for three hundred and sixty-seven hours, is $473.11, against $327.83, the cost of the electric light. The number of burners used with the oil-gas was one burner to two looms. The number of incandescent electric lights was one to four looms; making a saving in favor of the electric light of $145.28, or a percentage of saving over the oil-gas in favor of the electric light of thirty-one per cent. I consider the electric light superior to any other form of illumination, for several reasons. First, on account of the absence of heat. Second, it gives more light for the same money than any other system of lighting. Third, it is fully as safe, if not safer, than any other method of illumination. A very large fire at one of our mills, which caused us a loss of over a hundred thousand dollars, was occasioned by the burning off of a gas-pipe. While there are some dangers connected with electric lighting, they are of a characte that can be better guarded against than the dangers connected with any other method of lighting. It is no more than fair to the United States Electric Lighting Company, who furnished these machines to us, that I should call the attention of the association particularly to the fact that these results, although they are sufficiently favorable to the electric light, have been produced by an apparatus that they were unwilling to dispose of on account of the defects that they knew were inherent in that system. Their present

system will produce lights giving twenty-four to twenty-five candle power each, and from seven to eight lights per horse power. These, as you have heard read at the desk, have lasted a very much longer time than six hundred or a thousand hours. As far as the result of our experience with the incandescent electric light is concerned, I can say that it has been so far satisfactory. As far as I know, it is regarded by almost every one connected with the mill as the most superior form of lighting that we have. As an indication of our appreciation of it, we are this year to increase our plant some two hundred per cent.; and the new mill that we are building will probably be lighted entirely by the incandescent system of electric lighting.

Mr. Livermore.—Since your Secretary asked me to state the facts that have come to my knowledge in the use of electric light, I have endeavored to gather some statistics which we have collected in the use of the arc light in the Amoskeag Mills, which I will give you. We began using the Brush electric light in February, 1881, and have used it ever since. We are using four hundred and sixty-four arc lights, about half the Brush and half the Weston. We light about five thousand looms with them, besides some carding and spinning. I have fifteen rooms, large rooms, some of them having fifty thousand square feet, with these lights. They are hung, the lights are hung, all the way from eight feet and five inches to ten feet and five inches above the floor—usually above looms; and those heights, all of them, seem to give satisfactory results. I think in one case there is twelve feet and ten inches, and I think that that height is probably a little better than one lower. During the last eight months, since the position of the lights which we have had in use has been permanently settled, I have had an accurate account kept of the carbons used, and the number of hours that the lamps have burned, also the expenses for carbons, labor and repairs; and the use of carbons has been about thirty-seven thousand, about equally divided between the Weston and the Brush lamps, and they have burned to that extent that they have furnished one hundred and ninety-eight thousand lamps for one hour. In other words, the number of hours multiplied by the number of lamps gives one hundred and ninety-eight thousand of what we call units of the electric light for the purposes of our statistics. The total cost for carbons, labor, and repairs—and in repairs we include brushes and all those things that are worn out—has been five thousand seven hundred dollars. That has made an average expense of 2.89 cents per hour for each light. Now, that expense is subject to variation, because we have used some very poor carbons, and also because, I suppose, as the lamp-trimmers get more expert they will trim more lamps for the same wages. The carbons vary as much as oil in their quality. Good carbons ought to furnish light at the rate of one carbon for one lamp seven hours; but I have had them run down to four and one-half. The average of these carbons that I have made this calculation upon was, in the Weston lamp 5.14 hours, and the Brush 5.59 hours. Now, to the cost of 2.89 cents per hour per light is to be added the cost of power, the wear and depreciation and the interest. I do not think that it is safe to say that an electric light can be run without any cost for power excepting the extra coal used; because, while that may do on a small scale, yet I think, as a general rule, you cannot burn the candle at both ends without making it burn faster; and, for every hour that you use the electric light, I think you ought to charge for wages in making power, and for wear and depreciation. The same way I think you ought to charge for wear and depreciation of the electric plant. Supposing that you charge for power, every hour in Manchester, steam power, including interest, can be made for fifty dollars per horse power per annum. That may be thought large, but it is a safe estimate. That would make a horse power 16.7 cents per day. If you take out interest it reduces that considerably. Now, whether you run electric light one hour or two hours or six hours in a day, the interest on your plant is running for the whole day; so it is upon the steam plant that you put in to run that electric light; so that if you want to be safe you must charge the interest for the whole day against the electric light, whether you have run it one hour or more hours. Now, taking these figures that I have

given, and reckoning the depreciation and wear of the electric light plant on a cost of two hundred dollars a light—which is a liberal estimate at the present prices that the electric companies charge—charging interest at six per cent. both on that plant and the steam plant, and wear and depreciation at ten per cent., it will make the cost per hour for the arc light 4.8 cents without interest; with interest, for one hour it makes it 10.2 cents. Then for two hours it would be 15.2 cents; half of that for one hour; for three hours, 20.1 cents; four hours, 25 cents; five hours, 30 cents; making the cost diminish very rapidly as you increase in the use. I have compared that with the cost of gas at $1.60 a thousand, which is what we pay in Manchester; and the electric light, supposing that one arc will displace twelve gas-burners, for one hour would cost at that rate for gas, to displace gas, at the rate of $2.04 a thousand; for two hours, at the rate of $1.52 a thousand; for three hours, $1.33⅓; four hours, $1.25; five hours, $1.20 a thousand—unless I have made a mistake in the figuring; but I have given the items, upon which any one can figure for himself. We have colored work, and we have found that one arc light will take the place of twelve gas-burners over looms. We have had one gas-burner over a loom, and one arc light lights twelve looms well; possibly it would light more, but we prefer an abundance of light, because we think there is economy in it, and we have not yet found that we could light, on the average, over twelve looms with one arc light. I have heard it said here, and read in the letters, that an arc light will light, I think, twenty colored looms in some instances; but that is not our experience. If any one undertakes to introduce electric light in the place of gas that he has been using, and expects to realize in the cost of light itself the saving which these figures show, he will be mistaken, because the electric light will be used more than gas is. Our experience is, where a great deal of gas is used in a room, that people hold off as long as they can, because of the heat and of vitiating the air. They have no such scruples with the electric light. Unless people are more watchful than I have been able to be, they will find that the electric light is run more, and therefore the saving will not appear in the balance-sheet as to light, but I am confident it will appear in the quality of the work and in the quantity of the work. The difference between weaving in summer and winter, of course, is well known; and one of the results of the electric light, I think, will be to reduce that difference very considerably. I have heard it remarked, and have believed, that weaving-rooms which were dark, by reason of being basements, were now considered as good as those above ground, because of the electric light. I know that our weaving has improved both in quality and quantity, but I cannot attribute it all to the light; I do some; how much I cannot tell, because the causes have so commingled that we have not been able to get the result separate. As to the average number of hours which an electric light will be burned, I find, upon comparing the gas accounts in some fifteen carding and spinning rooms in our mills, that the average during the year for burning gas runs all the way from one hour to three hours; depending upon the situation of the rooms, whether they are dark or light rooms, and what their dimensions are. And I think it may fairly be calculated that any carding and spinning room well constructed and well lighted will during the year average to burn light in all its burners an hour and a half. Now, if power does not cost anything but for coal, and the electric light will displace twelve or more burners, then it is going to be a good deal cheaper than to use gas at the rate which we can get it. I have had the power required to drive the Weston machines tested by dynamometer, and it resulted in showing a little less than ten-horse power for ten lights; but I do not think it is safe to reckon less than ten horse power for ten lights. I have also compared the power required to run one hundred and twenty Weston lights with one hundred and twenty Brush lights upon forty-light machines, by noting the gate upon the water-wheel which drove them when the wheel drove nothing else, and noting the heights of the water, and there was not enough difference to be worth counting in computing the cost of electric lights; so that I think a horse power per light is what may be safely counted upon to drive

either the Brush or Weston arc lights. I have tried two other arc lights. but I don't know the power that they require. They were not satisfactory, and we have not continued the use of them.

Mr. LUDLAM—Mr. President and gentlemen, I have had some little experience with electric lights; but it is a subject altogether too new to arrive at any opinions with regard to results that we may look forward to as really fixed. I have in use at present some arc lamps of the Fuller system, and also some arc lamps of the Weston system. I am using them in part of my dye-house, where, owing to the conditions, the rooms are dark, and in some places artificial light of some sort is necessary all the time; and under such circumstances there is no question about the economy of electricity as compared with gas. The subject has been so completely ploughed and harrowed this morning that there remains very little more to say. The fundamental point seems to be, however, the question of interest as divided into the length of time during which the lamp is used; that is, when you come to consider the question of economy. In my place, apart from matters of economy, the light has another advantage with respect to the question of color. I have the light in use in two places where it is very important that we should be able to distinguish shades of color, and note any variations, and see that everything is kept to the shade we want; and there, of course, taking that advantage alone into account, the light is very valuable to us. With regard to the question of power, it seems to me that it is perfectly fair to charge electricity with all the power consumed in its manufacture. My business is to make cotton cloth; and, if I have got any power, I can profitably employ it in that way, and I don't see why I should give it to the electrical light people for nothing. I propose to charge them with all the power they get, and, indeed, everything else. And I have been, I say, unable to make any figures which, with the ordinary use of artificial light in mills, assuming it to be one hour a day, taking gas at $1.60 a thousand as it is furnished to us in Lowell, I have been unable to make any figures which prove to me that there is any very decided economy in the use of electric light. I have also two systems of incandescent lamps in use to a partial extent —one the Edison and the other the Maxim so called; that is, the incandescent light furnished by the Weston Company of New England, and which I believe is exactly the same lamp as the United States lamp which Mr. Lippitt has in use. They give very nice lights; and it is only fair to say that with the proposed change which the Edison Company are talking of instituting in my place, that, if they can meet the conditions which they propose to meet, they would be able to beat gas at $1.60. Up to this time I don't think that they have been able to quite do that. It has been touch and go; but, if anything, the gas has had a little the best of it. The collateral advantages, however, are very great; and what their pecuniary value is going to be, of course it would take more experience than I have had, or almost any one, in the use of electric lights, to determine. When I say the collateral advantages, I mean the improved quality of the atmosphere, the absence of heat, and I think a decided safety from fire. As Mr. Woodbury has pointed out. electricity is like a good many other agents that we employ about our mills, and if we don't take care of it it will do us a mischief; but I don't see that it is any more dangerous than it is to run steam about in pipes, as we are compelled to do, at very high pressures, or acids, as one does about a print works, all of which, if it was not looked after, would work you a very great damage. I don't think, with an electric plant properly put in in the first place, that there is very much reason for apprehension on the score of danger. I think the first lamp that was put into my mills was put in with uninsulated wires, and it was before the underwriters had tabooed that sort of thing, and we were all very green about it; and the people who put it in—a New York concern—put the plant in in that way, and we ran it for a year or more with perfect success as regards the light. We have since insulated it ourselves by wrapping it with paraffine cloth, and it is running to-day the lamps of the Fuller Company. It is a small machine. I put it in my packing-room, where color was also a matter of importance, and I put it in without any regard to the question of

economy, but simply to be able to perfectly distinguish colors and sort goods. I don't think that there are any other points that have been left untouched. Everything seems to have been so completely exhausted that I will not take any more of your time.

Mr. Charles L. Lovering—I do not see that I can add anything to what has been said. I am using in one room fifty-four Brush arc lights. Each light displaces seventeen and a half burners. I would use in that room nine hundred and forty-eight gas-burners—a burner to a loom on colored work. I thought up to a year ago, that there was nothing to be saved in the way of expense; but I am rather of the opinion now that there is a saving over gas. I pay for gas $1.75 a thousand. I do not charge against the electric lighting any interest upon the steam plant. Leaving that out, it seems to me, unless I am mistaken, it figures something like a dollar a thousand for gas; but of course all the interest upon a large steam plant should be charged. I have a surplus of power in that room, and I don't know that I put a shovelful more of coal under the fire; yet I am charging to the electric light account four pounds of coal per horse power for every hour I use it. It is a one-story mill, lighted sufficiently from overhead. The lamps are thirteen feet from the floor, and I get a very perfect light. As has been said by Mr. Kilburn, of New Bedford, I am never at a loss to find persons to work in the room; they are always ready to go there. On the floor below, which is only twelve feet high, I am running eighteen lights, and an arc light there will not displace more than twelve gas-burners. But where the room is high, and the rays are thrown so that they will cover more floor surface, an arc light will displace more burners. The space covered by these fifty-four lights is something like fifty-four thousand five hundred feet.

Edward Atkinson, Esq.—It may be expected that I shall have something to say in this debate with respect to electric lighting and its possible dangers; and perhaps you may wish me to add a few words as to its merits.

In making up my own mind as to the policy which the Boston Manufacturers' Mutual Fire Insurance Company and other mutual companies ought to pursue in the matter, I have been obliged, as I have said previously, to study a subject of which even scientific men know little, and of which I knew nothing; treated in technical terms which have themselves been changed by joint consent of electricians during the progress of the investigation. It has therefore been necessary for me to delegate to Mr. Woodbury, who is well prepared by his previous scientific training, the duty of making a complete investigation of the whole subject; and, during the last year, his work has mainly been devoted to the survey and inspection of electric lighting plants in the risks insured by us, including also a full inspection of the different works in which the different kinds of mechanism required are manufactured. In this investigation he has had the hearty co-operation of all the reputable patentees and makers of electric apparatus; and it is a somewhat singular fact, that the rules for the introduction of electric lighting plants, and for their use, which were adopted by the mutual companies with the co-operation of all the reputable electric light companies, have now been standing six months; have been adopted by stock underwriters, with such modifications as the conditions of miscellaneous hazards require; have been virtually copied by the Society of Telegraph Engineers and Electricians of Great Britain; and have required neither alteration nor substantial addition since they were first issued.

In respect to the mechanism for the development of this force known as electricity, it may be said that the art of generating this power is very old, and that mechanism for this purpose, of a very perfect kind, has been long in use in the application of the dynamo-electric machine to electro-plating. There are now a considerable number of dynamo-electric machines; perhaps not as many varieties as there are of turbine water-wheels, but there is substantially the same choice. You can pay a high price for a very perfect machine like the Boyden turbine, from which you may develop ninety or ninety-five per cent. of the power applied to its movement; or you may pay a less price for a machine which will give you the equivalent of a turbine water-wheel, rated at seventy-five per cent. of the actual weight of water applied.

There are various patents upon different kinds of mechanism used for this purpose; but they are upon mere details, as far as I can learn, and there are as many ways of constructing an efficient dynamo-machine as there have proved to be many ways of constructing effective automatic sprinklers and turbine wheels.

In respect to what is known as the arc system of lighting, it may also be said that the principle of the arc lamp is well understood. The carbon candles are now made substantially pure, and very nearly if not quite homogeneous. There is little to be done in improving the arc lamp, except in the clock-work by which its motions are actuated. The arc lamp is, perhaps, most suitable for use outside the factories, or in large places like dye-houses, iron-works, and rolling-mills; but it is probably not as well adapted to the inside work of the textile factories, as what is called the incandescent lamp.

There are three well-known varieties of so-called incandescent lamps, in each of which a film of carbon is interposed in the current of electricity causing friction or resistance; developing heat in the carbon, and, as the secondary effect, developing more or less light. These films of carbon are enclosed in glass globes from which the air has been substantially exhausted.

The question at issue in deciding upon the respective merits of the Edison, the Maxim, and the Swan light, is,—

1st. The durability of the film of carbon, and its capacity to bear the electric current without being destroyed.

2d. The durability of the lamp as a whole, including the glass globe.

There is probably more room for improvement in the carbons for the incandescent system, or for the substitution of some other material and in the making of these lamps, than in any other part of the system of electric lighting. The incandescent lamp may not give as much light in proportion to the power applied as the arc light will give in certain directions or lines of light: but the incandescent lamp can be placed where the light is most available; and more light can probably be developed of a useful kind, or better adapted to specific purposes, in proportion to the power applied, than from the other system.

As I have said, the whole subject of electric lighting is shingled and plastered over, and obscured by the claims of various patentees; and the relative value or validity of these patents can only be determined when they have been carried through the courts. Every user of every kind of light should secure a sufficient bond of indemnity to protect him against the litigation which has begun, and the possible damage which may ensue. Under what has been called the "*electro-mania*," various substantial corporations and many wildcat companies have been organized; and some of them have been floated at fancy prices for the stock. If it were within the province of an underwriter to issue policies of insurance upon the profits which are expected to be based upon a share in patent rights in electric lighting, I think it would be quite safe to issue policies of a kind once offered to me, when I was the clerk of a manufacturing company, upon a cotton mill, which I submitted to one of the oldest stock-underwriters in State Street for insurance. I unfolded the plans, and described the risk; and he cheered me by saying that he would issue a policy "at a very low rate of premium." As the mutuals had then just begun, and the usual stock rates were very high, I was much encouraged by this remark; and, as I prepared to draw my application, he said, "Perhaps you misunderstand me, Mr. Atkinson: the low rate will be upon a policy assuring you that the mill will be consumed by fire within a reasonable time. We will not insure you against loss by fire at any price."

I think it would be pretty safe to issue policies assuring the large portion of those who take stock in patent rights upon electric lighting, that they will make no profit out of them. The contest will be bitter, but the end is probably not far off when the manufacture of electric apparatus and the use of the electric light will come down to a commercial basis with a fair commercial profit to the best companies making the apparatus, and a fair commercial profit to those who apply this apparatus to use.

My conclusion individually is this—that an incandescent light is the true factory light of the future; that it is the safest light which can be put into a mill, the best light to assure perfect work, and the best means of lighting a mill if re-

gard be given to the conditions of health of the operatives. As a mere measure of economy and profit, I have been assured by skillful men who have made use of the electric light in their own factories, that, even if it should cost much more than gas, they would not give it up, because under its use they can secure more and better work from their machinery, owing to the more vigorous condition and vitality of the operatives who attend it.

While I say that I consider the incandescent system the safest, I do not object to the use of the arc lamp under proper safeguards. I consider either system better and safer than gas or oil, if put up properly, and operated under constant supervision, subject to the rigid control of a factory yard. It is not to be assumed that there is no danger of fire from the incandescent light; this force called electricity cannot be applied without danger; but all these dangers, as Mr. Woodbury has stated, are of a sort easily avoided, and the dangers from the incandescent system are more surely avoided than the dangers from the arc system of lighting.

In what I have said about danger, you will observe that I limit my observations to the factory yards of which you have charge. The dangers from the rapid extension of electric light wires in cities upon combustible roofs do not rightly come into the present discussion.

I am profoundly convinced, however, that the day is not far distant when even in our dwelling-houses we shall safely use the electric light and power, and when we shall draw gaseous fuel only from the pipes from which we now obtain our illuminating gas. This is the impression which the developments now being made in each of these directions make upon the mind of one who, without scientific training, must yet consider each application of electric science in the conduct of his business.

GUNNERY EXPERIMENTS AT SPEZZIA.

From the "London Times."

The year 1882 will be remarkable in the history of artillery science, because for the first time steel and compound armor have come into competition with guns of the highest calibre, and have shown their power of resisting any but almost fabulous blows. At the same time, a gun has been produced capable of dealing such extraordinary blows, while the whole process of loading and laying the piece can be carried out by a single hand, no stronger than that of a lady. England and France have vied with each other in constructing the plates. The breech-loading 100-ton gun is English. We shall presently attempt to give some idea of the method of working the Armstrong 100-ton breechloader, some of the features of which are quite new in the history of artillery. But before doing so, it should be noticed that the total abolition of trunnions has so narrowed the space occupied by each gun that two of them can be easily and comfortably worked in a turret if necessary, though the present arrangements designed for the "Italia" and "Lepanto" are intended to fire *en barbette* from behind a breastwork. The Germans being confronted with the difficulty occasioned by the width of guns, have solved it by placing only one gun in each turret. Mr. Rendel, formerly of the firm of Sir W. Armstrong, and now at the Admiralty, has solved the same problem by his designs for the mounting of the 100-ton breechloader. Instead of throwing away a gun, he has narrowed the space it occupies, and placed the whole of the loading and laying arrangements safely out of the way, so that there is plenty of room in a turret for two guns if necessary. The 100-ton breechloading gun is composed for more than half its weight of steel, the rest being wrought iron. The inner tube and the nest tube which embraces it are both of steel, and on the exterior of the gun are rings of the same material. It is by far the strongest piece of ordnance ever constructed by the Elswick firm. Its principal dimensions are as follows:—Length over all, 468 inches; length of bore (26 calibres), 442 inches; length of rifling, 335.4 inches; diameter at muzzle,

33.3 inches; diameter at breech, 65.5 inches; diameter of bore, 17.0 inches; diameter of powder chamber, 19.5 inches.

One of the most interesting points with regard to the gun is the method of mounting it. The usual trunnions are entirely absent. The gun lies imbedded on a sort of sledge carriage, which is a mass of steel, weighing about 14 tons. Projecting rings, which form part of the gun, rest in grooves, and prevent any backward or forward motion of the piece on the carriage, and rotatory motion is prevented by strong steel straps. Thus the gun and carriage are securely bound together, having their axles parallel, and recoil together in the same direction. The carriage rests and slides upon the planed surface of two cast steel beams of about 10 tons weight each. They are held together by the recoil press, and their front ends pivot vertically on a massive hinge. Thus the axes of the gun, the carriage, the recoil press, and the slide are all parallel, whatever the elevation, and the difficulty of restraining the rotatory motion caused in other systems by recoil is completely got rid of. The whole weight is taken by two powerful hydraulic presses, which work always together, being acted upon by one common supply pipe. If the muzzle of the gun is to be elevated, the hydraulic rams sink, and the slide, pivoting on its front end, is lowered in rear, carrying with it recoil press, gun and carriage. The reverse takes place when the gun is to be depressed. By this simple arrangement a host of difficulties are at once eliminated, and some terrible strains removed from the system. And not only is there the advantage of harmonious recoil, but the pivoting on the end of the slide enables the gun to be fired through a very small port, which it would fill almost completely. This is an improvement on the "Inflexible," where it has been found necessary to attach to the muzzle of the gun a steel shield formed of two-inch bars, to guard the port from the fire of rifles and machine guns.

The loading arrangements are also extremely simple, and present some features of novelty, besides the mere fact that the gun is loaded at the breech. With the exception of bringing up the ammunition and ramming, which are performed by another hydraulic apparatus, the whole business of opening and closing the breech is performed by two levers close together, which are worked by one man. He cannot make a mistake, for nothing can be moved out of its proper order, and whatever position a lever may be in at the end of its last movement, the next act is performed merely by pushing or pulling the lever to the opposite side. One pair of levers works the whole breech-closing apparatus. prepares the gun for loading, or opens the breech after discharge. Another pair of levers runs the gun out and in, and elevates or depresses it. It is impossible to run it back or forward too far, and the whole mighty mass of metal may be managed by the hand of a lady, who cannot possibly make a mistake. If she touches a lever it must be to pull it back or thrust it forward from the position in which it then lies, and no movement that can be made will set anything wrong. All the movements involved in opening the breech, withdrawing the breech screw, replacing and closing the breech, can be performed in less than one minute. No damage can be done in the heat of action, and the gun cannot be fired till the operation of loading and closing the breech has been completely performed. The whole process seems like magic, so simple is it, so easy, and so certain. The most inexperienced person can learn the movements in five minutes.

It is almost impossible to make the process of loading and laying the gun understood without the aid of drawings, but some idea of it may perhaps be given to those who have a knowledge of mechanics as applied to artillery. The hydraulic pumps are worked by a small steam-engine, which is governed in its rate of work by the pressure of water produced. It never ceases work, but when no movement is required of any of the parts its action is feeble and only keeps up a certain normal pressure. But if any motion of the system is required and the touch of a lever opens the way for water to create that motion, the engine instantly sets off briskly and continues to act till the cessation of movement tells it that its services are no longer required. It then drops back at once to its slow and feeble action. The engine is seated on a tank, from which the pumps draw their water, and to which the water is returned

after being exhausted from the various cylinders and pipes. Behind and across the breech of the gun, but entirely separate from it, is a slide-bed similar to that of a lathe, and on this bed moves a saddle which carries the loading tube and a rest for the breech screw when drawn out of the gun. Now, let us suppose that the gun has been fired and requires to be loaded. By touching the levers for elevating and running back, the gun is brought into the loading position exactly. It cannot go too far in either direction. A touch on another lever brings the saddle into its proper position, unlocking and turning the breech screw as it comes. A touch on the third lever brings up a piston from the rear and makes it engage a catch in the breech screw. The same lever moved in the opposite direction draws out the breech screw upon a bed made to receive it on the saddle, which is then drawn out of the way by a reverse movement of the lever which brought it up. As the saddle moves side ways, that part of it containing the loading tube comes into position exactly behind the rear end of the bore. The small piston which withdrew the breech screw now pushes the loading tube into the gun, the object of the tube being to protect the threads of the female breech screw from abrasion by the shot. All is now ready for loading, which is performed as in the muzzle-loading 100-ton gun. The projectile and its two half-charges are always kept ready on trolleys, which rise by hydraulic pressure from their places in the magazines, and arrive between the hydraulic rammer head and the breech of the gun. Other levers thrust them forward into their places; the loading tube is withdrawn and the breech closed by a reversal of the different movements just described, which do their work more quickly than the description of their action can be read. The breech of the gun cannot be moved till all is complete, and the piece cannot be fired unless the breech is accurately closed and locked to prevent its opening.

When mounted in the "Italia" and "Lepanto," for which they have been designed, these 100-ton breech-loaders will be *en barbette*—that is, they will be elevated so as to see over the top of the battery as in the French ships, but there will be this advantage, that, whereas in French men-of-war the men working the gun are exposed to the fire of small arms, machine guns, and shrapnel, not a single man will be exposed in the Italian ships. The whole of the machinery, which, though elaborate to describe, is simple and massive in reality, will be under an armored deck. The only break in protection by the deck is the portion through which the rear part of the gun descends, and that will be covered by the mass of metal above it composing the gun. In the French ships, gun and gunners are equally exposed. It needs no technical knowledge to understand how valuable this advantage is.

Having now described the gun, the method of mounting, and the progress of loading, it remains to tell what the piece has actually done, and to explain why such of huge weapons are required for the ships the future. The table on next page shows the rounds lately fired at Spezzia, but it should be remarked, first, that the strength of the gun is calculated to bear with safety a pressure of 29 tons per square inch, while the highest yet reached is only 16.5 tons; secondly, that though the greatest charges ever yet fired in a gun have now been much exceeded, the powder chamber has room for a much larger charge than any used at Spezzia; and, lastly, that there is an evident intention on the part of the Italians to try even more powder, experimentally at any rate. Indeed, it is not improbable that they may, as they did with the muzzle loading 100-ton gun, increase the charge till it passes the limits of safety, for the sake of experiment. Such a course would be interesting to scientific artillerists, but might damage the confidence of the Italian navy in its guns and the Italian people in their navy. It is also probable that the breechloader will be fired at the Schneider steel plate, which is still untouched, and the question will be decided whether the solid 19 inches of steel will resist the impact of a projectile which has a total energy of about 46,000 foot tons.

With regard to the above table, it is to be remarked that both Fossano (Italian) powder and prismatic (German) powder were provided for the experiment, but, finding that there was little to choose between them, the committee decided to adhere to their own explosive. Rounds 13, 16, and 18 were fired with almost the full elevation possible—namely 11 deg.

No. of round.	Powder charge.		Projectile weight.	Velocity—Feet per second.	Pressure in bore of gun—foot-tons per square inch.
	Weight.	Description.			
1	496	Fossano.	—	1433	10.9
2	551.2	"	—	1496	11
3	551.2	"	Chilled. 1974	1512	11
4	606.3	"	Do. 1942	1593	11.35
5	606.3	"	Do. 2001	1609	11
6	661.4	"	Do. 2005	1676	12.5
7	661.4	"	"	1686	12.4
8	716.5	"	"	1767	13.6 / 14.1
9	771.6	"	"	1833	16.5
10	716.5	"	"	1761	14.5
11	496	Prismatic.	"	1423	9 3
12	551.2	"	"	1506	10.4 / 10.3
13	551.2	"	"	Not taken.	11 6
14	771.6	Fossano.	"	1831	16.4
15	606.3	Prismatic.	"	1607	12.8 / 12.5
16	606.3	"	"	Not taken.	13.5 / 13.6
17	716.5	Fossano.	"	"	13.8 / 13.7
18	771.6	"	"	"	15 9 / 16

50 min.—and therefore did not register their velocity because they passed over the screens instead of through them. The range out to sea was evidently enormous, but formed no part of the test trial, and was therefore not measured. The shot was 18.4 secs. in the air before touching the water. Round 17 was fired with almost full depression—namely, 3 deg. 50 min.—and plunged into the sea below the screens, throwing up a magnificent column of water about 100 feet high.

The results of these experiments have shown that guns weighing 100 tons can be manipulated with greater ease by means of hydraulic power than the 12-ton 9-inch gun without it. The whole apparatus takes up very little room, and is perfectly simple in character. There is no reason why a gun of 150 or 200 tons should not be manipulated with equal ease. If it be asked why such monstrous pieces of ordnance should be used at all, the reply is that the condition of the controversy between guns and armor-plates has been completely charged by the construction of steel and compound armor. It has now become necessary to give the idea of perforating the armor of the future with anything less than such a piece as the 100-ton breechloader, and the power of guns against armor must now be estimated by the total energy of the blow delivered instead of by the old calculation of the energy per inch of shot circumference. In the days previous to the trial of the big breechloader, the muzzle-loading 100-ton gun was fired at two English compound plates, 19 inches thick—one made by Cammell and the other by Brown, and at a Schneider steel plate. The English plates were not sufficiently bolted to the backing, and therefore came to pieces more easily than they ought to have done. Moreover, from want of machinery designed to roll such very thick plates they had not been worked down so much as they should have been and will be in the future. The first round fired at each plate was with a charge of 328.5 lbs. of Fossano powder, less than half the highest charge fired from the breechloader. The striking velocity was in each case about 1,220 feet, and the total weight a little above or below 20,000 foot tons. In no case was the plate perforated, nor did

any serious injury occur to the backing, though the shot would have gone clean through a 19 inch wrought-iron plate. A second round was fired at each plate with a charge of 478.4 lbs. of Fossano powder, giving a striking velocity of 1,560 feet approximately, and a total energy of about 33,900 foot-tons. These rounds would have pierced a 25-inch wrought iron plate, yet they failed to perforate any of the three pieces of armor opposed to them, though the steel plate was cracked badly and the two compound plates were broken to pieces. In all three cases the backing suffered, but less in the case of the steel plate than of the others. Against the Schneider steel plate was now fired a Whitworth steel projectile, with a striking velocity of 1,538 feet, and a total energy of rather more than 34,000 foot tons. The plate was broken up and the backing dashed in by the total force of the blow, though the shot itself had been rejected, and lay defaced and distorted, among the débris at the foot of the front of the target. At these experiments were present experts from the principal nations of Europe, and among them all there was but one opinion, namely, that the object of the artillerist in designing ships' guns for the future must be to endeavor to gain, by means of large calibres and enormous projectiles, the greatest possible total energy of blow. Plates of such quality as those tried at Spezzia cannot be pierced, or, indeed, destroyed in any way by projectiles from moderate sized guns. The side of the ship must be driven in by the single blow of a huge projectile delivered from a monstrous piece of ordnance. This is what the future seems to have in store for us, and it can hardly be said that the prospect is satisfactory. It is necessary to correct a statement which appears to have got abroad, that in the experiments against the steel and compound plates the 100-ton breechloader was used. Not so; the gun was the 100-ton muzzle-loader, but it was generally understood at Spezzia that the breech-loader will be fired at a new 19-inch Schneider plate, which is already on the practice ground.

THE SUPPOSED NEED OF A NEW STYLE OF ARCHITECTURE.

By SAMUEL HUGGINS.

From "The Builder."

A FEW considerations against the often heard and even lately expressed wish for a new style of architecture, our great desideratum according to more than one eminent man, which I have not seen urged before, as they may have a tendency to promote that contentment with our old styles which is essential to progress, are here offered.

The advocate for the invention of a new style should be asked: Is there any hope that a new style could be invented equal to or bearing any comparison with the old ones? Or, by what other entirely different or absolutely new elements St. Paul's Cathedral, the Louvre, or Grimani Palace would have been equal to what they are? Supposing our English Palladio, instead of studying ancient and modern architecture in Italy and devising mosques and courtly pageants at home, had devoted the many years so spent to the invention of a new style, and employed it in his design for the Whitehall Palace; and supposing Wren, abjuring both that and the old styles, and resolving to have his great work absolutely original, had depended on geometrical and abstract beauty of form in voids and solids with what moulding and panelling, pronouncing of joints, and decorating of courses he could devise, what would have been the result in each case? Nay, what would the mighty Buonarotti himself have made of St. Peter's had he followed either of these courses?

I do not believe a door or window opening could be so beautifully framed and fashioned by any other elements as

by members of the columnar ordinance. I am not here speaking of window-tracery, in which the Gothic is pre-eminent.

Columns or pillars of some kind would be deemed essential to a new style; but would any other type of form be equal in beauty to the Græco-Roman? Some additions might possibly be made to it, as the bracket-capital, for instance, but it is as difficult to imagine columns with any pretensions to beauty, of a different type to the Doric, Ionic, and Corinthian respectively, as to imagine handsome men and women with other features than eyes, nose, and mouth, or with these in different relative positions.

But the great argument against the course in question is not the difficulty or impossibility of producing a new style equal to the old, but that no new style would be generally accepted. The greatest objection to a new style would be that it was new. I believe if a new style were invented, based on a deep study of nature and geometry, and perfectly beautiful, it would come still-born into the world. A building in such a style would be an architectural monster, with which we could have no sympathy. It could not come home to men's bosoms like one on which their eyes, and through them their whole souls, had long gazed. Its absolute originality would be a fatal defect. There is an instinctive feeling in the human breast hostile to the introduction of a new style. The same sentiment of veneration that disposes us to reverence ancient birth too often more than virtue itself; or, if I may quote in so serious a connection the passionate exclamation of *Hardcastle* in "She Stoops to Conquer," to love everything that is old—old friends, old times, old manners, old books, old wines—the feeling that leads the dying to "cast one longing, lingering look behind," inclines us to prefer old styles of architecture to new. "The mind," says Sir Joshua Reynolds, "can bear with pleasure but a small portion of novelty at a time. The main part of a work must be in the mode to which we have been used . . . . where all is novelty the attention and exercise of the mind is too violent." And now we are so rapidly leaving other traditions of the past behind us through the material progress of the present age that there never was a greater need to cling to our ancient architectural ones.

"The past fills the urns of the future at the fountain of life," and the new in architecture must flow out of the old as it has ever done. Connection with the past was never cut by any nation at any period of the world. There has been no stopping and no beginning again—inventing a new style, and abandoning the old one for the sake of novelty. The styles have merely changed—changed spontaneously by adaptation to new circumstances; as the Byzantine and other contemporary styles in various countries changed into the different Saracenic styles; in which case the new circumstances were the old styles falling into the hands of a new race holding a new religion, with the peculiar well-known prohibition to represent the human form.

Even the Greeks who, in such a matter, must be held above suspicion of error, did not invent an absolutely new style, but developed one out of the Egyptian and Assyrian; and the styles we are now using, whether Classic or Gothic, may be said to have a genealogical relation to the two oldest styles in the world—to have, so to speak, Egyptian and Assyrian blood in them. There are so many links connecting our present practice with the whole history of architecture, and London and Paris of to-day with the buried and lost cities of antiquity, so that every genuine new edifice is related to every other in the world—related as no poem, or picture, or statue, is related to its kind. Great books make foot-paths for the thoughts of nations, but they are not so linked in a descending chain from age to age as great typical buildings—examples of styles—through which the last erected columnar door case or portico may prove its descent from the temple of Karnack.

The introduction of the old styles into America and Australia has assimilated those new worlds to the old countries of Europe more, perhaps, than anything else.

There is immense advantage æsthetically in retaining the old style. The classic styles, for example, are so many lines of truth and beauty coming down to us from dark antiquity and becoming brighter and brighter, giving every new building the advantage of its predecessor

by the accumulating wealth of the style from the minds of the practitioners in all ages. Architecture differs in this from all the other fine arts: a piece of sculpture or a picture may have no mind beaming from it but its author's; but a modern neo-classic building of merit is not only an exponent of the mind of its designer, but is also, through its component elements, an embodiment of Greek and Roman genius, and has more power or virtue than its author put into it. The most interesting creature of architecture is that in which the old style is fully adapted and appropriated to the situation and purpose. It has not only an æsthetic interest, but an historic, while it is instinct with the mind and heart of the designer, and adorns nature, it is breathing of other days. It has come legitimately into the world, and is a legitimate offspring of art.

A new style is, therefore, not called for by the eternal law of intellectual progress under which the world lies, and of which we hear so much. That law is best obeyed by carrying on the old styles and handing them down to our successors enriched and exalted; in doing which conservatism and liberalism are alike served.

It appears evident to me that no new style that would respond to our multitudinous wants could be so completely based on nature, and so beautiful at the same time, as the columnar system; for the column is the natural pillar of necessity moulded, adorned, and raised into an element of art. With the entablature, which includes the frieze, designed as a girdle or zone of illustrative sculpture, and the modern pilaster, an echo of the column, and the Italian baluster or miniature column, it composes a system the possible viriations on which are all but infinite; and which may well be called, as it was by Goethe, a second or ideal nature.

The development and perfecting by the Greeks in the Grecian Doric, more especially of the Parthenon, by the generalized imitation and idealization of nature, of a combination of forms constituting the columnar ordinance, of such beauty, elasticity, and adaptability, and such power to charm a building into architecture, acting on it like an incantation, that it has become, in whole or in part, the omnipresent and everlasting concomitant, the bones and muscles and nerves of civil architecture, was one of the most fortunate operations of genius, and important events in the history of art.

That it has been misused, and not always applied structurally—wrought into the warp and woof of the building—is its misfortune and not its fault. It reminds me strongly of the identity in the fundamental principles, pointed out by Dr. Buckland, of the construction of animals and plants, and the uniform adoption of analogous means to produce various ends, with so much only of departure from one common type of mechanism as was requisite to adapt each instrument to its own special function, and to fit each species to its peculiar place and office in the scale of created beings.

It determined the main course and history of architecture through all ages and lands. The Parthenon in which it appeared in perfection is the Iliad of architecture.

No architect, I think, should need anything more systematized than the Græco-Roman architecture, a glance at which by a man of any imagination must be sufficient to show its capabilities and susceptibilities, and its proper application, without any other guide or help from the mode or manner of any particular period or place. He who should wish a more marked-out or definite road must be indebted to the great Italian architects, who, guided by the analogy of nature and their own powerful instincts, not only recalled the architecture of their forefathers out of the ruined baths, palaces, and temples, as from chaos, and organized it into an exhaustibly copious system (more copious in combining harmoniously the two principles of construction, the beam and arch, than either Greek or Gothic), but bent it to almost every purpose that modern European life and institutions could require, and showed different modes of using the anciet orders.

The elements of this style, taken by architects into different countries of Europe, as seed is carried by the winds we know not whither, producing varieties of the same plant according to the climate and soil, have in different countries given birth to different varieties of the style, and struck deep root here in Eng-

land; and our Anglo-Italian has been already fully acclimatized and embodied by Inigo Jones, Wren, and their followers, in structures of various classes which stand like finger-posts pointing in every direction.

There is not only any amount of scope for genius within the limits of this style, but the style, beautiful in itself and prone to beauty, has the power of inspiring forms and images of beauty and sublimity. It suggests beautiful ideas, and expands and intensifies those of the architect. No poet ever had so beautiful a language in which to express his ideas as the architect of the present day. Some poets have had almost to create the language in which they wrote. Dante, for instance, had to purify, and polish, and fit for his "Divine Comedy." the popular dialects of his country, and all but create the after-language of Italy. But the condition of the architect is the very reserve of this. He has a rich language, the accumulated stock or fund of the art, that no individual Cadmus, however inspired, could have composed—a language invented by Egyptian, Assyrian, Athenian, Ionian, and Roman, and reorganized into the most copious and elastic system ever framed, by a constellation of men who may each of them be said to have bestridden the world of art like a Colossus, and who touched nothing that they did not exalt.

No introduction of a new constructive principle, as the tensile, into architecture can supersede this style in favor of a new one A new decorative style is necessary to that class of buildings, which are chiefly of iron, and in which the great leading constructive principle is the tensile, a search after which is laudable as a search after truth; but the introduction of the tie along with the arch and beam into stone buildings will not overthrow the laws of stone architecture, and generate the style of the future, as I have seen it somewhere asserted. It can only dictate the style of a class of buildings of which the railway-shed is a prime example, in which the tensile covering is fully displayed.

With such a system as this acclimatized Italian style, which has been ruralized and embodied in villas and mansions of every grade, and shown itself capable of application to cottages and the very humblest residences, and to brick construction as well as to stone, we need no new style nor any backsliding to a past period, however refined, for models. In view of its all-sufficiency I cannot imagine how any want could have been felt for the style that prevailed in the reign of Queen Anne—a period which, however, otherwise interesting for richness of literature and eloquence, was certainly not the Augustan age of art. When the painter is taught to eschew the imitation of ordinary nature as unfitted to raise the conceptions or warm the heart of the spectator, and to imitate a more perfect image of beauty fixed in his own mind, approaching to that central form of the species or class every deviation from which is deformity, is not the imitation by the architect of the imperfect works of man of a non-artistic and limited period a still greater mistake and degradation? Is the style of Queen Anne's day the central or ideal form of architecture?

This new practice has been called designing. I doubt not its leaders will find some scope in it for the exercise of their taste and skill; but it is not properly designing; and in the hands of the younger men it will be imitation of single buildings, or copying, resulting too often in caricature.

A building, it is true, might be consistently copied, though I never saw it suggested—as a great sculptor would copy a human form with correction and idealization, for by so doing a man with a little artistic power could produce an original work of art greater than its prototype. But we do not hear of any operations of this sort in the Queen Anne school.

The fact of their narrowing themselves to a period exposes them to the suspicion that it is for imitation merely of the picturesque gables and dormer windows (which, by the way, are well-nigh obsolete features, come of the obsolete steep roof) of certain old buildings, ignoring the epochal character and true nature of architecture, which, like Nature herself, works from within outwards, and clothes the architect's preconception with material form of true expression and character, as far within the power of architecture or sculpture; and which is as essential in architecture as expression of

the character and passions of men in an historical picture. The movement in question appears to me to be a deviation from the true path of architectural progress, to save the labor of thinking and designing, and to evade the difficulties of art. Its authors resemble the two characters, Formalist and Hypocrisy, in the "Pilgrim's Progress," who, instead of ascending with Christian the Hill Difficulty, turn, for greater ease, each into a by-path on either hand, leading far away from the Celestial Gate.

It has no right to the name of a revival, or bringing to life, for it can have no artistic life in it. Totally unlike the Italian revival, which was a resurrection of the Roman in a new and more radiant form—in its general character it will be architectural mimicry of the air and manner of another age, than which there is nothing more contrasting with the life-giving operations of true art. There is no object in nature so devoid of interest as the produce of such operations. It is neither new nor old. It has proceeded neither from head nor heart. It is dead—

"Dead as old Chaos before motion's birth."

A preference of brick to stucco seems to be some way concerned in this movement. But the claims of brick depend in a great measure on its color—an important matter, especially in country or suburban houses, which should make harmonious contrast with the scenery around. Brick, as it has generally appeared among us, is a mean material, and not so fit to do the honors of architecture as could be wished. I allude to the London bricks of the clay-colored hue, and to the generally harsh crude red brick. But bricks might be colored to be worthy of any situation or purpose, and capable of adding grace to the finest natural scenery. I remember, in some of the older houses about Liverpool, bricks of a pale orange-red, which is the most beautiful of all colors, and the chief ingredient in the richness of autumnal scenes, and infinitely superior to the red stone hue, which can only be redeemed by the tonings of time, or, for the moment, by sunset glows, when it becomes a rich orange. Bricks of this color, combined with warm grey stone dressings, are fit for any building. The light stone is valuable for contrast with the generally dark foliage of the landscape, mention of which reminds me that the designer of country residences, to become part of the scenery of nature, which is sometimes such as no landscape painter has ever done justice to, has a difficult task. He will be best fitted for it who is conscious of those delicate relations which exist between the beauties of nature and his deepest emotions. But this is somewhat digressive.

If love of the picturesque had aught to do with the "Queen Anne" movement, let me say that picturesqueness is not a legitimate aim in architecture. A dwelling-house is to be made as beautiful in form and composition as due attention to its purpose and expression of its purpose will permit. If that leads to a picturesque outline, it is well—and Gothic being more in sympathy with the arboraceous forms of nature, will be more picturesque than classic. But picturesqueness is a quality not to be sought. It arises from fortuitous forms and colors, the result of sun, rain, wind, time, change, and accident, which are the creators of the picturesque both in nature and architecture. The effect of the latter agent, accident, is strikingly seen in the picturesque disposition of mountain chains and groups, which have been caused by repeated throes and convulsions of nature. Beauty, grace, grandeur, sublimity are the qualities to be aimed at in architectural design according to the class of subject; not picturesqueness which, indeed, is not a term in architecture, but in painting.

The follower of the Queen Anne style is not in the way on which to acquire the reputation of being a man of genius, which, according to Sir Joshua Reynolds, is the highest ambition of every artist; nor to gratify the higher and truer ambition of enlarging the boundaries of his art and raising it to its highest dignity. He is moving, in fact, in the diametrically opposite course to that by which the Greeks achieved the Parthenon, by which they became the arbiters of form and exponents of the ideal both in sculpture and architecture. That the productions of this new school are a great improvement on the monotonous stuccoed blocks of the preceding period I readily admit, and credit the school with the introduc-

tion of some pleasing and much-needed qualities into their works, especially with what was a great desideratum in streets and suburban rows—individuality in the houses by which a man is not in danger of knocking at his neighbor's door or walking into his neighbor's house instead of his own. They have got into a better way; but what I must maintain is that it is not the right way. It is a by-path, not the broad highway of architecture—the way of truth and progress marked out and hallowed by the footprints of genius in all ages.

It is boasted that the Queen Anne style is likely to hold its own for some time to come, which is in reality an acknowledgment of its temporary character, for if it were true art it would hold its own, not for some time, but for all time to come. But men have a natural taste for truth, and only genuine architecture retains its place in their affectionate veneration.

They order this matter (of architecture I mean) better in France, if I may judge by glimpses of Parisian doings I have now and then had. But I see no reason why we could not order it better in England. Certainly English architects could if they would, *i. e.*, if they were true to their own gifts and did themselves justice. It should, however, be borne in mind that every sane educated man will not make an architect, which a man cannot be in the true sense of the word without imagination and art feeling, nor a very great one without sound judgment and pure taste also; a combination of qualities which nature seems not very fond of making up. Architecture requires a broader intellect in its professors than some other of the fine arts. A man may go farther, for example, in painting, with the mere faculty for drawing than in architecture. Drawing alone will make some sort of landscape painter, but it will not make an architect worth the name, even when guided by insight into the principles of art. The poet is born, not made; the architect is born and made.

The fully endowed architect is one who to vigorous reasoning powers, well disciplined in the mathematics, adds a lively imagination, with a good measure of æsthetic feeling or sensitiveness, which together is the most glorious gift of Heaven. He has also a due measure of æsthetic culture, by which alone can be developed all his faculties and sympathies, and which Schiller deemed preliminary to moral culture; and a memory stored with all literary and scientific lore. He has that sublime ennobling desire for truth which marks the philosopher, and appropriate, and sympathy with the aspirations and hopes of humanity, which characterize the poet; qualities which insure the utmost amount of both truth and beauty in his work, which will have all the moral power, purifying and elevating, which it is possible to enshrine in stone. As it is his highest glory to so far imitate his Creator as to breathe the breath of artistic life into senseless matter, so it is his highest ambition to have spectators and witnesses of his achievements, fit though few, among his contemporaries and posterity.

The highest gift, says George Eliot, the hero leaves his race is to have been a hero; and I may say the highest gift an architect could leave his profession is to have been an architect in this higher sense of the word,—to have shown the possibilities of his art, and the way to advance it, not by inventing new styles, but by illustrating, enriching, and expanding the old ones, by the creation of new designs; and shedding his own independent spirit to future ages.

This, however, is the high-water mark of the architectonic soul, which all architects cannot, and need not, reach; and I doubt not there are among the leaders of the profession in the metropolis and elsewhere men of sufficient natural endowment so to lead the way, at least, in which their juniors should go, that when they are old they will not depart from it. Let me say to all earnest students, young or old, in the words of Wordsworth on a still more important subject than the present one, but equally applicable to it:

> "Access for you
> Is yet preserved to principles of truth,
> Which the imaginative will upholds
> In seats of wisdom, not to be approach'd
> By the inferior faculty that moulds,
> With her minute and speculative pains,
> Opinion ever changing."

Let them work in sincerity and truth, which will secure sufficient of unity of

aim, uninfluenced by popular taste or fashion, and, however thin their ranks, their works will be like a little leaven in the mass of the national production, which, if it do not leaven the whole lump, may yet exert much influence upon it.

THE KINETIC THEORY OF GASES.

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II.

THE experiments of Andrews* upon the behavior of carbonic acid gas above the critical temperature are far more decisive upon this question than those of Thomson and Joule. In these experiments the pressures were carried to more than 100 atmospheres at different constant temperatures, in which $d\tau=0$ and (34) may be written

$$-d(pv)=\tfrac{1}{3}\Sigma(\mathrm{R}dr+rd\mathrm{R}) \quad . \quad . \quad (40).$$

which is equivalent to (40) as previously written. On the supposition in (36) this may be written

$$-d(pv)=\tfrac{1}{3}f(1-i)\Sigma r^{-i}\,dr \quad . \quad . \quad (41).$$

The following are the numerical results at the temperature of 48°.1C. in which p is given in atmospheres, and the volume v of the gas at one atmosphere is taken as 1000.

p.	v.	pv.
62.60	11.57	724.28
68.46	10.06	688.70
75.58	8.49	641.67
84.35	6.81	564.42
95.19	5.04	479.55
109.40	3.35	366.40

In treating this experiment as a compression dr is negative and both numbers (41) are then positive, while $rd\mathrm{R}$ is shown by the numerical results just given to be much larger than $\mathrm{R}dr$ and of opposite sign. Other results at temperatures nearer the critical temperature of 31°C. are perhaps more striking than those just quoted. The experiments of Andrews as well as those of Regnault and other physicists have shown conclusively that as the temperature of an imperfect gas is augmented it becomes more and more nearly perfect, *i. e.*, it approaches more nearly the state in which there are no intermolecular forces. Hence R is also a function of τ, and such a function that $d\mathrm{R}$ is negative when $d\tau$ is positive, from which it appears that in (34) the terms in $rd\mathrm{R}$ are in all cases of different sign from those in $\mathrm{R}dr$. In fact if the volume remains constant the terms in $\mathrm{R}dr$ vanish because dr vanishes. Hence combining this result with that previously arrived at we see, when temperature and pressure are taken as the independent variables, that since $\Sigma d(r\mathrm{R})$ is negative for increments of each of these variables separately it is so for both together and hence is so always.

8. RATIO OF THE SPECIFIC HEATS OF PERMANENT GASES.—Let κk be the specific heat at constant pressure and k that at constant volume expressed in ft. lbs.; then is κ the ratio in question.

Let dh be the amount of heat imparted during any infinitesimal change of state of a unit of gas; this heat is employed in raising the temperature by $d\tau$ say, and in performing work internal and external. If the work be denoted by dw, by the principle of the conservation of energy we have

$$dh=\kappa d\tau+dw. \quad . \quad . \quad . \quad . \quad . \quad (42).$$

Now if the change occur at constant pressure we have by definition of κk

$$dh=\kappa kd\tau, \quad . \quad . \quad . \quad . \quad . \quad (43).$$

Also, $$dw=pdv+\Sigma\mathrm{R}dr \quad . \quad . \quad . \quad (44).$$

in which pdv is the work of expansion against the external pressure, and $\Sigma\mathrm{R}dr$ is the work done against the intermolecular attractions. It is to be noticed that we have included no term in (44) representing the work done against interatomic forces within the molecule. But such a

* Phil. Trans. Land. R. Soc. 1869, p. 575, or Wullner's Exp. Physik, Bd. 3, S. 680.

term is not needed, for the variation of the atomic distances dr either vanishes or is so small as to make $\mathrm{R}dr$ negligible. For suppose a molecule to consist of a system of atoms held by elastic forces at certain mean distances, then those mean distances are not essentially changed by atomic vibrations, which alternately make the distances greater and smaller.

Now in (34) let $dp=0$, and by it (44) becomes

$$dw=\tfrac{2}{3}akd\tau+\tfrac{1}{3}\Sigma(2\mathrm{R}dr-rd\mathrm{R}) \quad . \quad (45).$$

Now let $\quad \Sigma \mathrm{R}dr=bkd\tau \quad . \quad . \quad . \quad (46).$

in which b is an extremely small positive numerical coefficient (which is zero for perfect gases) expressing what fraction the increment of the work performed against the intermolecular attractions is of the increment of the kinetic energy $kd\tau$. If we adopt the approximate value given in (36) we get for the last term of (45),

$$\tfrac{1}{3}\Sigma(2\mathrm{R}dr-rd)=\tfrac{1}{3}(2+i)bkd\tau \,. \quad (47).$$

Now substitute from (47) in (45) and so obtain a value of dw, which can be itself instituted in (42). Also substitute in (42) the value of dh from (43) and divide by $kd\tau$; (42) then becomes

$$\kappa=1+\tfrac{2}{3}a+\tfrac{1}{3}(2+i)b \quad . \quad . \quad . \quad (48).$$

in which a needs further consideration. Let a' denote what fraction the increment of the mean rotary energy is of the total increment $kd\tau$ of the kinetic energy, and let a denote what fraction the vibratory energy (kinetic and potential) of the atoms within the molecule is of the same quantity. Since these together constitute the total increment of the kinetic energy, we have

$$a+a'+a=1 \quad . \quad . \quad . \quad (49).$$

But in the general case in which the molecule consists of more than two atoms, we have

$$akd\tau=a'kd\tau+\tfrac{1}{2}\Sigma d(r\mathrm{R}) \quad . \quad . \quad (50).$$

as appears from the fact that (34) is the precise value of the first member of (50), but as the intermolecular attractions cannot directly accelerate the rotary velocities by (29),

$$a'kd\tau=\tfrac{3}{2}d(pv) \quad . \quad . \quad . \quad . \quad . \quad (51).$$

is also exact and not approximate. Now substitute in (50) from (36) and (46), and divide by $kd\tau$.

$$\therefore \quad a=a'+\tfrac{1}{2}(1-i)b \quad . \quad . \quad . \quad . \quad (52).$$

Add (49) and (52)

$$\therefore \quad 2a=1-a+\tfrac{1}{2}(1-i)b \quad . \quad . \quad . \quad (53).$$

Substitute this value of a in (48),

$$\because \quad \kappa=\tfrac{4}{3}-\tfrac{1}{3}a+\tfrac{1}{6}(5+i)b \quad . \quad . \quad . \quad (54).$$

from which it appears that the work done against the intermolecular attractions dependent upon the terms in b probably have a very small influence upon the value of κ, but the vibratory motion of the atoms which is expressed by terms in a must in many cases be large, and consequently will, in such cases, exert a considerable influence upon the value of κ.

The experimental values of κ lie between 1.33 and 1.25. If we disregard the terms in b, the corresponding values of a are 0 and $\frac{1}{4}$. The larger values of a belong in general to the more complex molecules, and we seem here to have an experimental measure of the amount of the mean vibratory energy, kinetic and potential, within the molecule. If b is large in these gases, then a is for a given value of κ increased thereby.

In the case, however, in which the molecules consist of but two atoms each, we have by (30) the following equation instead of (52),

$$\tfrac{3}{2}a=a'+\tfrac{1}{3}(1-i)b \quad . \quad . \quad . \quad . \quad . \quad (55).$$

which combined with (48) and (49) gives

$$\kappa=\tfrac{7}{5}-\tfrac{2}{5}a+\tfrac{1}{5}(4+i)b \quad . \quad . \quad . \quad . \quad (56).$$

in which it is seen that b has a somewhat greater influence than in (54) while a has probably a much less influence, there being in this case but one pair of atoms to vibrate instead of three or more pairs, which will cause the value of a to be much smaller than previously.

The most probable experimental values of κ for diatomic gases lie between 1.41 and 1.39. In the case in which the molecules consist of a single atom each,

$$a'=0, \quad \therefore \quad a=1-a \quad . \quad . \quad . \quad (57).$$

in which case (48) becomes

$$\kappa=\tfrac{5}{3}-\tfrac{2}{3}a+\tfrac{1}{3}(+i)b \quad . \quad . \quad . \quad (58).$$

The experimental value of κ for the vapor of mercury, which is the only manatomic gas known, is 1.67.

9. The Second Law of Thermo-Dynamics.—Let (32) be divided by $\frac{2}{3}v$ and multiplied by dv,

$$\therefore\ pdv+\tfrac{1}{3}\Sigma r\mathrm{R}\frac{dv}{v}=\tfrac{2}{3}ak\tau\frac{dv}{v}\quad . \quad . \quad . \quad (57).$$

But v varies as r^3,

$$\therefore\qquad v=cr^3 \text{ and} \frac{dv}{v}=\frac{3dr}{r}\quad . \quad . \quad (58).$$

Substitute in the second term of (57)

$$\therefore\qquad pdv+\Sigma\mathrm{R}dr=\tfrac{2}{3}ak\tau\frac{dv}{v}\quad . \quad . \quad (59).$$

which is a general equation due to Clausius,* holding for any variation of state of a permanent gas. The first member expresses the work done, the first term being that done against external forces and the second term that done against the intermolecular attractions. If these equations are to be applied to gases in which the molecules themselves occupy an appreciable fraction of the total in which they move, the effect of their mutual encounters can be taken into account by regarding the volume v not as the total space occupied by the unit of gas, but as the free space remaining after deducting that actually occupied by the molecules themselves. The space so occupied may be taken to be approximately that of the gas when condensed to a liquid state. Our formulæ may then be all rendered more exact by defining v to be this free space. If the increment of the total work done be denoted by dw, and if for compactness we put $dv\div v=dlogv$ (59) becomes

$$dw=\tfrac{2}{3}ak\tau dlogv\quad . \quad . \quad . \quad . \quad (60).$$

If this general value of dw be substituted in the general equation (43), and the equation be then divided by τ, the result may be written in the form

$$\frac{dh}{\tau}=kd(log\tau+\tfrac{2}{3}alogv)\quad . \quad . \quad . \quad (61).$$

in which we have put for simplicity $d\tau\div\tau=log\tau$.

Since the last member of (61) is a perfect differential, (k and a being supposed to be constant) the first member of (61) is also a perfect differential, and if integrated the value of the integral will depend solely on the limits and not on the special path over which it is extended between those limits. This expresses, as is well known, the fundamental principle of the second law of thermo-dynamics.

* Phil. Mag. 1875, vol. 50, p. 195.

Errata and Corrections in the Feb. No. Page 123, etc.

Page 123,	column 1,	line 35,	for	with, read and.
" 123,	" 2,	" 46,	"	Nilyer, read Meyer.
" 124,	" 1,	" 58,	"	one, read one which.
" 124,	" 2,	" 32,	"	to, read be.
" 125,	" 1,	eq. (4)	"	$x\times dt$, read $x\mathrm{X}dt$.
" 125,	" 1,	line 23,	"	square, read large.
" 125,	" 2,	eq. (6),	"	$x\mathrm{X}$, read $-x\mathrm{X}$, etc.
" 126,	" 1,	" (14),	"	v^2, read V^2.
" 126,	" 2,	line 11,	"	not, read not differ much
" 127,	" 2,	" 49,	"	Enler, read Euler.
" 128,	" 1,	" 46,	"	there, read then.
" 129,	" 2,	eq. (34),	"	k', read k.
" 130,	" 2,	line 12,		read $d(pv)=0$.
" 130,	" 2,	eq. (38),	for	½, read ½f.
" 131,	" 1,	" (40),	"	$vdp=$, read $vdp-$.

The paragraph commencing on page 130, column 2, line 23, and extending to line 10, page 131, column 1, goes on the supposition that the internal work depends upon terms other than those in Rdr, which is not the fact, those being the only ones expressing the work performed against the intermolecular attractions. The correct general equation to replace (39) will be obtained later. The same error vitiates the meaning of Art. 8. We shall, therefore, give a revised investigation of the ratio of the specific heats, after detailing some further considerations which show that Rdr and rdR are of opposite sign, and that the latter is always numerically the larger.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia.—At the meeting of January 20th, Mr. Wm. E. Lockwood, presented a full description of the Shaw locomotive, profusely illustrated by magic lantern, working model, etc., etc.

The Shaw locomotive may be classed as a 37-ton soft-coal passenger engine, with two cylinders on each side, each 10½x24 inches; the two working in combination, being equivalent to one cylinder 14.85x24 inches; two cross-heads; two piston rods; two connecting and parallel rods on each side.

Her drivers are 5 feet 9 inches; weight of engine, 67,000 pounds; coal and water, when in use, 73,000 pounds. Total, 74,300 pounds. Weight of tender, 26,000 pounds; water, 15,000 pounds; coal, 6,500 pounds. Total, 47,500. Total combined, ready for use, 121,800 pounds, or 60.45 tons.

The improvement in engines claimed in the Shaw locomotive are:

First.—No counter-balanced drivers; ergo, no hammer blows and no nosing around.

Second.—A single movement of valve with duplex action.

Third.—Steam is the motor of balance as applied to the reciprocating parts.

Mr. Wilfred Lewis exhibited a machine for the graphical determination of center of gravity and moment of inertia of plane areas. The figure to be calculated is drawn to a suitable scale and placed in the machine, where the outline is followed by a tracing point in order to produce, upon another piece of paper, a figure whose area shall be proportional to the statical

moment of the given figure about an assumed axis. If now the second figure be followed by the tracing point and a third figure be drawn, its area will be proportional to the moment of inertia ; and from the areas thus drawn can be found, by simple arithmetical processes, the center of gravity and moment of inertia.

The machine is intended for application to such figures as cannot readily be solved by the usual methods, such as deck beams, steel rails, and castings, with round corners, large fillets and curved sides, which can only be approximately solved by long and tedious integrations. A planimeter is used to measure the areas and it is thought that by this graphical method, more accurate results can be obtained with less work and without so much probability of error in the operations. The machine can also be made use of to determine the contents of any solid of revolution or its radius by gyration.

Mr. Wilfred Lewis announced that Prof. Channing Whitaker, and a party of students of the Massachusetts Institute of Technology were about to visit Philadelphia. It was suggested that the use of our rooms be extended to them and an attendant employed during their visit, which suggestions met with the unanimous approval of the meeting.

February 3d.—Mr. W. S. Auchincloss exhibited and described his latest forms of Averaging Machine, which consists of an endless platform, the grooves of which represent days or distances. The various weights, representing quantities or values, are placed in these grooves and the endless platform rotated until a balance of the weights is secured, when the exact answer may be read from the accompanying scale and, by then continuing the rotation, the weights fall upon an inclined plane, reach their respective compartments in front, and are again ready for immediate use. The machine is of special interest to the engineering profession in its application to the location of engines, boilers and coal bunkers in steamers, the determination of diameters of pulleys, speed of shafting and average haul of material.

Mr. J. J. de Kinder described the United States Metallic Packing for piston rods, valve stems, pump rods and throttles—exhibiting sections and model; also model with packing it is intended to supersede. The metallic packing consists of eight composition blocks, held in a brass ring, on which are horns holding the springs which regulate the pressure of the block on the rod. It is said to preserve a steam tight joint without appreciable friction or binding. The vibration of the rod is provided for by a ball joint on one end and spring follower on the other, which contrivance is said to give the packing free play and preserve its tightness, notwithstanding a vibration of the rod.

The Secretary announced the sudden death of Mr. Wm. L. Billin, Member of the Club, and upon the motion of Mr. W. Bugbee Smith, the president, was requested to appoint a committee to prepare a memorial for publication in the proceedings.

ENGINEERING NOTES.

THE SUEZ CANAL.—The Works Committee of the Suez Canal Company has now decided upon the expenditure of about £920,000 on improving the canal and its dock accommodation. The first improvement consists in the construction of a siding or resting and passing place, 500 meters in length and 25 meters in width, along the sheltered slope northward from the Quai Eugénie, and the second is the construction of a new dock at Port Said, on the African bank of the canal and south of the Ismail dock. It is to be 750 meters long and 200 meters in width. The widening of the canal where it crosses the Bitter Lake, between Suez and the 152d kilometer station, already commenced, is to be pushed on to completion, so that the width will be 40 instead of 22 meters. The siding at Kantara is to be increased from 1,000 to 4,000 meters in length, while that at the 153d kilometer is to be increased from 700 to 2,000 meters in length, and increased from 26 meters on both sides, measuring from the center of the canal, to 40 meters on each side. The station at Lake Tamsah is to be doubled in area, and thus made to contain as many vessels as the Kantara siding. The curve at the north of El Guise is to be widened from 42 meters to 71½ meters, and other curves are to be similarly treated, to facilitate navigation. At Port Tewfik the siding accommodation is to be greatly enlarged, and the floating dock at the same place deepened. By these improvements, the execution of which is to extend over several years, the estimated traffic capacity will, it is expected, be doubled, or increased to 10 million tons. When the traffic reaches this the committee propose to take into consideration the idea, not the proposal, they say, of constructing a second canal parallel with that existing, so as to make up and down lines of traffic. If the English scheme for the Alexandria canal is carried, there will soon be a lot of work going on in this part of Egypt.

THE ANTWERP WATERWORKS.—At the eighth meeting of the Institution of Civil Engineers the paper on "The Antwerp Waterworks" was read by Mr. William Anderson, M. Inst. C. E.

The author commenced by stating that in 1879 the concession for the supply of water to the city of Antwerp fell into the hands of his firm. Antwerp had a population of 200,000 inhabitants; it ranked as the third largest port in Europe, and was being rapidly extended and embellished. Previous to the construction of the works the water supply was derived from shallow wells and open canals. As the sewage arrangements were very imperfect, the well water, though clear, bright and sparkling, was for the most part, dangerously contaminated. The scheme adopted by the author's firm, the only one practicable from a financial point of view, was originally suggested by Mr. J. Quick, M. Inst. C. E., and consisted in taking the waters of the river Nethe, an affluent of the Escaut, at a point eleven miles from Antwerp, where it was crossed by the Malines road. The waters of the Nethe were, however,

quite unfit to compete with the existing supply, after only ordinary filtration through sand, because they were greatly colored by peaty matter and very finely suspended mud, which could not be separated either by subsidence or filtration. Moreover, there would have been great risk in introducing into an important town water from a river which flowed through a highly cultivated and populous country; and the attempt to supply Antwerp from the Nethe would probably never have been made had not Professor Bischof's process of filtration through spongy iron come under the notice of the author. The properties of finely divided metallic iron as a material for filters had for some time attracted the attention of chemists. Professor Bischof, Dr. Frankland, and Mr. Hatton had demonstrated that it possessed the power of destroying organic impurities, removing color, separating finely-suspended matter, softening, and, above all, destroying the germs of putrefaction, of bacteria, and probably those of epidemic diseases. To confirm the evidence afforded by laboratory experiments, and by spongy iron domestic filters, which had been in use for some time, it was determined to carry out experiments on a large scale at Waelhem, the proposed site of the intake of the works, under the auspices of Mr. Ogston, Assoc. Inst. C. E. The arrangement recommended by Professor Bischof took the form of a pair of filters, having an aggregate area of 680 square feet. The first filter was to be placed on a higher level than the second, and to be filled with a bed of spongy iron and gravel, mixed in the proportion of one to three, covered by a layer of ordinary filter sand, the office of which was to separate the grosser suspended matter. In this filter the water would become charged with iron, to eliminate which it was to be exposed to the air, and passed through a second or ordinary sand filter, in which the red oxide would be deposited. The experiments were carried on for three months, and proved so satisfactory that all doubts about the efficacy of the process were removed, and the designs were made for the permanent works. The terms of the concession required a daily supply of 33 gallons per head for 175,000 inhabitants, or nearly six million gallons per day; but, in the first instance, the pumping machinery and main were to be laid down for only 40 per cent. of that quantity. The works consisted of a 42-in. intake pipe, two settling-ponds of an aggregate capacity of 2,640,000 gallons, a pair of Airy's screw-pumps, worked each by an independent engine, for raising the settled water 19 feet into the spongy iron filter beds; three spongy iron filters having an aggregate area of more than 31,000 square feet; three sand filters of the same area; two cast-iron filtered water tanks, containing together 340,000 gallons, and two pairs of beam pumping engines of 170 horse-power each, together with their boilers and fittings. The Nethe being a tidal river, carrying up the drainage of Malines on the flood and bringing down that of the villages on its upper waters on the ebb, the authorities prescribed certain limits within which alone the water should be taken. These restricted the time available for filling the settling ponds to about three quarters of an hour in each tide. The settling points, of a capacity to hold twelve hours' supply, were excavated immediately in the rear of the river bank and lined with dry stone pitching. The nature of the ground was exceedingly treacherous, a bed of water-logged silt extending under the whole area, at a depth of 6 feet to 7 feet below the surface. It was thought prudent, therefore, to construct the filter beds entirely of earthwork resting on the surface, and to trust to puddle linings to secure the necessary water-tightness, and to adopt pile foundations for the engine-house and chimney. The environs of Antwerp, being very flat, did not permit of a high-service reservoir being constructed. The filtered water tanks were, therefore, placed close to the engine-house, and the service was maintained by uninterrupted running of the engines, which, for this purpose, were arranged in pairs, each pair coupled at right angles, so that they could run at any speed between 1½ and 22 revolutions per minute. To provide against the effect of frost, the novel expedient was adopted of heating the water, as it flowed to the screw-pumps, by means of injected steam, the author stating that the experience of last winter seemed to indicate that the arrangement would prove efficient. The result of eighteen months' working had been very satisfactory, the water having remained pure, bright and clear throughout the time. The spongy iron had not shown any signs of deterioration or wasting; and Dr. Frankland, who had visited the works, had reported very favorably of the process employed, not only with respect to the chemical condition of the water, but also with reference to the complete destruction of bacteria and their germs. The water from the pumping-station was carried in a 20-in. main for ten miles along the Malines road; its course was described at length, together with the appliances for getting rid of air and of avoiding dangerous shocks. The distribution of subsidiary mains and service pipes in the city was explained, together with the manner in which the various services were laid on. By the system adopted a constant circulation was kept up, as far as possible, in the distribution pipes throughout the city. It permitted a range of pipes to be shut off without stopping the supply of the neighboring streets, and even often enabled the service to be kept up when portions of one of the mains had to be shut off. A comparison was instituted as to the relative cost of German and English pipes. The manner of testing, as fast as the pipes were laid, was described, and the paper concluded with the statement that the works were erected in fifteen months, at a cost of £280,000.

Excavating Wet Material by Hydraulic Process.—By J. T. Dodge, Member of the Western Society of Engineers. Read Dec. 5, 1882.

In constructing the "East Approach" of Bozeman Tunnel, which is about 550 feet long from the mouth of the cut to the portal of the tunnel, an unusual amount of wet and sliding clay was encountered.

The first 200 feet of the cut being dry was

excavated without difficulty. In the next 200 feet springs were encountered, and heavy slides occurred, amounting to three or four thousand yards. This material, tough and adhesive, and saturated with water, was extremely difficult of removal. Shovels had to be dipped in water to make it slide off. Water had to be put on the cars to make it slide out, and when once started on a slide it was not disposed to stop, but went from fifty to one hundred feet beyond the limits of the embankment.

The work was begun in February last and was prosecuted with as much diligence as the unfavorable season would allow. By the 15th of July the contractor and all immediately connected with the work had become very much discouraged. The division engineer was applied to for advice and direction.

Timbers had been tried and swept away by the resistless "mud glacier." Driving piles was suggested as a means of holding back the mass. Tunneling through it was seriously considered. To go on with the work as it had been going on seemed hopeless.

"Sluicing" had been thought of when the work was first commenced, but there did not appear to be a sufficient supply of water within a reasonable distance. The placer mines of Helena being still in process of working, the writer availed himself of the opportunity of examining their methods of using water in the removal of earth.

After doing so, and after having an interview with the principal owner, a gentleman of long experience in hydraulic mining, a survey was ordered, and it was ascertained that by digging a ditch three miles long we could reach a creek two miles west of the Summit, which gauged 250 miners' inches, and which would probably supply water enough to remove 500 yards of earth per day.

It was then decided that if the contractor would surrender that part of the work, the railroad company would try the experiment of "sluicing."

Accordingly, the ditch was commenced on the 20th of July and completed on the 9th of August, so that water commenced running through the cut on the latter date.

The grade of the ditch was assumed at 18.2 feet per mile, and its area at about 4 feet. Several flumes had to be constructed across narrow ravines, and some rock cutting was encountered. A "ground sluice" 2 feet wide by 11 inches deep was placed in one side of the cut, on a grade of 2.2 feet per 100, parallel to the grade of the railroad. Water for the jet was taken from the main ditch in a flume 11 by 12 inches, and conveyed by about 300 feet of 6-inch galvanized iron pipe and 112 feet of cotton hose, down into the cut, where it could be operated under a head of 60 feet. The nozzle was provided with one 2-inch ring and one 1¾-inch ring, to be used as circumstances might require. To supply this nozzle probably required about 40 inches of water when under 60 feet head.

The 1¾ jet was more compact and effective in cutting down tough and hard banks than a 2-inch jet.

The whole amount of water reaching the work during the first few days of our operations probably varied from 120 to 150 inches. The weather being dry and hot, it began to diminish soon after, and fell as low as 72 inches about the 10th of September. The weather then turning cooler, it increased to a moderate extent, and then remained about the same till work was completed on the 24th of September.

By a careful estimate of the work done in the first twelve days, it appears that the amount of earth moved was 727 yards per day.

The amount done between the 10th of August and the 24th of September was 17,060 cubic yards.

The cost of moving the same, including digging the ditch, buying pipe and hose, and all other expenses, was less than 30 cents per yard, which was far below what it would have cost by shoveling.

The work was prosecuted night and day by three shifts of two men each. Near the close of the work, a few men were employed in daytime in sloping, and some labor was used in making a dam to save part of the material in embankment.

The delay of the work during the time the ditch was being dug was probably a benefit, by giving the ground time to dry and drain out, so that no large slide occurred after "sluicing" was commenced.

The amount of earth moved between the 1st of February and the 20th of July, by shoveling at the bottom and scraping from the top, was 19,730 cubic yards, an amount which could have been removed in twenty-seven days by sluicing with the volume of water used during the first twelve days.

IRON AND STEEL NOTES.

Our Production of Bessemer Steel in 1882.—We have received complete statistical reports from the companies owning the fourteen completed Bessemer steel works which were in operation in the United States during the year 1882. From these reports we learn that the quantity of Bessemer steel ingots produced in the United States last year was 1,696,450 net tons, or 1,514,687 gross tons. The increase over the production of 1881 was 10 per cent. This is a much smaller annual rate of increase than has been made for several years. The increase in the production of 1881 over that of 1880 was 28 per cent., while 1880 increased 30 per cent. over 1879. The production of Bessemer steel ingots in the last six years has been as follows, in net tons:

1877	560,587	1880	1,203,173
1878	732,226	1881	1,539,157
1879	928,972	1882	1,696,450

The quantity of Bessemer steel rails produced in 1882 by the fourteen works above referred to was 1,334,349 tons, or 1,191,383 gross tons. This was an increase of 6 per cent. on the production of the same works in 1881, which amounted to 1,253,129 net tons, or 1,118,865 gross tons.

These figures do not cover the total produc-

tion of steel rails in the United States, as there were some rails rolled in 1882 from imported steel blooms, and there were also some open-hearth steel rails rolled. Complete statistics of the production of these kinds of rails, however, have not yet been received. They will probably not quite reach 100,000 gross tons, making our total steel-rail production in 1882 approximate 1,800,000 gross tons, against a total in 1881 of 1,210,285 gross tons.

The fourteen Bessemer steel works referred to above contain thirty-five converters. The Scranton Steel Company is building a 2-converter plant at Scranton, Pennsylvania, which it is expected will be completed and will be put in operation in the spring.—*Bulletin*.

A SINGULAR CASE OF CORROSION OF STEEL. By Prof. Chas. E. Munroe, U. S. N. A.—Through the kindness of Chief Engineer Farmer, my attention has recently been called to the appearance of two cold chisels found in the U. S. S. "Triana" in 1874, and which have since been preserved in the Department of Steam Engineering at the Naval Academy. These chisels were taken from the channelway leading from the jet condenser, and they were located between the foot valve and the air pump. Both chisels were of steel throughout, as was proved by tempering the head. For use, of course, only the points had been tempered. During the time of exposure to the action of the salt water in the channelway the chisels were deeply corroded, but the corrosion was confined entirely to the soft metal, the tempered points not being attacked in the least. The corrosion was deepest at the line of contact between the tempered points and the untempered metal of the haft. The line of immersion, on tempering, is as distinctly marked as if drawn with a shading pen. Since meeting with these chisels I have heard of a similar case of corrosion, although the object has been lost. It was a hammer which had been taken from the boiler of a merchant steamer, the tempered faces of which were intact, while the soft metal was corroded.

Remembering the heated discussion going on in metallurgical circles on the question "What is Steel?" I shall not attempt to decide whether the change which takes place in the tempering of steel is a chemical or a physical one; but it is evident that this change produces a body which is not so readily acted upon by salt water as untempered steel is. It is also probable that when the untempered and tempered steels are brought in contact in the presence of salt water we have an electro-chemical couple, and that this hastens the destruction of the untempered metal. I beg to suggest that this observation may have a practical bearing upon the construction of steel ships.—*Proc. U. S. Naval Institute, No.* 21.

RAILWAY NOTES.

RAILWAYS OF THE WORLD.—An article by M. Paul Trasenster, of the Government School of Mines, at Liege, tracing the growth of the railway system of the world, is quoted from the *Revue Universelle des Mines* in *The Economist*. Starting with the year 1840, when railway construction was in its infancy, M. Trasenster shows how the 5,000 miles of line then in operation have grown into a total of nearly 250,000 miles, and how the system, which was then practically confined to a few European countries and the United States, has now spread to all quarters of the globe. In the subjoined table the progress of the development is shown in detail, and it may be summarized thus:

LENGTH OF LINES IN OPERATION ON THE 31ST DECEMBER.

	Europe.	America.	Asia.	Australasia.	Africa.	Total.
	Mls.	Mls.	Mls.	Mls.	Mls.	Mls.
1881.	108,002	122,186	10,774	5,481	3,147	249,590
1880.	105,429	109,521	9,948	4,889	2,904	232,691
1879.	103,237	101,196	9,269	4,363	2,705	220,770
1875.	89,323	84,648	7.072	2,312	1,552	184,907
1870.	64,667	58,848	5,118	1,042	956	130,631
1860.	32,354	33,547	844	350	298	67,393
1850.	14,551	9,604	..	..	..	24,155
1840.	2,131	2,859	..	..	..	4,990

The rate of progression, it will be observed, has been rapid, and on the whole, continuous. There has not, of course, been an equal development each year. In years of prosperous trade and active speculation the work of construction has been pushed on with great energy, while in times of depression it has languished.

Between 1870 and 1881, the year of least activity was 1878, the length of new line opened in that year being slightly under 8,000 miles; while, on the other hand, the year 1881 was one of exceptional activity, no fewer than 15,100 miles of new lines—that being the largest total ever recorded—having been added to the various systems. Passing from the record of the past to the prospects of the future, M. Trasenster takes a survey of the condition of the various countries in which the work of railway construction is being carried on.

On the whole, the probability seems to be, that in the immediate future the railway systems of the world will be developed with much greater rapidity than has yet been attained, and M. Trasenster estimates that the increase in 1883 is not unlikely to be as much as 17,000 miles. Whether such a rapid growth can long be sustained is another question.

RAILWAY GROWTH IN AMERICA.—The past year has undoubtedly been the most remarkable in the railroad growth of the north-west States of America, as it has been the most remarkable in the railroad development of the United States. Nearly all the construction in the north-west is legitimate, in the sense that its object is the development of the country, and that the enterprises promise solid

returns upon the investment. The aggregate of new mileage in this territory, in which St. Paul and Minneapolis are directly interested, is 2,400. The Northern Pacific comes first, with 870 miles newly constructed on all its divisions. The energy with which construction has been pushed on this line is phenomenal, and promises the easy completion of the entire transcontinental trunk before the end of 1883. Then will begin the construction of the lateral lines, north and south from the main line, carrying on laterally the development now confined to a narrow belt along the main line. The St. Paul, Minneapolis, and Manitoba line cannot show, like the Northern Pacific, a long stretch of new trunk line, but it outdoes the other road in the rapidity with which it is gridironing their joint territory in Northern Dakota. With branches and feeders, the total extensions of the Manitoba road in 1882 amounted to 221 miles, of which more than half is in the Devil's Lake and Turtle Mountain country. The agreement between these two companies for an amicable division of territory promises energetic joint action next year in filling gaps and advancing the material growth of Northern Minnesota and Dakota. The North-Western has increased its total mileage in 1882 469 miles, of which 129 miles are in Dakota and 100 miles in Iowa. Perhaps the most significant construction is the north and south branch in the James River valley, which is initiated by the Milwaukee road running north from Millbank. The largest actual construction of the Milwaukee road has been in Iowa. On the division which gives the Milwaukee access to Council Bluffs 263 miles have been built. The next most important construction is the branch from Wabash up to Chippeway and the Valley line, 75 and 82 miles respectively.

MR. BRUNLEES in his address before the Institution of Civil Engineers, briefly referred to the want of railway communication in many productive countries. The immense population of China would derive great advantages from the construction of railways. It had been said that the objection of the Chinese proceeded chiefly from the fear of introducing foreigners in any considerable number. Chinese statesmen, even those most liberal and enlightened, at one time believed that railways were not adapted to the circumstances of China. They had recently formed a different opinion. An official memorial had been drawn up by one important Government officer, and favorably reported on to the Government by another high official, suggesting and recommending the construction of four important trunk lines, and no doubt if these were once executed many more would follow. In India somewhat more than 900 miles of railway were in course of construction, including three bridges of more than ordinary importance. When the works now in progress were completed, India would have nearly 12,000 miles of railway open for traffic. In New Zealand the length of railway in various stages of progress during the year ended 31st March last was 284 miles, and 1,333 miles were then open for traffic, and an additional expenditure of £1,650,000 had been ordered. In Queensland only a few miles appeared to be under construction; but an extensive system of railways was under the consideration of the Government. In South Australia considerable progress had been made in railway building, and this might also be said of Victoria and New South Wales, where there were 342 miles under construction. He regretted that the Australian colonies had not adopted the same gauge for their lines. With the disadvantages which had arisen in England, in India, and in America, from a break of gauge, and from the great advantages which Western and Central Europe had derived from a uniform gauge, it might have been thought prudent on the part of the Australian Colonies to have accepted the experience of older communities. In Canada, 2,910 miles of railway were under construction; and in the United States some 11,000 miles had been constructed during the last year. In the United States and in Canada the tendency was towards a uniformity of gauge.

ORDNANCE AND NAVAL.

A NEW IRONCLAD.—In accordance with Admiralty instructions, preparations are being made at Chatham Dockyard for the construction of another large armor-plated ship, to be named the *Mersey*. She will be over 300 feet long, and about 40 wide. Her engines will be very powerful.

LOCOMOTIVE TORPEDOES FOR COAST DEFENCE.—The session of the Royal United Service Institution, Whitehall Yard, was opened on January 19 with a lecture by Lieutenant C. Sleeman, R. N.—Admiral Boys presiding—on "Locomotive Torpedoes." The lecturer divided the submarine weapons known as locomotive torpedoes, carrying within themselves the power of locomotion, into two classes—first, the uncontrollable (those which cannot be controlled at the will of an operator); and second, the controllable (those capable of being directed by the operator). In the uncontrollable class he placed the Whitehead fish torpedo, and he held that this was the most perfect torpedo of its class. The advantage of this weapon was that up to its limit of accurate range, 500 yards, it was entirely self-acting, and the vessel using it had nothing further to do with it when once it was discharged. Moreover, it had great speed, covering the 500 yards in 40 seconds. Its disadvantages were its limited accurate range, the complicated nature of its adjustments, and the necessity of discharging it from special apparatus. He mentioned that the German government had been making experiments with sunken batteries, containing several Whitehead torpedoes, discharged from the battery by electrical means under the control of those on the shore batteries, with the purpose of increasing the range of this torpedo; but he did no know what success had attended the experiment. He then dealt with the second class, the controllable, and described the Ericsson, the Brennan, and the Lay, the last men-

tioned the invention of Colonel Lay. The first, he said, had been under trial in America, but the trials were not regarded as successful. The Brennan was an Australian invention, and had lately been under experiment at Chatham. This he described as a weapon moved ahead and steered entirely by mechanical means, those means being the connecting of the shaft of two drums placed in the body of the torpedo. These are made to revolve by fine wire reeled on them and connected with the directing point. The steering is effected by increasing or decreasing the velocity of either of the reels at the directing point. At Chatham a speed of 14 knots an hour was attained, and the weapon was controllable up to about 1,200 yards. The lecturer considered that the disadvantages of the weapon were many, while its advantages were few. The Lay then came under notice, and this was illustrated by diagrams. The weapon, which had been successfully experimented with in Russia and Belgium, was the only existing type of its class. It was constructed of steel, one-eighth of an inch in thickness, and was divided into three distinct sections—first, the bow section; second, a reservoir section, with the motive power, carbonic acid gas; and third, the after section, containing the steering apparatus. The total length of the whole was 26 feet, its maximum diameter 24 inches, and it weighed when completely loaded, one and a half ton. The amount of explosive carried in one type of the Lay was 90lbs. of dynamite, while the Russians had increased the charge to 150lbs. of that explosive. The charge could be exploded at will, or by contact. The lecturer described the controllable character of the Lay for its run of a mile and a half for harbor defence, or of three quarters of a mile to a mile when used against a ship. He dwelt upon the value of this torpedo in harbor defence, and he held that the ingenious Nordenfelt sub-marine boat, armed with two of the Lay torpedoes, would make the most successful attack on a harbor most doubtful. He also graphically and clearly described the Lay torpedo in ship attack and defence, and the advantages which a ship possessing the Lay would possess over a better armed vessel without this torpedo. He also pointed out the circumstances under which this torpedo would prove a most valuable factor in the defence of rivers, estuaries, and channels. Admiral Gore Jones, who opened the discussion, dwelt upon the inconvenience and danger arising in the Lay torpedo from the motive power arising from carbonic acid gas in the reservoir, and he stated that in the earlier experiments, the cable kinked, and the experiments had to end. He contended that the use of the torpedo could only be limited. Colonel Gallway, R. E., dwelt upon the danger in a ship of carrying dynamite for the arming of the Lay, and he questioned whether in practice in sea battles the Lay would have all the advantages claimed for it. Captain M'Clear, R. N., offered some observations, and then Mr. Nordenfelt answered some of the objections started by Admiral Gore Jones. Captain Crozier and other speakers took part in the discussion, which closed after the lecturer had replied, with a vote of thanks to that officer.

BOOK NOTICES

PUBLICATIONS RECEIVED.

FOURTEENTH ANNUAL REPORT OF THE MASSACHUSETTS BOARD OF RAILROAD COMMISSIONERS.

MONTHLY WEATHER REVIEW FOR DECEMBER. Washington: Gov't Printing Office.

THE NAVAL USE OF THE DYNAMO-MACHINE AND ELECTRIC LIGHT. By Lieut. Murdock, U. S. N.

PRIMARY TRIANGULATION OF THE UNITED STATES LAKE SURVEY. By Lieut.-Col. Comstock, Corps of Engineers. Washington: Gov't Printing Office.

A TREATISE ON THE DISTILLATION OF COAL TAR AND AMMONIACAL LIQUOR. By George Lunge, F. C. S. London: John Van Voorst.

This purely technical treatise describes with great minuteness the processes and machines employed in the separation of the coal-tar products.

Chapter I. is devoted to the origin and nature of Coal Tar. Chapter II. deals with the chemical constitution of Coal Tar and the properties of the constituents. Chapter III. Applications of Coal Tar without Distillation. Chapter IV. The First Distillation of Coal Tar. Chapter V. Pitch. Chapter VI. Anthracene Oil. Chapter VII. Creosote Oil. Chapter VIII. Carbolic Acid and Naphthalene. Chapter IX. Light Oil and First Runnings. Chapter X. Rectification by Steam. Chapter XI. Ammoniacal Liquor

Free use has been made throughout of pictorial illustrations, and the printing is excellent.

SUGAR GROWING AND REFINING. By Chas. G. Warnford Lock, F. L. S.; G. W. Wigner, F. C. S., and R. H. Harland, F. C. S. London and New York: E. & F. N. Spon. Price, $12.00.

The topics treated in this voluminous treatise are in their order as follows:

Chap. I. Cultivation of the Plant (cane).
II. Composition of the Juice.
III. Extraction of the Juice.
IV. Defecation and Clarification.
V. Concentration and Granulation.
VI. and VII. Curing the Sugar; The Factory.
VIII. Cultivation of the Beet.
IX., X. and XI. Extracting, Defecating and Concentrating the Juice.
XII. Curing the Sugar.
XIII. to XVIII. Maple, Melon, Milk Palm and Sorghum Sugars.
XIX. Sugar Refining.
XX. to XXIV. Factories. Statistics, Analysis and Distillation of Rum.

Ten plates and two hundred and five excellent engravings illustrate the text.

THE HEAVENS ABOVE. By J. A. Gillet and W. J. Rolfe. New York: Potter, Ainsworth & Co. Price $2.00.

A new impulse has been given to the study of astronomy by the celestial phenomena of the

past few years, and a new issue of elementary works is the natural result.

It is doubtless true that a liberal display of diagrams aids the learner in this department quite as much as in any other department of science.

The new book before us is abundantly supplied with well-selected illustrations, and is the most attractive looking school-book we have seen this season. More than the usual amount of space is given to stellar astronomy and rather less than the usual amount to the planets.

ELECTRO-MAGNETS. THE ELEMENTS OF THEIR CONSTRUCTION. Translated from the French of Du Moncel. Science Series No. 64. New York: D. Van Nostrand. Price 50 cents.

The thoroughly practical character of this latest addition to the Science Series, no less than the fame of the original author, will recommend this little manual to amateur scientists, as well as practical electricians.

The deductions of Du Moncel are chiefly based upon patient and skillful experiments. The well known formulas have been severely tested, and the limits of their applicability are duly stablished.

The mathematical investigations are such as the student of ordinary algebra can follow, and the principles finally deduced are so emphasized as to give them a special prominence.

Indeed, the laws, which are concisely formulated and italicized, would, if gathered together, present in a few pages a summary of all the principles necessary to guide the constructor to the making of electro-magnets according to the best known plans, whatever the use for which they are designed.

MISCELLANEOUS.

INTERNATIONAL ELECTRIC EXHIBITION OF VIENNA, 1883.—Among the many companies and societies now existing for the propagation of Electrotechnic science and its applications, the Society of Telegraph Engineers and Electricians is the oldest.

They count among their members not only the most eminent European names famous for scientific knowledge but also well-known in practice. Sir William Thomson, Messrs. Siemens, Preece, Col. Webber, Fleeming-Jenkins, W. Smith, Carey Foster, are among the number. This society has formed an Executive Committee for the special purpose of considering all affairs respecting the International Exhibition in Vienna. They will send the invitations and collect the applications of all English firms interested. There is no doubt, that interest in the exhibition will be much enlivened by the efforts of this distinguished party.

SOME time ago, the *Engineering Review* says, "A Philadelphia machinist devised a substitute for the resin or lead with which it is customary to fill copper pipes in order to bend them without buckling. This consisted of a close-wound square steel coil forming a mandril. To remove the mandril after the pipe was bent, it was only necessary to pull on one end, at the same time giving it a slight twist to lessen its diameter. At the time it was brought out it was said to do well, but nothing has recently been heard of it." Lead or resin meet the requirements for any size of pipe, while a large number of mandrils would be necessary. Perhaps this explains it.

IN chloride of sodium and chloride of potassium solutions, with access of cold air and carbonic acid, copper, brass, German silver and zinc, are violently attacked, while in the absence of carbonic acid, they undergo comparatively little change. The contrary is the case with lead, tin, and Britannia ware, these being attacked more violently when exposed to air free from carbonic acid than in air and carbonic acid. In the latter case, lead is only half as much affected as in former, tin not at all, and Britannia metal very little. In the course of experiments by Professor Wagner, with access of air free from carbonic acid, not a trace of any of the metals was dissolved; with access of air and carbonic acid, considerable quantities of copper, brass, German silver, zinc, and lead were converted into soluble compounds, only a trace of Britannia metal went into solution, and no tin was dissolved.

ALUMINIUM.—The experiments of Prof. Kennedy of London University show that the weight of a cubic inch is .0972 lbs., showing a specific gravity of 2688, and that its ultimate tensile strength is about 12 tons per square inch. The range of elasticity is large, the extension at the yielding point being $\frac{1}{100}$ part of its length. The modulus of elasticity is 10,000. The ductility of samples, 2 inches long, was only 2.5 per cent.; but it is probable that the metal could be improved in this respect.

Taking the tensile strength of this metal, in relation to its weight, it shows a high mechanical value. Its characteristics in this respect, as compared with those of other well-known metals, are shown in the following summary:

	Weight of a cubic foot in lbs.	Tensile strength per square inch, in lbs.	Length of a bar which is just capable of bearing its own weight.
			Feet lineal.
Cast iron.......	444	16,500	5,351
Bronze.........	525	36,000	9,893
Wrought iron...	480	50,000	15,000
Steel of 35 tons per inch....	490	78,000	23,040
Aluminium.....	168	26,880	23,040

It thus appears that, taking the strength of aluminium in relation to its weight, it possesses a mechanical value about equal to steel of 35 tons per inch.

Aluminium is, unfortunately, a very expensive metal, and although an increased de-

mand might lead to a considerable reduction in price, yet the process by which it is extracted leaves little hope at present of obtaining it at a rate sufficiently low for general use. But its mechanical properties point to its suitability for those cases where strength, combined with lightness and a great range of elastic action, are required. The elastic range is about three times that of steel, and about five times that of wrought iron. The discovery of a cheap process of extraction has just been announced.

Several of the trunk lines on the continent have been giving special attention to the establishment of rapid train services between important points for the accommodation of through travelers. A more or less important saving of time is expected to be obtained when the working arrangements are fully completed. According to the *Hamburger Nachrichten* the general arrangements of the "lightning express," which it is proposed to run from Ostend to St. Petersburg (*via* Aix la Chapelle, Dusseldorf, Berlin and Konigsberg), will correspond in every way with those of the train of the same description between Paris and Vienna. The halts will be few, and will only be for such lengths of time as the railway service requirements render indispensable. All the meals for passengers will be served in the dining-room of the train while in motion, and the arrangements for turning the saloons into sleeping cars are very complete. There is a gangway between each carriage, and the staff of attendants is to be sufficiently numerous to ensure the comfort of the travelers in every respect.

The British Association Committee on underground temperatures in their last report adopt 64 ft. per degree rise in temperature, or 0.01566 of a degree per foot depth. To obtain an approximation to the rate at which heat escapes annually from the earth, they reduce the above rate of increase .01566 to Centigrade degrees per centimeter of depth. For this purpose we must multiply by .0182, giving .000285. To calculate the rate of escape of heat, this must be multiplied by the conductivity. Prof. Herschel, in conjunction with a Committee of the British Association, has made a very extensive and valuable series of direct measurements of the conductivities of a great variety of rocks, and has given additional certainty to his results by selecting as two of the subjects of his experiments the Calton Hill Trap and Craigleith sandstone, to which Sir William Thomson's determinations apply. From combining Prof. Herschel's determinations with those of Sir William Thomson, .0058 is adopted as the mean conductivity of the outer crust of the earth, which, being multiplied by the mean rate of increase, .000285 gives the flow of heat in a second across a square centimeter. Multiplying by the number of seconds in a year, which is approximately 31¼ millions, we have $1633 \times 315 \times 10^{-4} = 41.4$. This, then, is the British Association Committee's estimate of the average number of gramme degrees of heat that escape annually through each square centimeter of a horizontal section of the earth's substance.

At the opening meeting of the winter session of the Society of Arts on Wednesday evening, Dr. C. W. Siemens delivered an address in which he dwelt at considerable length on the present and probable future position and cost of electric lighting, and gave information leading to the conclusion that the progress of electric lighting had been greatly checked by that parasite, the promoter, that electric companies, even those promoted for work and not speculation, afford doubtful investment to present shareholders, that electric lighting can be accomplished at a less cost per year than gas, but that the plant will cost more.

A patent has been granted to Herr Beck, of Nordhausen, Germany, for a machine of which the motive force is supplied by gunpowder. In a horizontal cylinder a piston is set in motion by small quantities of powder, which are alternately ignited before and behind it. The gases which have been used escape through lateral openings closed by side valves at the return movement of the piston. The heavy residuum accumulates in the deepest part of the cylinder, and is pushed by the piston into receptacles which are emptied from time to time. The ignition of the gunpowder is effected by a spirit flame or by a gas jet, which is brought to bear upon it by the sucking action of the piston, through an opening provided with a side valve. A Cologne firm of engineers has, according to the *Deutsche Industrie Zeitung*, undertaken the construction of this machine, with a view to its being introduced for sale during this autumn. Amongst the advantages claimed for it is the comparatively small space it takes up, and the fact of its being constantly ready for use. The consumption of powder is relatively small, and no special attendance is required, as the machine is self-regulating.

An interesting article on Lavoisier, Priestley, and the discovery of Oxygen, by G. F. Rodwell, in *Nature* contains the following:—"Now what are the facts in favor of Lavoisier? On November 1st, 1772, he deposited with the secretary of the Academy a note, which was opened on May 1st following, in which he stated that he had discovered that sulphur and phosphorus, instead of losing weight when burnt, actually gained it, without taking into account the humidity of the atmosphere. He traced this to the fixation of air during the combustion, and surmised that the gain of weight by metals during calcination was due to the same cause. He reduced litharge in close vessels 'avec l'appareil de Hales,' and observed the disengagement of a great quantity of air. 'This note leaves no doubt,' says Dr. Thompson, 'that Lavoisier had conceived his theory, and confirmed it by experiment, at least as early as November, 1772.' . . . 'Il est aisé de voir, writes Lavoisier, just before his death, 'que j'avais conçu, dès 1772, tout l'ensemble du système que j'ai publié depuis sur le combustion.'" This date is apparently more than a year before Priestley discovered it without knowing it.

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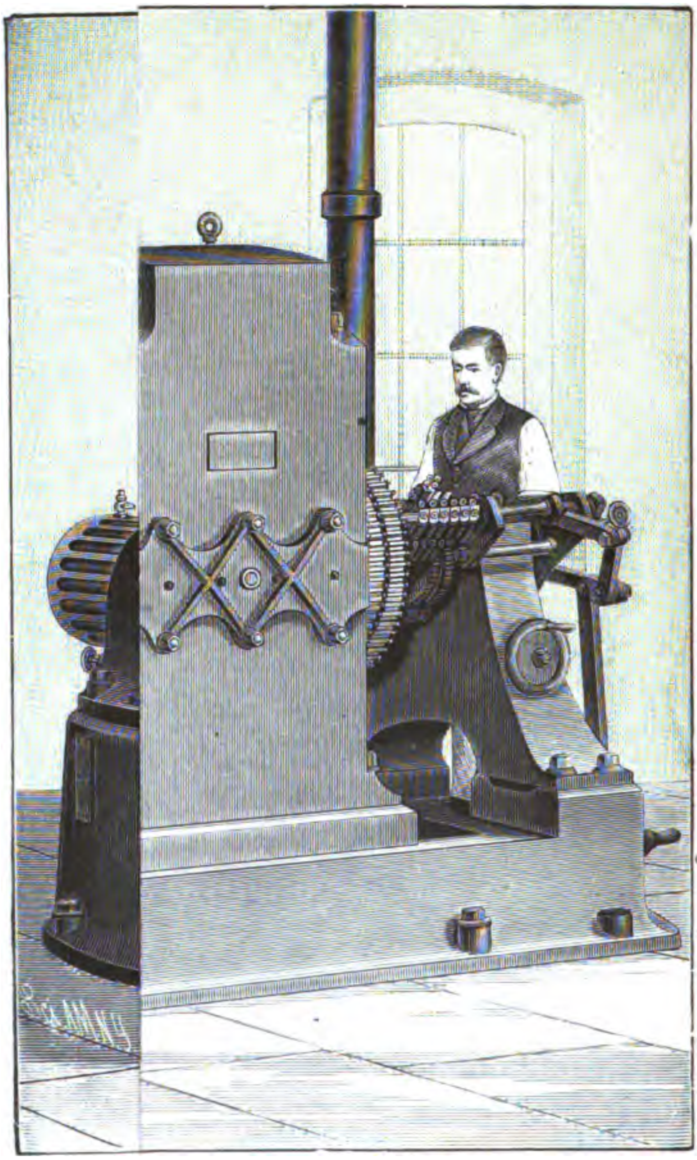


VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXII.—APRIL, 1883.—VOL. XXVIII.

DYNAMO-ELECTRIC MACHINERY.

By PROFESSOR SILVANUS P. THOMPSON, B. A., D. Sc., M. S. T. E.

From the "Journal of the Society of Arts."

II.

The Dynamo in Practice.

It was pointed out, at the conclusion of the former lecture, that none of the four fundamental methods of exciting the field-magnetism of a dynamo was perfect in theory, since none could enable a machine either to generate a constant current, or to maintain a constant electromotive force, whatever the resistance of the external circuit for the time being. Now, as the first function of a dynamo in practice is to feed with sufficiency and regularity a system of lamps, and as those lamps are always* in practice arranged either in series or in parallel, it is clear that in the former case a constant current, and in the latter a constant electromotive force is required.

Combination Methods.

The discovery of the method of rendering a dynamo machine automatically self-adjusting, so that either its electromotive force (according to circumstances) shall be constant, is due to M. Marcel Deprez, and is a result of the considerations arising from the study of the diagrams of the characteristic curves of dynamos.* M. Deprez has, in fact, shown mathematically that if a dynamo be wound with a double set of coils, one of which can be traversed by an independent current, whilst the other set is traversed by the current of the machine itself, there can always be found a certain critical velocity of driving, for which, provided the field-magnets are far removed from their saturation point, the desired condition is fulfilled. Other combination methods have been suggested by Professor Perry and others; and, as the whole matter promises to be of utmost importance in the future practice of constructing dynamos, a summary of the principal methods may be worth tabulating.

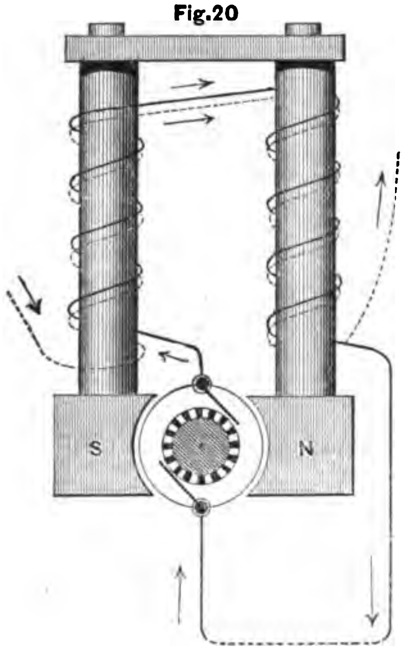
(1.) *Series and Separate* (*for Constant E. M. F.*), *Deprez*.—This method, illustrated in Fig. 20, can be applied to any ordinary dynamo, provided the coils are such that a separate current from an independent source can be passed through a part of them, so that there shall be an initial magnetic field, independent of the main circuit current of the dynamo. When the machine is running, the electromotive force producing the current

* I am aware that occasionally incandescent lamps have been arranged with two or three lamps, in series, in each parallel, or on a multiple series plan. I am not aware of any such arrangement having been satisfactory.

* See *La Lumière Electrique*, December 3, 1881.

will depend partly on this independent excitement, partly on the current's own excitement of the field magnets. If the machine be run at such a speed that the quotient of the part of the electromotive force due to the self-excitement, divided by the strength of the current, is numerically equal to the internal resistance of the machinery, then the electromotive force in the circuit will be constant, however the external resistances are varied. M. Deprez has further shown that this velocity can be deduced from experiment, and that, when the critical velocity has once been determined, the machine can be adjusted to work at any desired electromotive force, by varying the strength of the separately exciting current to the desired degree.

Fig.20



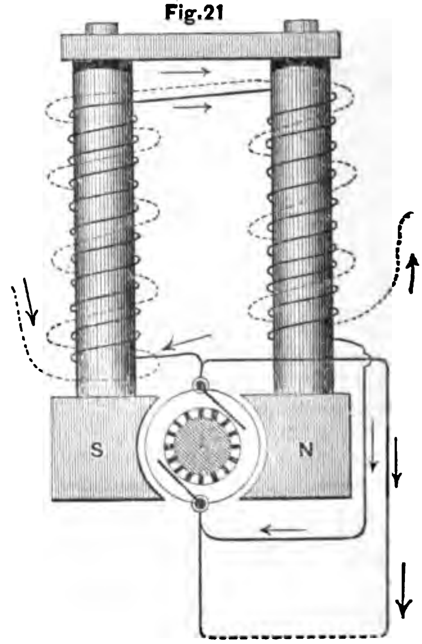
COMBINATION OF SERIES AND SEPARATE.

(2.) *Shunt and Separate* (*for Constant Current*), *Deprez.*—When cases arise, as for a set of arc lamps in series, it is desired to maintain the current in the circuit at one constant strength, the previous arrangement must be modified as indicated in Fig. 21, by combining a shunt winding with coils for a separately exciting current. This arrangement is, in fact, that of a shunt-dynamo, with an initial magnetic field independent of the strength of the current in the circuit.

Seeing that the only object in providing the coils for separate excitement is to secure an initial and independent magnetic field, it is clear that other means may be employed to bring about a similar result.

(3.) *Series and Magneto* (*Constant E. M. F.*), *Perry.*—The initial electromotive force in the circuit, required by Deprez's theory, need not necessarily consist in their being an initial magnetic

Fig.21



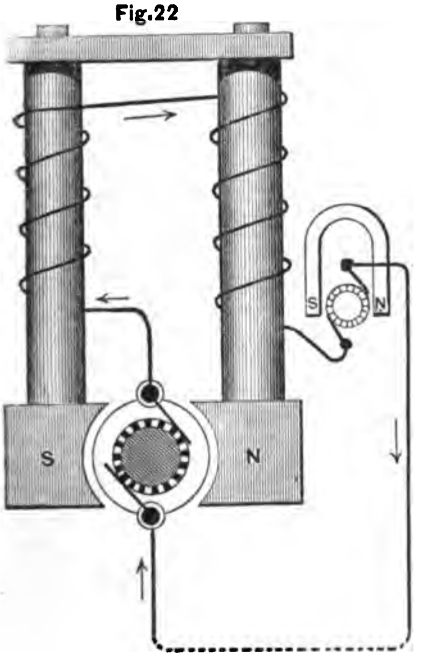
SHUNT AND SEPARATE.

field of independent origin. It is true that the addition of a permanent magnet to give an initial partial magnetization to the pole-pieces of the field magnets, would meet the case to a certain extent; but Professor Perry has adopted the more general solution of introducing into the circuit of a series-dynamo a separate magneto machine, also driven at a uniform speed, such that it produces in the circuit a constant electromotive force equal to that which it is desired should exist between the leading and return mains.*

*Professor Perry gives, in his specification, the following numerical illustration, to which the only

This arrangement, which is depicted in Fig. 22, may be varied by using a shunt-wound dynamo, the magnets being, as before, included in the part of the circuit outside the machines. The combination of a permanent magnet with electro-magnets in one and the same machine, is much older than the suggestions of either Deprez or Perry, having been described by Hjörth in 1854.

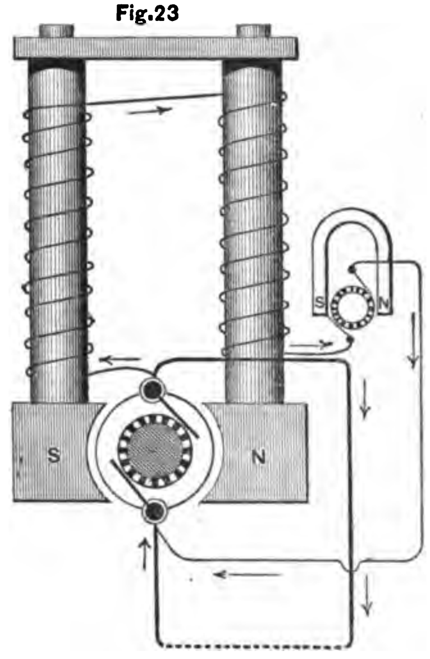
Fig.22



SERIES AND MAGNETO.

(4.) *Shunt and Magneto* (*Constant Current*), *Perry.*—Perry's arrangement for constant current is given in diagram in Fig. 23, and consists in combining a shunt-dynamo with a magneto machine of independent electromotive force, this magneto machine being inserted either in armature part or in the magneto-shunt coils of the machine. As before, a certain critical speed must be found from experiment and calculation.

Fig.23



SHUNT AND MAGNETO.

(5.) *Series and Shunt.*—A dynamo having its coils wound, as in Fig. 24, so that the field-magnets are excited partly by the main current, partly by a current shunted across the brushes of the machine, is no novelty, having been

---

exception that can be taken is, that with so high a resistance as that of three ohms in the machine the system must be very uneconomical. "As an example, if there is only one dynamo machine, and if the resistance of the main cable, return cable and machines, in fact, of that part of the total circuit which is supposed to be constant, be, let us suppose, three ohms, then we find that the dynamo machine ought to be run at such a speed that the electromotive force, in volts, produced in its moving parts is three times the current in ampères, which flows through the field-magnets, consequently this speed can readily be found by experiment. Suppose this constant electromotive force of the magneto machine to be 50 volts; its resistance 0.3 ohms, and the resistance of the dynamo machine and of the other unchanging parts of the circuit 2.7 ohms; and suppose that the speed is that at which the electromotive force produced by the rotating armature of the dynamo is three times the current. Now let there be a consumer's resistance of 2 ohms, the total resistance is 5 ohms. Evidently the electromotive force produced in the dynamo is 75 volts (for call this electromotive force x, then the current will be

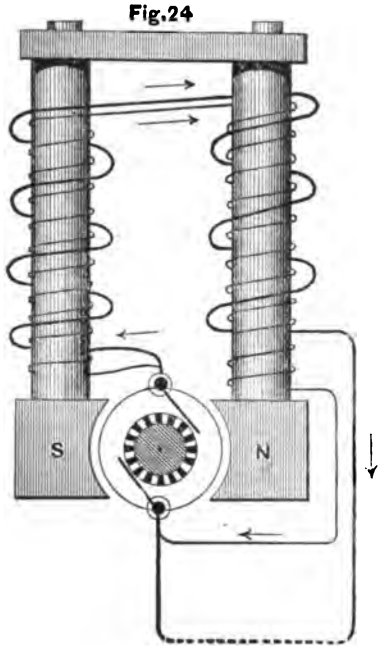
$x \div 3$ whence it follows that $\frac{x+50}{5} = \frac{x}{3}$

or $3x + 150 = 5x$, or $x = 75$) and the total electromotive force in the circuit is, therefore, 125 volts, and, as the total resistance is 5 ohms, the current is 25 ampères, giving an electromotive force of 50 volts between the ends of the consumers' part of the circuit. Now, if the consumers' resistance increases to, say 12 ohms, by some of the consumers ceasing to use their circuits, there is an instantaneous alteration of the electromotive force produced in the dynamo to 12½ volts, or 62½ volts in the whole circuit, and 62½ divided by 15 gives 4⅙ ampères; and this means 4⅙ multiplied by 12 or 50 volts as before, between the ends of the consumers' part of the circuit. Here, again, the calculation is:

$\frac{x+50}{15} = \frac{x}{3}$ whence $3x+150=15x$, or $12\frac{1}{2}=x$. (S.P.T.)

used in Brush dynamos* for some years past. The arrangement is not so perfect as either of the preceding, being more limited in operation. If the shunt coils be comparatively few, and of high resistance, so that their magnetizing power is small, the machine will give approximately a uniform electromotive force; whereas, if the shunt be relatively a powerful magnetizer, as compared with the few coils of the main circuit, the machine will be better adapted for giving a constant current; but, as before, each case will

Fig.24

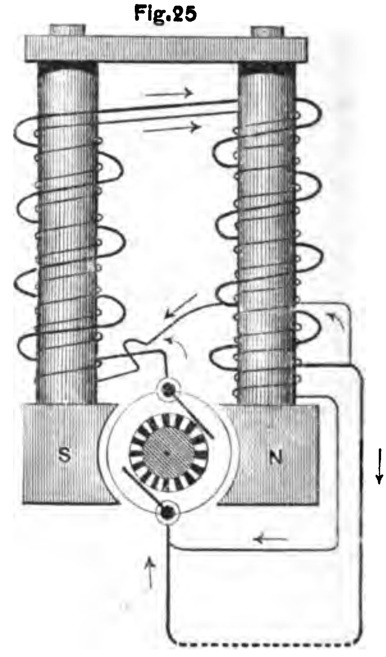


SERIES AND SHUNT.

correspond to a certain critical speed, depending on the arrangements of the machine.

(6.) *Series and Long Shunt.*—I propose to give this name to a combination closely resembling the preceding, which has not yet, so far as I am aware, been actually tried. If, as in Fig. 25, the magnets are excited partly in series., but also partly by coils of finer wire, connected as a shunt *across the whole external circuit*, then the combination should be more applicable than the preceding to the case of a constant electromotive force, since any variation in the resistance of the external circuit will produce a greater effect in the "long shunt" than would be produced if the resistance of the field-magnets were included in the part of the main circuit external to the shunt.

Fig.25



SERIES AND LONG SHUNT.

Although the last two combinations are not such perfect solutions of the problem as those which precede, they are more likely, in my opinion, to find an immediate application,* since they can be put into practice upon any ordinary machine, and do not require, as in the first four combinations, the use of separate exciters, or of independent magneto-machines.

All these arrangements presuppose a constant velocity of driving; but they

* The shunt part of the circuit, originally called the "teazer," was adopted at first in machines for electroplating, with the view of preventing a reversal of the current by an inversion of the magnetization of the field-magnets, but has been retained in some other patterns of machine on account of its usefulness in "steadying" the current. I am informed that Messrs. Siemens and Halske have also used this combination for some time past.

* Since the above was written, on account of a similar combination to the 5th mentioned above has appeared in the pages of the *Electrician*, under the name of Crompton's Compound Dynamo. Mr. R. M. Rosanquet, of St John's College, Oxford, has also suggested a similar arrangement for charging accumulators with a constant electromotive force.

are not the only ones consistent with this condition. An ordinary series dynamo may be made to yield a constant current, by introducing across the field-magnets a shunt of variable resistance, the resistance of the shunt being adjusted automatically by an electro-magnet, whose coils form part of the circuit. This is actually done in the automatic regulator attached to Brush dynamos, as used in supplying a series of arc lights. A shunt-dynamo may similarly be controlled, so as to yield a uniform electromotive force, by introducing a variable resistance into the shunt-magnet circuit, as is done in some of Edison's dynamos. To make the arrangement perfect, this variable resistance should be automatically adjusted by an electro-magnet whose coils are an independent shunt across the mains of the external circuit.

Yet another way of accomplishing the regulation of dynamos is possible in practice, and this without the condition of a constant speed of driving. Let the ordinary centrifugal governor of the steam-engine be abandoned, and let the supply of steam be regulated, not by the condition of the velocity of driving, but by means of an electric governor, such as an electro-magnet working against an opposing spring. If this electro-magnetic governor is to maintain a constant electromotive force, its coils must be a shunt to the mains of the circuit. If it is to maintain a constant current, its coils must be part of the main circuit. Such a governor ought to be more reliable and rapid than any centrifugal governor intended to secure a uniform speed of driving.

Organs of Dynamos.

I now propose to return to some of the rules and suggestions which we arrived at a week ago, in considering the dynamo in theory, and see how they are borne out in the dynamo as constructed in practice. I shall try to illustrate the various points that come under review, as far as possible, from the more recent types of dynamo.

Field-Magnets.

In the classification of dynamos, laid down in my first lecture, we found that those of the first class required a single approximately uniform field of force, whilst those of the second class required a complex field of force differing in intensity and sign at different parts. Accordingly, we find a corresponding general demarcation between the field-magnets in the two classes of machine. In the first, we have usually two pole-pieces on opposite sides of a rotating armature. In the second, a couple of series of poles set alternately round a circumference or crown, the coils which rotate being set upon a frame between two such crowns of poles.

Confining ourselves, at first, to the first-class of machines, we find that, in practice, their magnets differ widely in construction and design. In very few of the existing patterns is there much trouble taken to secure steady magnets, by making them long, heavy, and solid, or with very heavy pole-pieces. I have repeatedly, in testing dynamos, had to report that an unnecessary amount of wire had been wound upon the field-magnets; and I find that the usual reply is that, with less wire, the machine does not work so well. If, however, it is found necessary to wind on so many coils upon the magnets as to bring these practically to saturation long before the machine is doing its maximum work, it is clear that either the iron is insufficient in quantity, or it is deficient in quality. In the Bürgin machines, where cast-iron field-magnets are employed, the smaller magnetic susceptibility of this metal is made up for by employing a great weight of it. In Siemens's smaller dynamos, the amount of iron employed in the field-magnets would be quite insufficient if it were not of high quality. As it is, I am of opinion, the mass of it (especially in the polar parts) might, with advantage, be increased. In some of the early machines of Wilde, and in Edison's well-known dynamos, long field-magnets, with heavy pole-pieces, are found. Edison's dynamos, indeed, are all remarkable in this feature; the pole-pieces and the yoke connecting the iron cores of the coils are made abnormally heavy. This is not more noticeable in the giant dynamos, used at the Holborn viaduct (see Fig. 30), than in the smaller machines used in isolated installations for sixty and for fifteen lights.

The principle of shaping the magnets, so that their external form approximates

to that of the magnetic curves of the lines of force, is to some extent carried out in such widely differing types of machine as the Gramme with "Jamin" magnet, the Jürgensen dynamo, and Thomson's "mousemill" dynamo. The two machines last named exhibit several curious contrasts. In the Jürgensen, the field-magnets have heavy pole-pieces; in the Thomson there are none; and in the Thomson machine the iron core is thicker at the middle than at the ends. In both there are auxiliary internal electro-magnets, fixed within the rotating armature, to concentrate and augment the intensity of the field, according to the device patented by Lord Elphinstone and Mr. Vincent. In the Thomson machine the coils are heaped on more thickly at the middle of the field-magnets; in the Jürgensen, the coils are crowded up around the poles. The latter arrangement I condemned from the point of view of theory last week. If we may judge from a report on this machine by Professors Ayrton and Perry,* the arrangement is not satisfactory in practice, as there are more coils than suffice to magnetize the magnets. Is it possible that the mistake is not in having too many coils, but in having them in the wrong place?

Another suggestion, which was indicated from theoretical considerations, was that of laminating the pole pieces, so as to prevent the production in them of wasteful Foucault currents. Here I find that but one machine has been designed in which this precaution is carried into effect. This is the disk dynamo of Drs. Hopkinson and Muirhead, the field-magnets of which are made up of laminæ of iron, cast into a solid iron backing.

Another matter in which, up to the present time at least, there is nothing but empirical practice to guide us, is the form to be given to pole-pieces, in order to produce the best effect. In fact, we have such singular divergence in practice, as to suggest the thought that little importance has been attached to the matter. Yet upon the form and extent given to the pole-pieces, depend considerations of no less importance than the reduction of idle wire in the armature, the reduction of sparking at the commutator, and the avoidance of counter-electromotive forces in the armature. If the pole-pieces are badly shaped for their work, or approach one another too far round the armature, they may completely perturb the approximate uniformity of the field, and may cause the central portion of the field to be of much weaker intensity than the two lateral regions between the edges of the pole-pieces. When this is the case, the rotating coils are virtually moving in a double field, and it is even possible that, in consequence, the direction of the currents induced in the individual coils may be reversed four or six times as they make one rotation. In such a case the distribution of potential round the separate bars of the commutator will be abnormal, as we shall see later on.

* See *Electrical Review*, Sept. 23, 1882.

Armatures.

The armatures of dynamos of the first class may be roughly classified in three groups, according to the manner of arranging the coils. These three groups are—

(1.) *Ring armatures*, in which the coils are grouped upon a ring, whose principal axis of symmetry is its axis of rotation also.

(2.) *Drum armatures*, in which the coils are wound longitudinally over the surface of a drum or cylinder.

(3.) *Pole armatures*, having coils wound on separate poles, projecting radially all round the periphery of a disc or central hub.

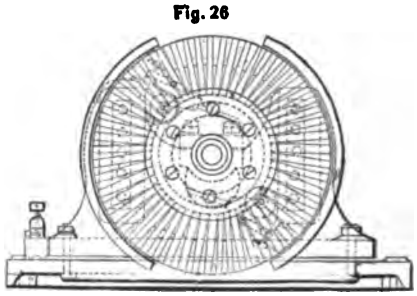
(To these we shall add a fourth form, namely, that of *Disk armatures*, when we deal with dynamos of the second class.)

The object of all these combinations is to obtain the practical continuity of current spoken of in section *t* of the first lecture. Some of the individual coils should be moving through the position of maximum action, whilst others are passing the neutral point, and are temporarily idle. Hence, a symmetrical arrangement around an axis is needed. Ring-armatures are adopted in practice in the dynamos of Pacinotti, Gramme, Schückert, Gülcher, Fein, Heinrichs, De Meritens, Brush, Jürgensen, and others. Drum armatures are found in the Siemens (Alteneck), Edison, Elphinstone-Vincent, Laing, and other machines.

Pole armatures are used in the dynamos of Allen and of Lontin. There are several intermediate forms. The Bürgin armature consists of eight or ten rings, side by side, so as to form a drum. The Lontin (continuous-current dynamo) has the radial poles affixed upon the surface of a cylinder. The Maxim armature is a hollow drum wound like a Gramme ring, and has, therefore, a great quantity of idle wire on the inner surface of the drum. The Weston armature has the drum surface cut up into longitudinal poles; there is a similar armature by Jablochkoff, in which the poles are oblique.

Ring armatures are found in many machines, but the ingenuity of inventors has been exercised chiefly in three directions; the securing of practical continuity; the avoidance of Foucault currents in the cores, and the reduction of useless resistance. In the greater part of these machines, the coils that form the sections of the ring are connected in series, the end of one to the beginning of the next, so that there is a continuous circuit all round, an attachment being made between each pair to a bar or segment of the collector. Most inventors have been content to secure approximate continuity by making the number of sections numerous. One inventor, Professor Perry has built up a ring with coils wound obliquely, so that the one coil reaches the neutral point before the preceding one has passed it. I cannot help doubting the advantage of this arrangement; which, moreover, presents mechanical difficulties in construction. Pacinotti's early dynamo had the coils wound between projecting teeth upon an iron ring. Gramme rejected these cogs, preferring that the coils should be wound round the entire surface of the endless core. To prevent wasteful currents in the cores, Gramme employed for that portion a coil of varnished iron wire of many turns. In Gülcher's latest dynamo, the ring-core is made up of thin flat rings cut out of sheet iron, furnished with projecting cogs, and laid upon one another. The parts of the coils which pass through the interior of the ring (in spite of the late M. Antoine Breguet's ingenious proof that some of the lines of force of the field bent round and turned back into the core in this interior region) are comparatively idle. They cut very few lines of force as they rotate, and therefore offer a wasteful resistance. Inventors have essayed to reduce this source of loss, by either fitting projecting flanges to the pole-pieces (as in Fein's dynamo) or by using internal magnets (as in Jürgensen's dynamo), or else by flattening the ring into a disk form, so as to reduce the interior parts of the ring coils into an insignificant amount. This is done in the dynamos of Schückert and of Gülcher. In Fig. 26 is given the latest form of Gülcher's dynamo. The field-magnets, front and back of the ring, are united on the right and left sides in a pair of hollow pole-pieces, which form cases over the ring, covering a considerable part of it. The collector is identical with that of Gramme, but very substantial.

Fig. 26

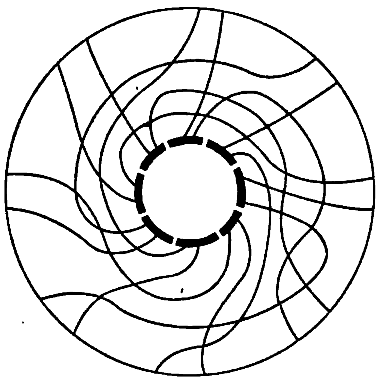


GULCHER'S DYNAMO (NEW PATTERN).

The drum armatures may all be regarded as modifications of Siemens's well-known longitudinal shuttle-form armature of 1856, the multiplicity of sections of the coils affording practical continuity in the currents. In some of Siemens's machines the cores are of wood, overspun with iron wire circumferentially, before receiving the longitudinal windings. In another of their machines there is a stationary iron core, outside which the hollow drum revolves; in other machines again, there is no iron in the armature beyond the driving-spindle. In all of the Siemens armatures the individual coils occupy a diametral position with respect to the cylindrical core, but the mode of connecting up the separate diametral sections is not the same in all. In the older of the Alteneck-Siemens windings the sections were not connected together

symmetrically, the connections (for an eight-part collector) being as in Fig. 27. But in the more recent machines a symmetrical plan has been adhered to, as shown in Fig. 28.

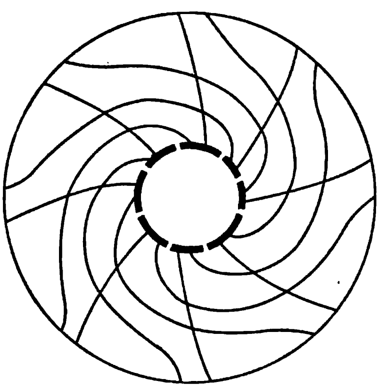
Fig.27



ALTENECK-SIEMENS WINDING (OLD).

In this system, as in the Gramme ring, the successive sections of coils ranged round the armature are connected together continuously, the end of one section, and the beginning of the next, being both united to one segment or bar

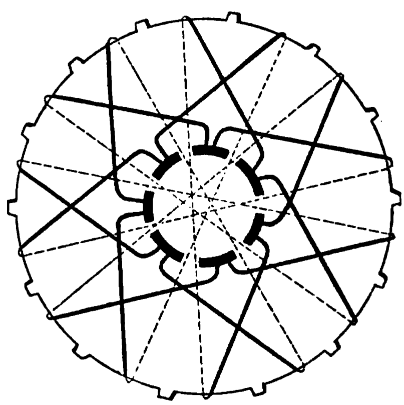
Fig.28



ALTENECK-SIEMENS WINDING (NEW).

of the collector. A symmetrical arrangement is, of course, preferable, not only for ease of construction, but because it is important that there should never be any great difference of potential between one segment of the collector and its next neighbor; otherwise there will be increased liability to spark, and form arcs across the intervening gap. In Edison's modification of the drum-armature, the winding, though symmetrical in one sense, is singular, inasmuch as the number of sections is an odd number. In the first machines there were seven paths as shown in Fig. 29, taken from Edison's Patent Specification. In his latest giant machines, the number of sections is forty-nine. One consequence of this peculiarity of structure is that, if the brushes are set diametrically opposite to one another, they will not pass at the same instant from section to section of the collector: one of them will be short circuiting one of the sections, whilst the

Fig.29



EDISON'S WINDING.

other is at the middle of the opposite collector. The armature of these latest of Edison's dynamos (Fig. 30, frontispiece) is not wound up with wire, but, like some of Siemens's electro-plating dynamos, is constructed of solid bars of copper, arranged round the periphery of a drum. The ends are connected across by washers or disks of copper, insulated from each other, and having projecting lugs, to which the copper bars are attached. Such disks present much less resistance than mere strips would do. The connections are in the following order: Each of the forty-nine bars of the collector is connected to one of the forty-nine disks at the anterior end of the drum, which is connected, by a lug-piece on one side, to one of the ninety-eight copper

bars. The current generated in this bar runs to the further end of the machine, enters a disk at that end, crosses the disk, and returns along a bar diametrically opposite that along which it started. The anterior end of this bar is attached to a lug-piece of the next disk to that from which we began to trace the connections it crosses this disk to the bar next but one to that first considered, and so round again. The two lug-pieces of the individual disks at the anterior end are, therefore, not exactly opposite each other diametrically, as the connections advance through $\frac{1}{49}$ of the circumference at each of the 49 paths. It will be noticed in Fig. 30 that the collector is very substantially built, and that a screen is fixed between the collector and the rest of the armature, to prevent any copper-dust from flying back or clogging the insulation between the bars or disks. There are no fewer than five pairs of brushes, the tendency to sparking being thereby greatly reduced. The figure does not show the structure of the armature itself, nor indicate the means taken to suppress Foucault currents. The core of the armature is made of very thin disks of iron,* separated, by mica or asbestos paper, from each other, and clamped together. Some exception may be taken to the use of such stout copper bars, as being more likely to heat from local currents than would be the case if bundles of straps, or laminæ of copper were substituted. And, indeed, the presence of the 4-horse power fan to cool the armature, is suggestive that continuous running is liable to heat the armature.

Before passing on from the subject of armatures, it is worth while to mention the peculiarity of form of the Bürgin armature, which has already been spoken of as consisting of eight, or in the newest Bürgin machines, as constructed by Mr. R. E. Crompton, of ten rings, set side by side.† Each ring is made of a hexagonal coil of iron wire, mounted upon light metal spokes, which meet the corners of the hexagon. Over this hexagonal frame six coils of covered copper wire are wound, being thickest at the six points intermediate between the spokes, thus making up the form of each ring to nearly a circle. Each of the six coils is separated from its neighbor, and each of the ten rings is fixed to the axis $\frac{1}{60}$ of the circumference in advance of its neighbor, so that the 60 separate coils are, in fact, arranged equidistantly (and symmetrically, as viewed from the end) around the axis. There is a 60 part collector, each bar of which is connected to the end of one coil and to the beginning of the coil that is one-sixtieth in advance; that is, to the correspondirg coil of the next ring. This armature has the great practical advantages of being easy in construction, light, and with plenty of ventilation.

In the Elphinstone-Vincent dynamo there is a drum-armature of a somewhat distinct order, the separate coils being made of a rectangular form, and then laid upon the sides of a hollow papier-maché drum in an overlapping manner, and curved to fit it. The field is a complex one, with six eternal and six internal poles, and is very intense, owing to the proximity of these poles. The parallelogram-shaped coils are connected together so as to work as three machines, and feed three pairs of brushes; which may again be united, either tn series or in parallel, or may be used to feed three separate circuits.

Collectors.

In section *ab* of the first lecture, the main points to be observed in the construction of collectors are enumerated. Collectors of substantially such type as there described are common to all dynamos of the first class, except only the Brush dynamo, in which there is a multiple commutator, instead of a collector. The collector of Pacinotti's early machine differed only in having the separate bars alternately a little displaced longitudinally along the cylinder, but still so that the same brush could slip from bar to bar. Niaudet's modification, in which the bars are radially attached to a disk, is a mere variety in detail, and is not justified by successful adoption. In the collector, as used in Weston's dynamo, and in some forms of Schuckert's dynamo, the bars are oblique or curved, without, however, any other effect than that of prolonging the moment during which the brush, while slipping from contact

* Disks of thin iron for a similar purpose are found in the dynamos of Jablochkoff, Weston, and Gülcher.

† The *Bürgin* armature exhibited, was kindly lent by Messrs. R. E. Crompton & Co.

with one bar to contact with the next, short-circuits one section of the coil.

In a well-arranged dynamo of the first class, the sections of the collector are traversed by currents, which run from the negative brush in two directions round the successive coils, and meet at that bar of the collector which touches the positive brush. Each section of the coil thus traversed adds its own electromotive force to the current passing through it. Consequently, if one measures the difference of potential between the negative brush and the successive bars of the collector, one finds that the potential increases regularly all the way round the collecting cylinder, in both directions, becoming a maximum at the opposite side where the positive brush is. This can be verified by connecting one terminal of a voltmeter to the negative brush, and touching the rotating collector at different points of its circumference with a small metallic brush or spring attached by a wire to the other terminal of the voltmeter. If the indications thus obtained are plotted out round a circle corresponding to the circumference of the collector, the values give a curve like that shown in Fig. 31, which is plotted out from observations taken upon a Gramme dynamo.*

Fig.31

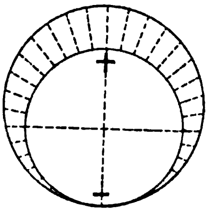
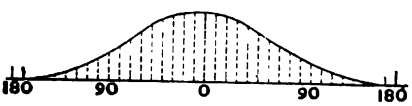


DIAGRAM OF POTENTIAL ROUND THE COLLECTOR OF GRAMME DYNAMO.

Fig.32



HORIZONTAL DIAGRAM OF POTENTIALS AT COLLECTOR OF GRAMME DYNAMO.

It can be seen that, taking the negative brush as the lowest point of the circle, the potential rises perfectly regularly to a maximum at the positive brush. The same values are also plotted out as ordinates upon a horizontal line in Fig. 32. This form of diagram shows very clearly that the rise of potential is not equal between each pair of bars, otherwise the curve would consist merely of two oblique straight lines, sloping right and left from the central point. On the contrary, there is very little difference of potential between the collector bars close to the + brush on its right and left respectively. The greatest difference of potential occurs where the curve is steepest, at a position nearly 90° from the brushes, in fact, at that part of the circumference of the collector which is in connection with the coils that are passing through the position of best action. Were the field perfectly uniform, the number of lines of force that pass through a coil ought to be proportional to the sine of the angle which the plane of that coil makes with the

Fig.33

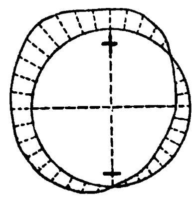
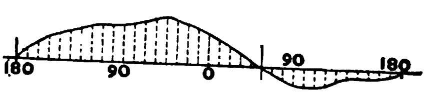


DIAGRAM OF POTENTIAL ROUND THE COLLECTOR OF A BADLY-ARRANGED DYNAMO.

Fig.34



HORIZONTAL DIAGRAM OF POTENTIALS AT COLLECTOR OF FAULTY DYNAMO.

* This diagram was plotted for me, and the measurements made at my request, by Mr. W. M. Mordey, who first drew my attention to some of the abnormal phenomena mentioned later.

resultant direction of the lines of force in the field, and the rate of cutting the lines of force should be proportional to the cosine of this angle. Now the cosine is a maximum when this angle = 0°; hence, when the coil is parallel to the lines of force, or at 90° from the brushes, the increase of potential should be at its greatest—as it is very nearly realized in the diagram of Fig. 32, which, indeed, is very nearly a true "sinusoidal" curve. Such curves, plotted out from measurements of the distribution of potential at the collector, show not only where to place the brushes to get the best effect, but enable us to judge of the relative "idleness" or "activity" of coils in different parts of the field, and to gauge the actual intensity of different parts of the field while the machine is running. If the brushes are badly set, or if the pole-pieces are not judiciously shaped, the rise of potential will be irregular, and there will be maxima and minima of potential at other points. An actual diagram, taken from a dynamo in which these arrangements were faulty, is shown in Fig. 33, and again is plotted horizontally in Fig. 34; from which it will be seen, not only that the rise of potential was irregular, but that one part of the collector was more positive than the positive brush, and another part more negative than the negative. The brushes, therefore, were not getting their proper difference of potential; and in part of the coils, the currents were actually being forced against an opposing electromotive force.

I believe that this method of plotting the distribution of potential round the collector will prove very useful in practice, and will explain various puzzling and anomalous results found by experimenters who have not known how to explain them. In a badly-arranged dynamo, such as that giving such a diagram as Fig. 33, a second pair of brushes, applied at the points showing maximum and minimum potential, could draw a good current without interfering greatly with the current flowing through the existing brushes! In fact, I find that this bad distribution, giving rise to anomalous maxima and minima, has actually been patented by a gentleman of the name of Kennedy,* who puts brushes on *six* different points of a collector!

* British patent, No. 1,640 (1882).

Curves, similar to those given, can be obtained from the collectors of any dynamo of the first class—Gramme, Siemens, Edison, &c.—saving only from the Brush machine, which, having no such collector, gives diagrams of quite a different kind. It is, of course, not needful in taking such diagrams that the actual brushes of the machine should be in contact, or that there should be any circuit between them, though in such cases the field magnets must be separately excited. It should also be remembered that the presence of brushes, drawing a current at any point of the collector, will alter the distribution of potential in the collector; and the manner and amount of such alteration will depend on the position of the brushes, and the resistance of the circuit between them.

The Brush Dynamo.

Before passing on to the dynamos of the second class, I have some remarks to make on that much mis-understood and mis-described machine, the Brush dynamo. Its armature—a ring in form, not entirely overwound with coils, but having projecting teeth between the coils like the Pacinotti ring—is unique. Though it thus resembles Pacinotti's ring, it differs more from the Pacinotti armature than that armature differs from those of Siemens, Gramme, Edison, Bürgin, &c.; for in all those the successive sections are united in series all the way round, and constitute, in one sense, one continuous bobbin. But in the Brush armature there is no such continuity. The coils are connected in pairs, each to that diametrically opposite it, and carefully isolated from those adjacent to them. For each pair of coils there is a separate commutator, so that, for the ordinary ring of eight coils, there are four distinct commutators side by side upon the axis—one for each pair of coils. The brushes are arranged so as to touch at the same time the commutators of two pairs of coils, but never of two adjacent pairs; the adjacent commutators being always connected to two pairs of coils that lie at right angles to one another in the ring. The arrangement is best studied graphically

from the diagram given in Fig. 35.* In this figure, the eight coils are numbered as four pairs, and each pair has its own commutator, to which pass the outer ends of the wire of each coil, the inner ends of the two coils being united across to each other (not shown in the diagram). In the actual machine, each pair of coils, as it passes through the position of least action (*i. e.*, when its plane is at right angles to the direction of the lines of force in the field, and when the number of lines of force passing through it is a *maximum*, and the rate of change of these lines of force a *minimum*) is cut out of connection. This is accomplished by causing the two halves of the commutator to be separated from one another by about one-eighth of the circumference at each side. In the figure it will be seen that the coils marked 1_1 1_1 are "cut out." Neither of the two halves of the commutator touches the brushes. In this position, however, the coils 3_1 3_1, at right angles to 1_1 1_1, are in the position of best action, and the current powerfully induced in them flows out of the brush marked A, (which is, therefore, the negative brush) into that marked A′. This brush is connected

Fig. 35

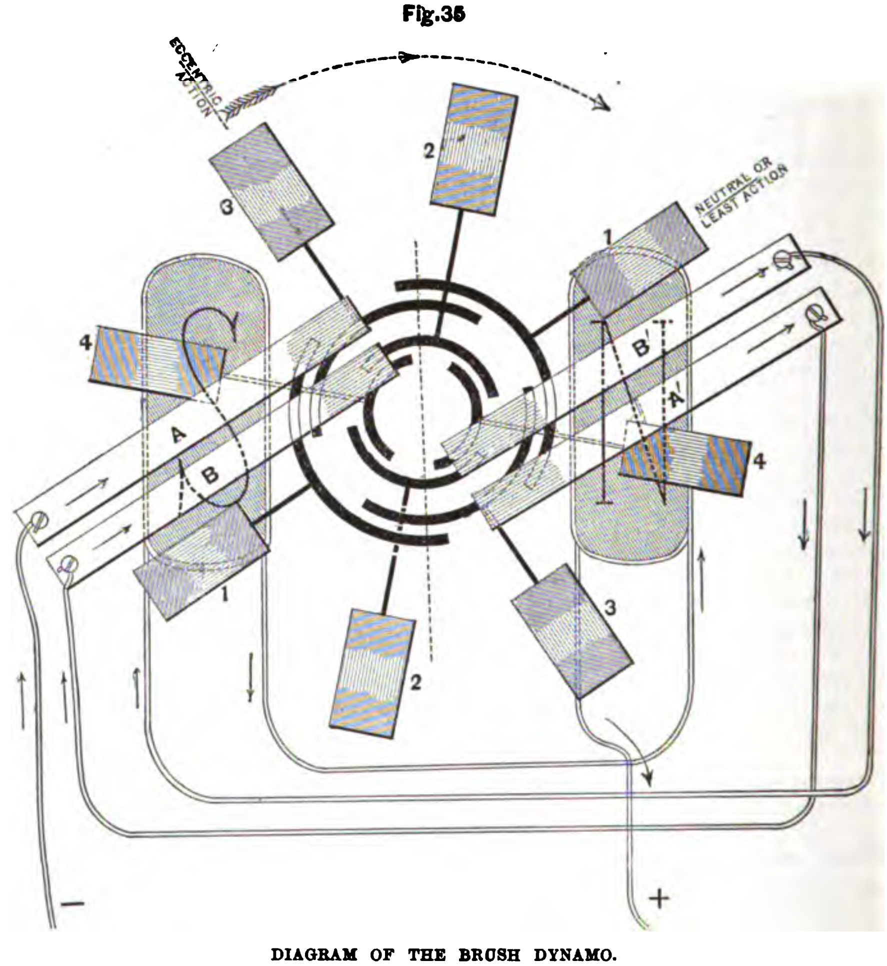


DIAGRAM OF THE BRUSH DYNAMO.

* This diagram I have constructed somewhat on the lines of a working model kindly lent me by Mr. Percy Allen, of the Anglo-American Corporation; which, however, was designed to show the action of larger dynamo, having twelve coils on the ring and six commutators.

across to the brush marked B, where the current re-enters the armature. Now, the coils 2, 2, have just left the position of best action, and the coils 4, 4, are beginning to approach that position. Through both these pairs of coils, therefore, there will be a partial induction going on. Accordingly, it is arranged that the current, on passing into B, splits, part going through coils 2, 2, and part through 4, 4, and reuniting at the brush B′, whence the current flows round the coils of the field-magnets to excite them, and then round the external circuit, and back to the brush A. (In some machines it is arranged that the current shall go round the field-magnets after leaving brush A′, and before entering brush B; in which case the action of the machine is sometimes, though not correctly, described as causing its coils, as they rotate, to feed the field-magnets and the external circuit alternately). The rotation of the armature will then bring coil 2, 2, into the position of least action, when they will be cut out and the same action is renewed with only a slight change in the order of operation. The following table summarizes the successive order of connections during a half revolution:

First position. (Coils 1 cut out.)

$\mathrm{A}-3-\mathrm{A};\ \mathrm{B}<\genfrac{}{}{0pt}{}{4}{2}>\mathrm{B};$

Field-magnets—External circuit—A.

Second position. (Coils 2 cut out.)

$\mathrm{A}<\genfrac{}{}{0pt}{}{1}{3}>\mathrm{A};\ \mathrm{B}-4-\mathrm{B};$

Field magnets—External circuit—A.

Third position. (Coils 3 cut out.)

$\mathrm{A}-1-\mathrm{A};\ \mathrm{B}<\genfrac{}{}{0pt}{}{2}{4}>\mathrm{B};$

Field-magnets—External circuit—A.

Fourth position. (Coils 4 cut out.)

$\mathrm{A}<\genfrac{}{}{0pt}{}{3}{1}>\mathrm{A};\ \mathrm{B}-2-\mathrm{B};$

Field-magnets—External circuit—A.

From this it will be seen that whichever pair of coils is in the position of best action, is delivering its current direct into the circuit; whilst the two pairs of coils which occupy the secondary positions are always joined in parallel the same pair of brushes touching the respective commutators of both.

One consequence of the peculiar arrangement thus adopted is, that measuring the potentials round one of the commutators with a voltmeter gives a wholly different result from that obtained with other machines. For one-eighth of the circumference on either side of the positive brush, there is no sensible difference of potential. There then comes a region in which the potential appears to fall off; but the falling off is here partly due to the shorter time during which the adjustable brush connected with the voltmeter and the fixed positive brush are both in contact with the same part of the commutator. Further on there is a region in which the voltmeter gives no indications corresponding to the cut out position; and again, on each side of the negative brush, there is a region where the polarity is the same as that of the negative brush. Fig. 36 is a diagram of a 6-light Brush

Fig. 36

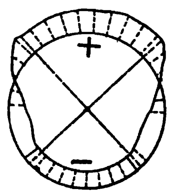


DIAGRAM OF POTENTIALS AT COMMUTATOR OF BRUSH DYNAMO.

taken at one commutator, the main + brush being, however, allowed to rest (as in its usual position), in contact with both this commutator and the adjacent one.

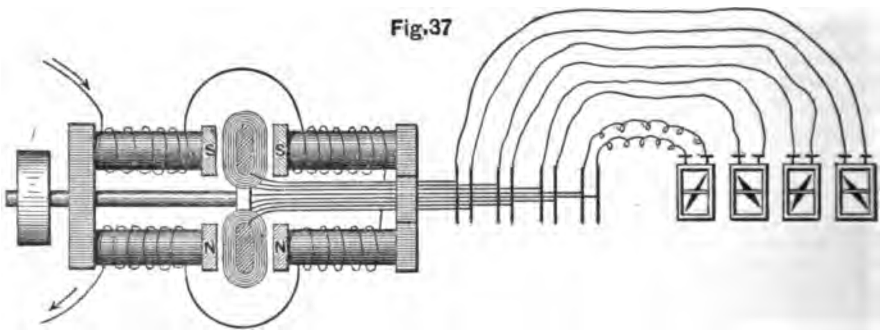
From the foregoing considerations, it will be clear that the four pairs of coils of the Brush machine really constitute four separate machines, each delivering alternate currents to a commutator, which commutes them to intermittent unidirectional currents in the brushes; and that these independent machines ar ingeniously united in pairs by the devic of letting one pair of brushes prese against the commutators of two pairs os coils. Further, that these paired maf chines are then connected in series, by bringing a connection round from brush A′ to brush B.

The following experiment illustrates the independence of the four pairs of

coils in the Brush dynamo.* The usual commutators of a small Brush dynamo were removed and replaced by eight brass collars, to each of which was united one end of one of the four pairs of coils. Against these rings pressed eight separate brushes, and the circuit of each of the four pairs of coils was completed, as shown in Fig. 37, through an ordinary detector galvanometer. On rotating the armature by hand, at a moderate speed, the needles of all four detectors are set vibrating to and fro by the alternate currents, not synchronously, but one after the other. If any of the four circuits are broken, the others go on as before.

circuit, is furnished with the radial collector mentioned above. In the Wallace-Farmer dynamo is very nearly realized the condition of field of Fig. 13, there being a pair of poles at the top arranged so that the north faces the south pole, and another pair at the bottom where the south faces the north pole. The coils are carried round, their axis being always parallel to the axis of rotation, upon a disk; there being two sets of coils on opposite faces of two disks of iron set back to back. They are united precisely as in Niaudet's dynamo, and each disk has its own collector. Each bar of the collector is, moreover, connected, as in

Fig. 37



BRUSH DYNAMO ARRANGED WITH 4 DETECTOR GALVANOMETERS SHOWING ALTERNATE CURRENT INDUCED IN INDEPENDENT PAIRS BY COILS.

Dynamos of the Second Class.

I now pass on to dynamos of the second class, in which coils are carried round to different parts of a magnetic field, whose intensity differs in different regions, or one, in different parts of which the lines of force run in opposite directions. Fig. 13 of my first lecture illustrated this principle; and we shall now consider how it is carried out in practice. In the early machine of Pixii a single pair of coils was mounted so as to pass in this fashion through parts of the field where the magnetic induction was oppositely directed. Such a machine, therefore, gives alternate currents, unless a commutator be affixed to the rotating axis. Niaudet's dynamo, which may be regarded as a compound Pixii mcahine, having the separate armature coils united as those of Gramme and Siemens into one continuous

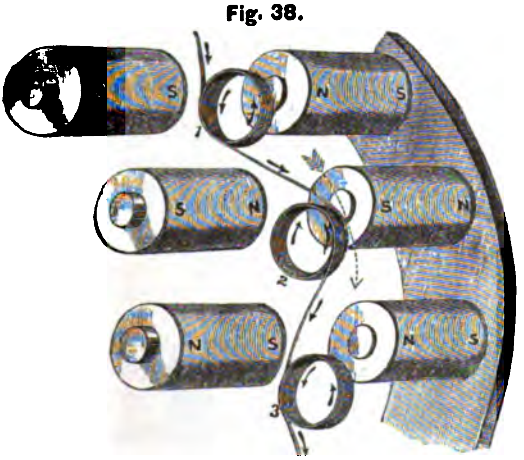
the dynamos of Pacinotti, Gramme, Siemens, &c., with the end of one coil, and the beginning of the next. In fact, the Wallace-Farmer machine is merely a double Niaudet dynamo with the cylindrical collectors. There is a serious objection to the employment of solid iron disks such as these. In a very short time they grow hot from the eddying Foucault currents engendered in them as they rotate. This waste reduces the efficiency of the dynamo. In the dynamo of Hopkinson and Muirhead, the disk-armature takes a more reasonable shape. Instead of a solid disk of iron to support the coils, there is a disk built up of a thin iron strip wound spirally round a wooden center. The coils, of approximately quadrangular shape, and flat form, are wound upon the sides of this compound disk. The dynamo of Ball (the so-called "Arago-disk" machine) is similar in many respects, but has no iron cores to the armature coils.

* This beautiful experiment was first shown me by Mr. P. Allen, who kindly fitted up the little Brush dynamo with the eight collars for this occasion.

ALTERNATE-CURRENT DYNAMOS.

But by far the most important of the dynamos of this second class are those usually known as *alternate-current machines.* This type of dynamo was originally created by Wilde, in 1867. The field magnets, consist of two crowns of fixed coils, with iron cores, arranged so that their free poles are opposite to one another, with a space between them sufficently wide to admit the armature. The poles taken in order round each crown are alternately of north and south polarity, and opposite a north pole of one crown faces a south pole of the other crown. This description will apply to the magnets of the alternate current machines of Wilde and Siemens, to the so-called Ferranti machine, and, with certain reservations, to the machines of Lachausée and of Gordon. The armatures in almost all machines of this type con-

Fig. 38.



PRINCIPLE OF ALTERNATE-CURRENT DYNAMOS.

sists of a *disk,* bearing at its periphery a number of coils, whose axes are parallel to the axis of rotation. The principle will be best understood by reference to Fig. 38, which gives a general view of the arrangement. Since the lines of force run in opposite directions between the fixed coils, which are alternately S—N, N—S, as described above, the moving coils will necessarily be traversed by alternating currents; and as the alternate coils of the armature will be traversed by currents in opposite senses, it is needful to connect them up, as shown in the figure, so that they shall not oppose one another's action.

In Wilde's dynamo, the armature coils have iron cores, and the machine is provided with a commutator on the same principle as that used by Jacobi in his famous motor of 1838, consisting of two metal cylinders, cut like crown wheels, having the teeth of one projecting between those of the other, so that the brushes make contact against them alternately as they rotate. The brushes are, of course, fixed, so that they do not both touch the same part. This commutator Wilde usually applied to a few, or only one, of the rotating coils, and utilized the current thus obtained to magnetize the field-magnets. The main current was not so commuted, but was led away from a simple collector, consisting of two rings connected to the two ends of the armature circuit, each being pressed by one brush.

Siemens prefers to use a separate direct current machine to excite the field-magnets of alternate-current dynamo. In the armature of the latter the coils are wound usually without iron, upon wooden cores. In some forms of the machine, the individual coils are enclosed between perforated disks of thin German-silver. When currents of great strength are required, but not of great electromotive force, the coils are coupled up in parallel arc, instead of being united in series.

In a dynamo by Lachausée, which very strikingly resembles the preceding one, there is iron in the cores of the rotating coils. But the main difference is that the rotating coils are the field magnets, excited by a separate Gramme dynamo, whilst the coils, which are fixed in two crowns on either side, act as armature coils in which currents are induced.

Gordon's dynamo, the largest yet constructed, is constructed on the same lines as the Lachaussée machine; but with many important improvements. In the first place, there are twice as many coils in the fixed armatures as in the rotating magnets, there being 32 on each side of the rotating disk, or, in all, 64 moving coils; while there are 64 on each of the fixed circles, or 128 stationary coils in all. The latter are of an elongated shape, wound upon a bit of iron boiler-

plate, bent up to an acute V form, with checks of perforated German silver as flanges. The object of thus arranging the coils, so that the moving ones shall have twice the angular breadth of the fixed ones, is to prevent adjacent coils of the fixed series from acting detrimentally, by induction, upon one another. The alternate coils of the fixed series are united together in parallel arcs, so that there are two distinct circuits, in either or both of which lamps can be placed; or they can be coupled up together. Great care appears to have been taken, in the construction of this large machine, to guard against the appearance of Foucault currents, by arranging the cores, frames, and coils, so that all metal parts of any size shall be slit, or otherwise structurally divided at right-angles to the direction of the induced electromotive forces.

Fig. 39.

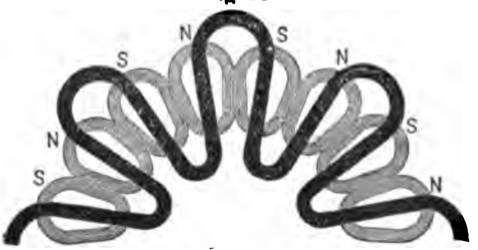
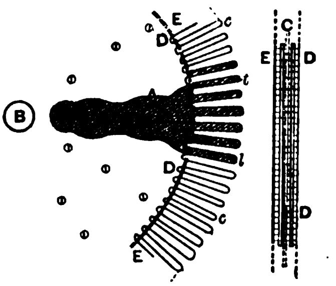


DIAGRAM OF FERRANTI'S ALTERNATE-CURRENT DYNAMOS.

Yet another alternate-current dynamo, identical in many respects with the Siemens alternate-current dynamo, has lately been brought out, under the name of the Ferranti machine. As in the machines of Wilde and Siemens, the electromagnets form two crowns with opposing poles. The point of difference is the armature, which, like that of Siemens, has no iron cores in his coils; but which, unlike that of Siemens, is not made up of coils wound round cores, but consists of zigzags of strip copper folded upon one another. There are eight loops in the zigzag (as shown in Fig. 39,) which depicts half only of the arrangement, and on each side are sixteen magnet poles; so that, as in Gordon's dynamo, the moving parts are twice the angular breadth of the fixed parts.

The advantage of the armature of zigzag copper lies in its simplicity of construction. Sir W. Thomson, who is the real inventor of this armature, proposed originally that the copper strips should be wound between projecting teeth on a wooden wheel, as indicated Fig. 40, in

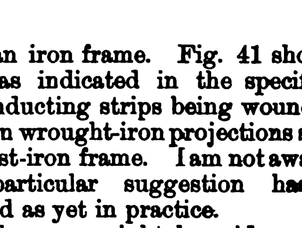
Fig. 40.



SIR W. THOMSON'S ZIGZAG WINDING OF ARMATURE FOR ALTERNATE-CURRENT DYNAMO.

which is taken from the drawings of his British patent of December, 1881. He also proposed to use as field-magnets a form of electro-magnet of the kind known as Roberts, and also used by Joule, in which the wires that bring the exciting current are passed up and down, in a zigzag form between iron blocks projecting

Fig. 41.



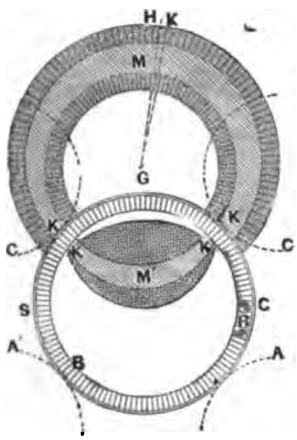
SIR W. THOMSON'S PROPOSED FIELD MAGNETS.

from an iron frame. Fig. 41 shows the form, as indicated in the specification, the conducting strips being wound round between wrought-iron projections screwed to a cast-iron frame. I am not aware that this particular suggestion has been adopted as yet in practice.

Much more might be said concerning the two machines last described—the Gordon and the Thomson dynamos—but

time does not permit me to dwell longer upon them. And, indeed, I am not at all convinced that this type of machine, though at present it appears to be the fashionable one, is destined to prove of such very great value, simply because I doubt whether any dynamo that yields alternate currents can compete with continuous-current machines. For the purposes of a general system of distribution, where more than one dynamo must be available, and also for the purpose of supplying motors, alternate-current machines are quite out of the question. I will not therefore dwell longer upon them, than merely to remark that, besides the disk armatures now described, pole armatures have been employed in

Fig. 42.



SIR W. THOMSON'S "MOUSE-MILL" DYNAMO.

alternate-current machines by Gramme, Jablochkoff, and by Lontin. Hefner Alteneck has gone a stage further, and, by the device of employing a disk armature in which the number of coils differed by two, or some other even number, from those of the field, and by the employment of a multiple-bar collected with complicated cross connections, has succeeded in converting this type of dynamo into a continuous-current machine.

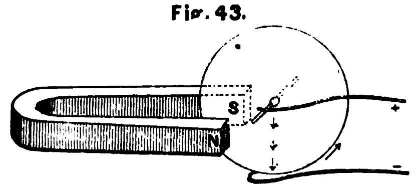
Thomson's "Mouse-mill" Dynamo.

One other dynamo, not belonging to the type I have been dealing with, is worthy of mention. This is Sir W. Thomson's "mouse-mill" dynamo, shown in Fig. 42, in diagram. I have noticed, *en passant*, several points in this machine—the form of its field-magnets and their coils, the internal electro-magnets, etc. The armature is a hollow cylinder, S S, made up of parallel copper bars, arranged like the bars of a mouse-mill (whence the name of the machine). These bars are insulated from each other, but are connected all together at one end. At the other they serve as collector-bars, and deliver up the currents generated in them to the "brushes," which here are rotating disks of springy copper shown as dotted circles at C C in the figure. As the armature is a hollow barrel, with fixed electro-magnets within, it cannot be rotated on a spindle, but runs on friction rollers, A A′, by one or more of which it is driven.

Dynamos of the Third Class.

I now come to the third class of dynamos—those in which rotation of a conductor effects a continuous increase in

Fig. 43.



BARLOW'S WHEEL.

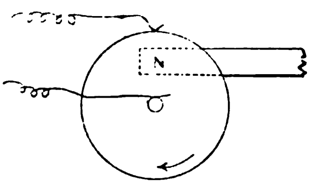
the number of lines of force cut by the device of arranging one part of the conductor to slide on or round the magnet.

The earliest machine which has any right to be called a dynamo was, in fact, of this class. Barlow and Sturgeon had shown that a copper disk, placed between the poles of a magnet, Fig. 43 rotates in the magnetic field when traversed by an electric current from its axis to its periphery, where there is a sliding contact. Faraday, in 1831, showed that by rotating a similar disk mechanically between the poles of a magnet continuous currents were obtained. These he drew off by collecting springs of copper or lead, one of which touched the axis (see Fig. 44), whilst the other pressed against the amalgamated periphery. He was thus "able to construct a new electrical machine."*

* "Experimental Researches," § 83.

"Here, therefore, was demonstrated the production of a permanent (*i. e.* continuous) current of electricity by ordinary magnets." But Faraday did not stop short with ordinary magnets; he went on to employ the principle of separate excitement of his field-magnets. "These effects were also obtained from *electro-magnetic poles*, resulting from the use of copper helices or spirals, either alone or with iron cores. The directions of the motions were precisely the same; but the action was much greater when the iron cores were used, than without."* The invention of the dynamo dates, therefore, from 1831, and Faraday was its inventor, though he left to others to reap the fruits of his splendid discovery.† Such a machine, however, is impracticable, for several reasons; the peripheral friction is inadmissible on any but a small scale; moreover, the disposition of the field-magnets necessarily evokes

Fig. 44.



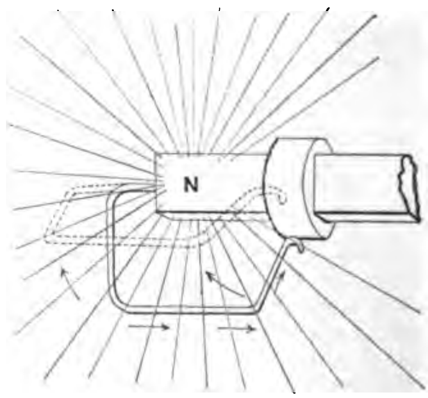
FARADAY'S DISK DYNAMO.

wasteful eddy-currents in the disk, which, even if slit radially, would not be an appropriate form of armature for such a limited magnetic field.

Another method of obtaining a continuous cutting of the lines of force, is indicated in Fig. 45, where a sliding conductor travels round the pole of a magnet. Faraday even generated continuous currents by rotating a magnet with a sliding connection at its center, from which a conductor ran round outside, and made contact with the end-pivots which supported the magnet.

A similar arrangement was devised by Mr. S. Alfred Varley, about the year 1862. He rotated an iron magnet in a vertical frame, having a mercurial connection at the center. The current which flowed from both ends of the magnet toward the center was, in this machine—which, by the kindness of Mr. Varley, I am able to exhibit to you to night—made to return to the machine, and to pass through coils surrounding the poles of the rotating magnet; thus anticipating the self-exciting principle of later date. Mr. Varley also proposed to use an external electro-magnet to increase the action.

Fig. 45.



ROTATION OF CONDUCTOR ABOUT POLE OF MAGNET.

Quite recently, the same fundamental idea has been worked upon by Messrs. Siemens and Halske, who have produced a so-called "unipolar" machine.* In this remarkable dynamo there are two cylinders of copper, both slit longitudinally to obvitate eddy-currents, each of which rotates round one pole of a U-shaped electro-magnet. A second electro-magnet, placed between the rotating cylinders, has protruding pole-pieces of arching form, which embrace the cylinders above and below. Each cylinder, therefore, rotates between an internal and an external pole of opposite polarity, and

* "Experimental Researches," § 111.

† "Experimental Researches," § 158:—"I have rather, however, been desirous of discovering new facts and new relations dependent on magneto-electric induction, than of exalting the force of those already obtained; being assured that the latter would find their full development hereafter." Can any passage be found in the whole range of science more profoundly prophetic, or more characteristically philosophic, than these words, with which Faraday closed this section of his researches?

* This sounds like a *locus a non lucendo*, for the machine has two poles. But the name is derived from the term "unipolar induction," which Continental electricians give to the induction of currents by the process of "continuous cutting," which we are now dealing with. I do not adopt the term, as it is needlessly mystifying.

consequently cuts the lines of force continuously by sliding upon the internal pole. The currents from this machine are of very great strength, but of only a few volts of electromotive force. To keep down the resistance, many collecting brushes press on each end of each cylinder. This dynamo is actually at work for electro-plating.

The only other dynamo of this class, of which I have seen any published notice, is one recently patented by Mr. E. L. Voice, in which a coil armature, wound upon an iron ring, is so placed that the iron ring is itself one pole of a magnet, a projecting pole-piece from the other pole being fixed near it, so that the coils fixed upon one pole glide round and cut the lines of force proceeding from the other pole. Whether this machine will be a practical one remains to be seen.

We are, however, far from having arrived at finality in the design and construction of dynamo-electric machines. All we can yet say is, that we appear to be approaching the time when practice will no longer be a species of blundering along into success or failure. For every-day theory is more brought to bear in practice, and will soon enable us to predict with certainty beforehand what will be the merit of a dynamo of any particular design; and even to say not only what its cost will be, and what its efficiency and maximum duty, but also how many volts of electromotive force, and how many ampères of current it will put at our disposal. In short, the application of theory in the manufacture of dynamos must rapidly lead to great and substantial improvements in the dynamo in practice.

NORWEGIAN GEODETICAL OPERATIONS.*

From "Nature."

In 1861 an Association was formed, under the auspices of Lieut.-General von Baeyer, having for its object the measurement of arcs of meridians, and parallels, in Europe. Most of the Continental nations joined this Association, and have carried out triangulations and spirit levellings of precision to further the objects in view. It is the intention of the Association to measure an arc extending from Palermo to Levanger in Norway, which will, however, probably be extended to the North Cape. The work before us is the report of the measurement of two base lines, and of their connection with the Norwegian triangulation which is to form part of the measurement of the above-mentioned arc. It was thought in 1862 that the existing Norwegian triangulation, supplemented and verified by some new work, would meet the requirements of the Association; but it was found, on investigation, that such was not the case, and moreover that the verifications could not be carried out, because the old trigonometrical stations could not be refound with any certainty. It was, therefore, decided to commence a new triangulation extending in a chain from the Swedish frontier (south of Christiana), where the chain is connected with the Swedish triangulation, to Levanger, where again a connection is to be made with another portion of the Swedish triangulation. The two base lines already mentioned are situated at the extremities of this chain of triangles, one at Egeberg, near Christiana, and the other at Rindenleret, near Levanger; both were measured during the summer of 1864, and Part I. is the report of these measurements.

The base measuring apparatus used is similar to that employed by Struve for the measurement of several base lines in Russia; it belongs to the Swedish Government, and was used for the measurement of their base lines. The apparatus consists of four cast-iron tubes, each approximately 2 toises† in length: One end of each tube is fitted with a small highly polished steel stud, and the other end with a "contact lever." The short arm of the contact lever terminates in a steel stud, which is intended to press

* Publications of the Norwegian Committee of the European Association for the Measurement of Degrees. Geodetical Operations. Published in Three Parts. (Christiana, 1880 and 1882.)

† A toise is 2.13151116 yards as determined by Col. A. R. Clarke, C.B., R.E., F.R.S., &c.

against the fixed stud of the adjoining tube; the long arm moves on a scale. A measuring rod capable of varying its length to a slight extent is thus obtained, and this alteration in length can be measured with great delicacy, since the long arm of the lever greatly exaggerates it. This arrangement insures that the pressure between the rods is constant. Each tube is provided with two thermometers, the bulbs of which are bent nearly at right angles to the stem, and are inserted into small holes in the tubes. In order to protect the tubes as far as possible from changes of temperature they are wrapped round with several thicknesses of cloth, and are further inclosed in a wooden box, out of which the two ends of tube just project. During the measurement of a base line each rod is supported on two trestles, at one-fourth and three-fourths of its length, provided with screw arrangements giving slow motions laterally and in elevation. The rods are not, however, accurately levelled, and a correction has to be made for dislevelment. To measure the small angle of inclination each rod is fitted with a very sensitive level. One end of the level works on trunnions, the other is connected to a micrometer screw by means of which the level can be raised or lowered. The bed of the level is attached to the top of the box, but in such a manner that it can be adjusted truly parallel to the tube. The value of each micrometer division was determined by means of the meridian circle in the observatory at Christiana. It will be seen from the above that, as the measurement of a base line proceeds, the following readings are required for each rod: (1) the contact lever; (2) the thermometers; (3) the micrometer for inclination. These readings were taken and booked independently by two observers. Both base lines were measured twice, once in each direction.

Before and after the measurement of each base line each rod was compared with a *standard* rod, the exact length of which was known, namely:

$$=1727.96641\ (1+0.000011476\ (t-16^{\circ}.25))\pm 0.00058$$

expressed in Paris lines* based on Bessel's toise, t being expressed in degrees Centigrade. It was found that the rods were slightly dimished in length during the measurement of a base line (on an average 0.005 lines) owing to abrasion. An allowance was made for this diminution in length. The apparatus with which these comparisons were made consists of a massive cast-iron beam, turned up at both ends, and carrying two supports fitted with rollers upon which the rod to be measured rests. One end of this beam is fitted with a fixed steel stud, against which the contact lever of the rod under comparison bears; the other end carries a sliding scale, connected with a contact lever, and read by means of a micrometer microscope. A set of readings consisted in first measuring the standard rod, then each of the four measuring rods in succession, and lastly the standard rod again: the temperature of each rod was carefully noted. For a complete comparison twelve such sets of readings were taken.

The time occupied in measuring the Egeberg base was 18 days, and the observations for each measuring rod occupied 4 minutes; the Rindenleret base was measured more rapidly, namely, $2\frac{1}{2}$ minutes per rod, due to the site being more level.

A considerable portion of Part I. is taken up in considering the errors to which the measurements of these base lines are liable, in estimating the allowances to be made to correct these errors and in computing the probable errors of the final results. These errors are due: (1) to errors of observation in the actual measurement of the base lines; (2) to the error in the adopted length of the measuring rods.

Firstly, the errors to which the actual measurement of a base line is liable are as follows:

A slight uncertainty attaches to the micrometer readings of the levels measuring the inclination of the rods. The probable error is computed to be

Egeberg base............. ± 0.350 lines
Rindenleret base.......... ± 0.183 "

The errors due to the contact levers are next considered. It is shown that the error caused by the small uncertainty in the value of a degree of the scale over which the long arm of the lever

* A Paris line is defined by 1 Paris line $=\frac{1}{864}$ toise; hence 1 Paris line $=0.08813$ English inch.

moves, is too small to be taken into account, but the error caused by uncertainties in reading the scale is of sensible amount, and is computed to be

Egeberg base.............± 0.015 lines
Rindenleret base..........± 0.014 "

Further, the surface of the steel studs, at the end of the rods, is a portion of a sphere whose radius is considerably less than the length of a rod. Hence an error will occur each time a contact lever does not touch at the center of the stud, that is if it makes an eccentric contact, and although every care was taken to obtain accurate contacts, it is considered that a correction of the following amounts should be made—

Egeberg base......-0.351 ± 0.175 lines
Rindenleret base...-0.314 ± 0.157 "

The next source of error is that due to errors in alignment, these errors will always be negative, and are due to the uncertainty in placing the rods in the line given by the directing theodolite. This error is computed to amount to

Egeberg base......-0.294 ± 0.101 lines.
Rindenleret base...-0.262 ± 0.090 "

The computed variation of length of the rods due to alterations in temperature is vitiated by several errors. In the first place, the coefficient of expansion of the rods, as determined by Prof. Lindhagen, is affected by the small uncertainty, 0.000000015. Further, the correction for expansion is computed on the supposition that the thermometers do actually indicate the mean temperature of the rods at the time of taking the readings; but this is an assumption. and in fact it is estimated that the temperature indicated by the thermometers is the temperature the rod had 20.0 ± 5.9 minutes before taking the reading. This estimate is arrived at as follows: It will be remembered that each base line was measured twice; the difference between the two measurements is due to the various errors under consideration, and its probable value can, therefore, be computed; this computed value will contain as an unknown, the time of which an estimate is required. Hence, by equating the computed difference to the actual difference the time can be found. The total error in the allowance made for expansion is found to be

Egeberg base.........$\pm 0.085 + 0.525 \wedge$
Rindeleret base.......$\pm 0.071 + 0.250 \wedge$

where $\wedge = 20. \pm 5.9$ minutes.

Secondly, the errors due to the uncertainty in the accepted length of the rods are considered under four heads, namely: (1) the error in the length of the standard rod; (2) the error due to the bending of the beam of the comparing apparatus (some experiments were made to obtain data for the calculation of this error); (3) the error in comparing the rods with the standard; (4) the error due to the assumption that the diminution in length of the rods by abrasion is proportional to the length of time in use. The probable error of the accepted length of a rod during the measurement of the Egeberg base is computed to be $\pm$ 0.00081 lines, and during the measurement of the Rindenleret base $\pm$ 0.00071 lines.

Finally, the base lines had to be reduced to the sea-level; data had been obtained for this purpose by means of spirit-levelling operations. The reduction in the length of the base lines due to this cause is

Egeberg base ....-33.89 lines
Rindenleret base.........$-$ 0.852 "

Applying all these various corrections to the measured lengths of the base lines the final results are as follows:

Egeberg base........2025.28316 toises,

with a probable error of ± 0.00120, or $\frac{1}{1,570,000}$ of its length.

Rindenleret base......1806.3177 toises,

with a probable error of ± 0.00120, or $\frac{1}{1,500,000}$ of its length.

This is a high degree of accuracy as compared with older base lines (as for instance several base lines measured in France between 1798 and 1828, of which the probable errors are $\frac{1}{250,000}$); but this accuraccy has frequently been attained of late years, and even surpassed. as, for instance, the base line of Madrid-

ejos, measured by General Ibañez, in 1858, with a probable error of $\frac{1}{5,865,800}$.

Part II. is the account of the connection of the Egeberg base with the side Toass-Kolsaas, and Part III. that of the connection of the Rindenleret base with the side Stokvola-Haarskallen of the principal triangulation. The observations were made during 1864–66, but owing to an error at one of the stations, due to the bisection of a wrong object, further observations were made at that station in 1877. The connection in each case is very complete, and the work is well tied in. The centers of the trigonometrical stations were very carefully defined by letting an iron bolt into the rock, or, into a large block of stone; the center of the face of this bolt, marked by a small hole, was the trigonometrical station. The signals, to which the observations were taken, consisted of an upright beam, to which was attached one or two boards about 0.75m. square, which were painted white or black, and occasionally a vertical stripe 0.11m. broad was painted on the center of the board. At several of the stations the theodolite could be placed beneath the signal, and at such stations the signal was placed over the bolt, but in several cases, owing to the nature of the ground, or other causes, the trigonometrical station had to be placed at some distance from the signal, in one case as much as 54 Norwegian feet. In such cases the corrections to be applied to the observations were obtained by measuring a short base line, one end of which was the trigonometrical station, and the direction nearly at right angles to the line joining the station and the signal. Observations were taken from the ends of this base to the various points on the signal, which were bisected from the other stations, and these, together with the observed bearings to and from the other stations, enabled the necessary corrections to be made. The greatest correction thus required was 10′ 37″.34. But even at stations where the theodolite was placed beneath the signal, corrections were required to reduce the observations to the trigonometrical station, because different points on the signal were observed from the other stations, and these points were not vertically over the bolt. In these cases the corrections were computed in the following manner: A piece of paper, mounted on a board, was placed horizontally on the ground over the center of the station, and this center marked on it. Then, by means of a small theodolite, the "traces" of the vertical planes passing through the various points observed to, were marked in pencil on the paper. The theodolite was now shifted, and the corresponding traces marked as before; the intersections of these traces gave a series of points vertically beneath the points on the signal to which observations had been made. From these points, the corresponding bearings to the various stations were plotted on the paper; and, lastly, perpendiculars were dropped, from the point representing the center of the station, on to these bearings; the length of any one of these perpendiculars divided by the approximate distance to the corresponding station is the tangent of the correction to be applied.

Two instruments were used for measuring the angles; a 10′ universal instrument by Olsen, read by two micrometer microscopes, and a 12″ theodolite by Reichenback, read by four verniers. The errors of graduation of these instruments were investigated, and are given in a tabular form in Part II. Although, owing to the numerous observations taken to each object starting from different parts of the horizontal limb, the errors of graduation must have been eliminated to a very large extent, yet it was thought advisable to apply these corrections to the observations, in order to obtain a more accurate idea of the bearings of each station. The errors of the micrometer microscopes are also given in a table. The 10″ instrument was used at all, the 12″ theodolite appears to have only been used at two stations. A third instrument, a 10″ universal instrument by Breithaupt & Sons, was used for the observations of 1877.

When observing, the instrument was first set at 0°, and a round of angles taken; the telescope was then reversed and the round taken again. The instrument was then set at 15° in the case of the triangulation connecting the Egeberg base, and at 20° (nearly) in the case of the Rindenleret base triangulation,

and two rounds taken as before. The instrument was then again moved on 15° and 20° respectively, and so on. Thus in the first case forty-eight, and in the second thirty-six observations were taken to each station. In some few instances even a greater number were taken. The actual observations are not given in the Report, only the mean of four observations—two taken in the same position of the horizontal limb, and two in that position increased by 180°. The time occupied at each station averages four days; some stations were completed in two days.

The observations were compensated by the method enjoined by the Association for the Measurement of Degrees in Europe, namely, Bessel's method. The observed angles at each station are first compensated amongst themselves. A correction is then applied to each angle thus found, subject to the condition that the sum of the squares of these corrections for the whole triangulation is a minimum, and subject further to the geometrical conditions that the sum of the three angles of a triangle=180+spherical excess, and that the length of any side is the same by whatever *route* it is calculated. The necessary calculations are very laborious, and in the case of the Rindenleret base require the solution of simultaneous equations containing seventy-six unknowns. It is very questionable whether the result repays this labor; the method of compensation adopted for the Ordnance Survey, although perhaps not so rigid, compares favorably in this respect. The calculations for compensation are given very fully in the Report.

The Report is accompanied by plates showing the base measuring apparatus and the connecting triangulations.

THE WORTHINGTON PUMPING ENGINE AT BUFFALO, N. Y.

By JOHN W. HILL, M. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE city of Buffalo has now in use three Worthington compound pumping engines, one of ten million gallons capacity, and two of fifteen million gallons capacity each; the engine under consideration is one of the latter, and the only one of the three which was contracted for under a "duty" guarantee.

The new fifteen million Worthington engine for the City of Buffalo is one of the largest of this type ever built, and is a splendid specimen of symmetry in design and excellence of finish. Whatever may be the fault of the engine in point of economy, it certainly fails not for lack of good materials and accurate workmanship.

There was a time when the Worthington pumping engine had no successful rival for the purposes of a public water supply. It seems, however, from recent developments that this type of engine—*i. e.*, the direct acting—must in the future hope to enjoy no better than a second-rate reputation. The excellent annual "duty" records of the compound, rotative pumping engines at Lowell, Lynn, Lawrence, Pawtucket, Providence (Putaconsett Station), Memphis, Milwaukee, Chicago (West Side), and Trenton, N. J., as well as the exalted "duties" obtained upon contract trials of the Leavitt compound, rotative beam engines at Lynn and Lawrence; of the Corliss compound, rotative beam or lever engines at Pawtucket and Providence: of the Quintard compound, rotative beam engines at Chicago; of the Gaskill compound, rotative engines at Memphis; and last, but not least, of the Gaskell compound, rotative beam or lever engine at Saratoga, prove more completely and more eloquently than any argument of words that the direct-acting pumping engine for public water supply (excepting upon a very small scale) cannot hope to compete for "duty" contracts in the future.

By this it is not meant that direct-acting pumping engines, even for a large supply, may not be built and used for years to come; but in every instance where an otherwise excellent performance must be coupled with a "high duty," the direct-acting engine cannot and will not attempt to compete.

The writer (during the contract trials at Buffalo) was informed by the contractors'

representative, and fully believes, that this engine was in all respects equal to the best of Mr. Worthington's build, and that the performance of this engine is a fair reflex of the possibilities of the type, cannot be successfully gainsaid.

The Worthington pumping engine for the city of Buffalo was contracted for a daily delivery (capacity) of 15,000,000 U. S. standard gallons, and a "duty" under conditions decidedly favorable to the engine of "seventy millions."

How well the engine complied with these guarantees is fully shown by the following extract from the writer's report to the Water Commissioners:

"The contractors' detailed specification provides that the engine shall be a first-class expanding and condensing engine, with four steam cylinders of the following dimensions: High pressure cylinders, 38 in. diameter; expanding cylinder, 66 in. diameter; length of stroke, 4 ft., connected to pumps with double-acting plungers 38 in. diameter, and 4 ft. stroke, capable of raising 15,000,000 U. S. standard gallons in twenty-four hours against a pressure of 70 pounds per square inch when working at a piston speed of not more than 110 ft. per minute, and to pump against above pressure either directly through the mains or into the reservoir as required.

"The contract provides that the "duty" of said engine shall be seventy million pounds of water raised one foot high with the consumption of one hundred pounds of coal, the calculation of duty to be based upon an evaporation by the boilers of ten pounds of water for every pound of fuel consumed.

"The engine is of the well-known Worthington duplex direct-acting type, and varies in no material respect from many which have preceded it from the same builder, excepting in the addition to the high pressure cylinders of cut-off valves of the Corliss form, but worked by positive connections from the rocker shafts.

"The time of closing these valves is variable by hand, and they are designed to assist the cushion valves on the low pressure cylinder in controlling the motion of the piston toward the end of stroke, for which purpose they seem well adapted.

"The steam cylinders and the heads of the low pressure cylinders are steam-jacketed, the condensation being tropped from the jackets into a collecting well, into which the overflow from the condenser is also delivered, and from which the feed for the boilers is pumped.

"An independent steam pump taking steam from the main pipe in engine room supplies the feed water to the boilers.

"The boilers, two in number, furnishing steam to the engine are of the marine fire-box pattern, with two furnaces to each.

"The total heating surface of both boilers is approximately 3500 sq. ft., and the total grate surface 87 sq. ft., presenting a ratio of heating to grate surface of 40.23.

"The coal burned was Lehigh, of very inferior quality, judging from the percentage of refuse weighed back at the end of duty trial. The boilers proved inadequate to the demands of the engine during the capacity trial, it being found impossible to maintain the contract steam pressure of 60 pounds whilst pumping at the rate of 15,000,000 gallons per diem against the contract pressure upon pump gauge of 70 pounds.

"In making the capacity trial it was decided to maintain the contract water pressure, and maintain, as nearly as possible, with the boiler power at command, the contract steam pressure and corresponding piston speed of engine. That is, it was deemed advisable rather to comply with the maximum water pressure called for with a reduced delivery by the pumps than with the contract delivery of water. The latter being amply provided for by the dimensions of pumps within the contract piston speed.

"In the following table I have reproduced the principal dimensions of engines and pumps, the pump measurement being made by the writer in behalf of the Water Commissioners, and by Mr. D. H. Johnston for the contractor, at the pump house, a few hours previous to the trial for capacity:

ENGINE AND PUMP DIMENSIONS.

H. P. cylinders diam. (taken from contract)	38	in.
" piston rods (single) diam. (measured)	4.5	"
L. P. cylinders diam. (taken from contract)	66	"
" piston rods (double) diam. (measured)	4	"

Engine No. 1 contact stroke...	49.875	in.
" No. 2 " " ...	49.8125	"
" No. 1 plunger diam.....	38.1212	"
" No. 2 " "	38.1014	"
Plunger rods diam...........	5.	"
" engine No. 1, mean area..................	1131.5461	sq. in.
" " No 2, "	1130.3608	"
" mean area both.....	1139.9585	"
" rings width bearing surface..................	16.	in.

DATA FROM THE CONTRACT.

Maximum piston speed per minute...	110	ft.
Steam presure by engine guage........	60	pds.
Water " pump "	70	"

"The test for capacity involved an actual measurement of the water delivered by the pumps, for which two feasible methods offered. The first by lowering the level of Prospect reservoir, closing all outlets and pumping in a known volume of water, against an artificial head of 70 pounds on pump gauge, produced by throttling with a 36-in. stop valve in the discharge main; and the second by making a special connection with the force main at a point three miles from the pump house, and diverting the delivery over a weir.

"By the first method the actual delivery of water upon which to estimate the capacity of pumps was necessarily small, and by the second method upwards of one hundred stop-valves required closing for the period of weir measurements, with no means of estimating the probable leakage, besides depriving a large section of the city of water during the hours of trial.

"The first method had the advantage of time in that the supply of water to all parts of the city might be made under direct pressure whilst the reservoir was in use for test purposes.

"After carefully canvassing both methods it was finally decided to adopt the first, filling into the reservoir through such a section as was susceptible of reasonably accurate measurement.

"In order that this method might be successfully employed, careful experiments (before and after the test for capacity) were made to determine the tightness of walls and stop-valves, with no apparent leakage, and repeated measurements of lengths and slopes were made, to insure correctness of the data upon which to estimate the discharge. The vertical rise or surface levels in the reservoir were read from a measured rod, divided into feet and tenths, with intermediate graduations to twentieths, which was carefully fixed and leveled in the south basin near the division wall. That portion of the reservoir above the division wall was selected for the test as offering the best facilities for close measurement, and the measured rod was so located that the arbitrary zero level of the water corresponded with 2.35 on the rod. The maximum rise of water level was agreed upon at 5 ft., corresponding to 7.35 on the rod, between which levels accurate measurements of lengths and slopes were taken for the purpose of the trials by Mr. Louis H. Knapp, Superintendent of the Water Service.

"During the capacity trial, when the surface of water in the reservoir coincided with the lowest and highest marks on the rod, the times were read to second from an accurate watch, and between these points the levels were read from the rod at expiration of eacher gular quarter hour. In order that the readings of engine counters in pump house might agree for time with the readings of the measured rod in the reservoir, the rise of level in the latter was carefully noted, and a few minutes previous to the coincidence of the surface of the water and the arbitrary zero point (2.35) on the rod, a messenger was dispatched from the reservoir to the pump house, upon whose arrival the assistants detailed for the purpose began minute readings of the engine counters. Directly the time was read for the agreement of water-level with the zero point on the measured rod in the reservoir, a second messenger with a memorandum of the time started for the pump-house. Upon arrival of the second messenger from the reservoir the minute readings of the counters were discontinued, and readings of the instruments at the expiration of each regular quarter hour were substituted for the remainder of trial.

"The same procedure was observed for the completion of trial. In this manner, with an agreement of timepieces at the two points of observation (reservoir and pump-house), the reading of the engine counters at the time when the surface of the water coincided with any known point on the measured rod can be read correctly or interpolated from the record.

"To insure correctness in the record all data were taken by two intelligent observers and all measurements were carefully repeated.

"Two observers independently read the measured rod in the reservoir and agreed upon the readings, two more read the engine counters at the pump house, whilst the indications of the pressure gauges (steam and water) and the strokes (length) of plungers were observed by the writer in behalf of the Water Commissioners, and by Mr. Johnston for the contractor.

"The trial for capacity began at 4:57:30 P. M., July 2d, previous to which time the engine had been delivering into the reservoir for several hours, and terminated at 10:42:28 P. M., embracing a period of 5h. 48m. 08s., during which interval the surface level of the reservoir was raised from 2.35 to 7.35 on the measure rod, or 5 ft. head was added.

"The section of reservoir filled was a true prismoid, of which the dimensions are given in the following table of reservoir measurements:

DIMENSIONS OF RESERVOIR.

Head 2.35 on gauge rod =	0 level.
Mean length $\frac{506\ 85+507.55}{2}$ =	506.95 ft.
Mean width $\frac{175.7+174\ 5}{2}$ =	175.10 ft.
Area 506.95 × 175.1 .... =	88,766.945 sq. ft.
Head 4 85 on gauge rod.... =	2 5 level.
Mean length $\frac{506.95+522.425}{2}$ =	514.6875 ft.
Mean width $\frac{175.1+190.925}{2}$ =	183.0125 ft.
Area 514 6875 × 183.0125... =	94,194.2461 sq. ft.
Head 7.35 on gauge rod ... =	5 level.
Mean length $\frac{521+523.85}{2}$ ... =	522.425 ft.
Mean width $\frac{191.35+190.5}{2}$ =	190.925
Area 522.425 × 190 925 ..... =	99,743.993 sq. ft.
Head added.... .... =	5 ft.

Then by prismoidal formula the volume of the section of reservoir filled represented

$$\frac{(94,194.2461\times4)+88766.945+99743.993\times5\times7.48}{6}$$

$=3,523,628.04838$ U. S. standard gallons,

corresponding to a daily (24 hours) delivery at observed piston speed (93.772 feet) of

$$\frac{3,523,628.048\times86400}{20708}=14,701,635.3 \text{ gals.}$$

and at contract piston speed (110 feet), for which the boilers are at present entirely inadequate in heating and grate surface.

$$\frac{14,701,625.3\times110}{93.772}=17,246,938.13 \text{ gall's.}$$

The counter reading at 4:57 P. M., July 2d, was 18,728, and at 4:58 P. M., same date, 18,738; and by interpolation at 4:57:30 P. M. was 18,733.

"The counter reading at 10:42 P. M., July 2d, was 22,630, and at 10:43 P. M., same date, 22,642, and by interpolation at 10:42:38 was 22,637.6, from which the double strokes, one engine, or quadruple strokes, both engines, were

$$22,637.6-18,733=3,904.6.$$

The mean length of stroke, engine No. 1, was 49.8125 inches, and mean length of stroke, engine No. 2, 49.651 inches, from which the mean piston speed during capacity trial was

$$\frac{49.8125+49.651\times3904.6}{12\times345.133}=93,772 \text{ feet}$$

per minute.

The calculated delivery of the pumps during the capacity trial has been estimated for the pump of engine No. 1 as

$$\frac{(38.1212^2\times.7854)+(38.1212^2\times.7854-5^2\times.7854)\times49\ 8125}{2\times231}=$$

244.005 U. S. standard gallons per single stroke; and for the pump of engine No. 2 is

$$\frac{(38.1014^2\times.7854)+(38.1014^2\times.7854-5^2\times.7854)\times49651}{2\times131}=$$

242.959 U. S standard gallons per single stroke; and a mean per single stroke for both pumps of

$$\frac{244\ 005+242.959}{2}=243.482 \text{ gallons,}$$

and

$$243.482\times3904.6\times4=3,802,799.2688$$

U. S. standard gallons as the pump displacement corresponding to a delivery of 3,523,628.048 gallons into the reservoir. From which the slip or loss of action of pumps is obtained as

$$100-\frac{3,523,628.048}{3,802,799.269}=7.34$$

per cent. of calculated delivery.

"The loss of action is surprisingly large, but a careful comparison of the

data at reservoir and pump-house during the capacity trial, by half hourly intervals, fails to suggest any cause other than true loss of action.

"The average steam pressure furnished by boilers as read on engine gauge was 563.52 (for water column) = 52.48 pounds, and the mean head pumped against by water-pressure gauge, connected with discharge-chambers of pumps, was 69.86 pounds.

"The water pressure corresponding to a continuous steam pressure of 60 pounds, as provided by the contract, is from the data of trial

$$\frac{69.86 \times 60}{52.48} = 79.8704 \text{ pounds.}$$

"As previously stated, the boilers are wholly unequal to the task of operating the engine at contract piston speed against a water pressure of 70 pounds by pump gauge. Either the quantity pumped must be less than 15,000,000 gallons, if the pressure (70 pounds is to be maintained, or the pressure pumped against must be less than 70 pounds if the 15,000,000 gallons delivery is desired.

"Both the delivery—15,000,000 gallons per diem. and pressure—70 pounds, may be had with ease by adding one more boiler of same capacity as either of the *present pair*.

"The trial for duty began at 1:30 P.M., July 3d, previous to which time the engine had been in operation under the regimen of the trial for several hours, and terminated at 9:30 A.M., July 4th, embracing a period of twenty hours.

"The terms of the contract eliminates the actual consumption of coal as a factor in the duty equation, and provides in lieu thereof that coal shall be charged at the rate of one pound for each and every ten pounds of steam consumed by the engine, or if W represents the net steam consumed for the entire trial, then $\frac{W}{1000}$ equals the weight of coal to be employed as a denominator in the equation for duty.

"The water to the boilers was pumped from the hot well by an independent donkey taking steam from the south battery of boilers, and carefully weighed in two large casks mounted upon platform scales, from which it was drawn as required into a supplemental cask connected with the suction of the feed-pump. An overflow in the supplemental cask formed a gauge point at which the water appeared at beginning and at end of trial, and to which the head of water was caused to approximate as nearly as possible during the trial to check the weight delivered to boilers for given intervals of time.

"The steam came from the north battery of boilers into the engine-room through a 16 in. pipe, from which a branch 8 in. diameter connected with the engine. A joint in the 16 in. pipe, near the branch, was opened previous to the trials, and a blank flange introduced to prevent transfer of steam to or from the pipe during the trial of duty.

"In this manner no steam could pass to the engine other than that accounted for, nor could any portion of the steam changed to the engine be diverted in transit.

"A suitable connection with the 16 in. steam pipe was made near the branch to the engine, from which samples of the evaporation were drawn from time to time for calorimeter tests of quality.

"Certain boiler leaks were detected, and the water caught and weighed as a rebate on the amount charged to the boilers. These were insignificant in effect, but together with the small quantities of steam drawn into the calorimeter constituted all the direct losses.

"Measured gauge-sticks were lashed to glass tubes of water-gauges, from which the water levels in boilers were read. The water levels at commencement of trial being restored at end of trial.

"The trap connected with the jackets was caused to deliver into a cask on the engine room floor from which the condensation was drawn and weighed at intervals for the purpose of estimating the proportion of steam delivered to the engine consumed in the jacket.

"The level of water in the pump well was taken by means of a copper float set in a carefully leveled pipe, the position of the float being read on a vertical scale in the engine-room to exact vertical difference of surface of water in well and center of water-pressure gauge.

"The steam and water pressures were read from Crosby gauges, which were compared with a mercury column in Buffalo, and with a Crosby test gauge before the trials, and again with the test gauge after the trials, through a range of press-

ures greater than were used. The gauges were found to be substantially correct upon first comparison, and to have suffered no depreciation in use.

"The reading of engine steam gauge is in excess 3.52 pounds, occasioned by a vertical water column of 8 feet 1.5 inches, which was necessary to bring the gauge into a position convenient for reading.

"The temperatures of injection and overflow and temperatures for calorimeter purposes were taken with James Green thermometers, known to be correct.

"Time was taken from a Litherland, Davis & Co. chronometer, kindly loaned for the purpose by one of the local watchmakers, which from frequent comparisons with an accurate pocket timepiece was found to vary so slightly from true time as to be substantially correct.

"The barometer used was an Aneroid.

"A series of eighty indicator diagrams were taken from the steam cylinders during the trial, with Thompson indicators, taking motion from arms secured to the rocker shafts. Thirty pound springs were used for the high pressure cylinders and twelve pound springs for the low pressure cylinders; upon test the thirty spring registered 30 pounds and the twelve spring 12.75 pounds per inch of pencil movements.

"The vacuum in condenser and double strokes of engine No. 2 were read from the Crosby instruments furnished with the engine.

"The coal burned during the trial was weighed at end of trial, and the contents of furnaces and ash pits were likewise weighed to determine weight of actual combustible. The weighed coal covers a period of 21h. and 49m.; but the estimate of boiler performance is based upon the average hourly consumption of coal and water.

"The engines worked approximately to contract stroke during the trials for capacity and duty, but as a check upon the strokes a measured stick for each engine, 50 in. between extreme points with 2 in. marked to sixteenth at each end was lashed to one of the horizontal columns joining the steam and water ends, over which moved a pointer attached to the cross-head. By carefully observing the position of pointer at terminal of stroke the exact length of stroke could be read with great accuracy.

"As a check upon the readings of engine steam gauge, a steam gauge found upon comparison with the test gauge to be substantially correct, was connected with the steam drum of boilers and mounted in the engine room for convenience of observation.

"The coal was weighed in gross, as required, and dumped in the boiler house. The water to boilers was weighed in approximately uniform charges (each cask) and net weight and time of final delivery entered in the observer's note-book.

"The pressure gauges, engine counter, vacuum gauge, water levers in boilers, length of plunger strokes, barometer, temperature of injections, overflow and air and pump well gauge were read quarter hourly upon signal by a time-keeper.

"The condensation from the jackets was weighed from time to time as required.

"The calorimeter data and indicator diagrams were taken at random for each hour of the trial.

"In the following table are given the everages and total for the twenty hours of duty trial:

STEAM PRESSURE.

At boiler 65.4406 − 4 pounds	61.4406
At engine 61.1937 − 3.52 "	57.6737
Difference	3.7669

WATER PRESSURE.

By pump gauge, pounds	71.3594
By diff. level of water in pump well and center of pressure guage, feet	17.0433
By diff. level of water in pump well and center of pressure gauge, pounds	7.3844
Total observed head, pounds	78.7438
" " feet	181.7407

PISTON TRAVEL.

Engine counter 1:30 p. m., July 3d	24479
" " 9:30 a. m., ' 4th	37513
Difference	13034
Mean stroke engine No. 1, inches	49.872
" " No 2, "	49.8008
" both engines, feet	4.153

Total piston travel for trial,

$$13034 \times 4.153 \times 4 = 216{,}520.808 \text{ ft.}$$

Piston travel in feet per minute,

$$\frac{13034 \times 2 \times 4.153}{20 \times 60} = 90.217 \text{ for one engine.}$$

STEAM CONSUMPTION.

Total water weighed to boilers, pds	299473
Leakage from boilers, pds	1482.5
Drawn off for calorimeter, pds	211.5
Delivered to engine, pds	297779

CALORIMETER DATA.

Mean weight of steam condensed.	10.208
" " water heated.....	200.000
Mean initial temperature.........	77.208
Mean final temperature...........	130.625
" range "	53.417

Thermal value of steam,

$$\frac{53.417 \times 200}{10.208} + 130.625 = 1177.195.*$$

Temperature of steam at 57.674 pds 1206.85

Percentage of water entrained,

$$\frac{1206.85 - 1177.195}{899.53} \times 100 = 3.297$$

Water entrained in the steam.....	9817.7736
Net steam delivered to engine, pds.	287961.2264
Corresponding consumption of coal as per terms of contract, pds....	28796.1226

TEMPERATURES.

Injection....	65.153
Overflow....	115.7375
Air....	76.7875
Barometer....	29.4437
Vacuum by gauge....	26.9125

WATER LEVELS.

Boiler No. 4 at 1:30 p. m., July 3d, in..	5
" " 9:30 a. m., " 4th, ..	5
" No. 5 at 1:30 p. m., " 3d, ..	6
" " 9:30 a. m., " 4th, ..	6
" No. 4 mean level for trial......	4.4259
" No. 5 " "	4.4389

CONDENSATION FROM JACKETS.

Total water weighed from trap, pds. 15111 54

Percentage of total water and steam to engine consumed in the jackets,

$$\frac{15111.54}{297779} \times 100 = 5.07475.$$

"Directly the trial was completed a brief summary of results was submitted in which the duty was calculated by the formula:

$$D = \frac{A \times P \times E \times 1000}{S}.$$

Where D represents the duty in foot pounds, P= the observed head in pounds per sq. in. pumped against + an allowance of *one* pound for frictional resistances of water passages into and out of pump, and S the net steam delivered to the engine from which the duty was obtained of

$$\frac{1130.9535 \times 79.7438 \times 216520.808 \times 1000}{287,961}$$

$$= 67,812,169.996 \text{ foot pounds (A).}$$

* This estimate is based upon temperatures by thermometer, with no corrections for difference of specific heat. Correcting for true temperature produces a better quality of steam, a smaller entrainment of water, a larger actual consumption of steam by engine, and a diminished "duty." The error, although slight, is in favor of the engine.

"After a more careful consideration of the contract, I am inclined to believe a duty calculated in this manner is not meant by '*seventy million pounds of water raised one foot high* with the consumption of one hundred pounds of coal,' the coal to be estimated at the rate of one pound to each *ten* pounds of steam consumed.

"Construing the terms of the contract literally, the work done is the actual water pumped during the trial into its weight per unit of volume into the head against which it is delivered, with no allowance for loss of action in the pumps nor for frictional resistances of pump passages.

"The evaporation upon which the duty shall be estimated is considerably above the average of boiler economy, and it is not unreasonable to suppose in the light of this evaporation, that the contractor meant to furnish an absolute duty upon contract trial.

"The calculated delivery of the pumps during the duty trial was

$$\frac{1130.9535 \times 49.836 \times 4 \times 13034}{231}$$

$$= 12,720,777.339$$

gallons for 20 hours, of which quantity, by the data of capacity trial, .9266 was actually delivered, or

$$12,720,777,309 \times .9266 = 11,787,072.282$$

gallons, and estimating the weight per gallon at 8.33 pounds, and head through which the water was raised as 181.7407 feet, then the absolute duty was

$$\frac{11,787,072.282 \times 8.33 \times 181.7407 \times 1000}{287961}$$

$$= 61,968,284.22 \text{ (B)}$$

equivalent to 61,968,284 22 pounds of water raised one foot high per hundred pounds of coal upon contract evaporation.

"The calculated delivery of the pumps during the trial for duty per diem of 24 hours was

$$\frac{12,720,777.339 \times 6}{5} = 15,264,932.807 \text{ gals.}$$

"As a check upon the work of the water end of the engine from the data previously given, the indicator diagrams were carefully measured with the following results:

Average initial pressure H. P. cyl....	47.678
" " " L. P. "	12.206
Average counter pressure at mid. stroke H. P. cylinder.............	5.551
Average counter pressure at mid. stroke L. P. cylinder (absolute)....	5.423
Average terminal pressure L. P. cyl..	4.262
Average vacuum in pounds at mid. stroke L. P. cylinder.............	9.031
Expansions, by pressures...........	3.3154
" by volumes............	4.0268
Average mean effective pressure front side H. P. piston.	41.34262
Average mean effective pressure back side H. P. piston	40.36612
Average mean effective pressure front side L. P. piston....	12.8305
Average mean effective pressure back side L. P. piston	13.78365

FACTORS OF HORSE POWER.

Front side H. P. piston, one engine..	1.5285
Back " " " ..	1.5502
Front L. P. " " ..	4.6422
Back " " " ..	4.6765
Indicated horse power	499.5774
Steam per indicated horse power, per hour..............................	28.8205
Coal per indicated horse power upon evaporation of ten to one, per hour	2.88205
Duty of steam end of engine........	68701100
Percentage of useful effect by duty A.	98.706
" duty of steam end represented by duty B	90.200
Percentage of total power developed expended in overcoming engine and pump friction by duty A...........	1.294
Percentage of total power developed expended in overcoming engine and pump friction by duty B...........	9.800

"As matter of some interest, although forming no part of the contract requirements for duty trial, the performance of the boilers has been estimated and reproduced in the following table:

Pressure by gauge in engine room..	61.4406
Total coal charged (21h 49m) pds..	45.100
" " 20 hours, pds..	41313.61
Ash, clinker and some unburnt coal weighed back from ash pits and furnaces during and at termination of trial, pounds...	6785
Percentage of non-combustibles....	15.0443
Total water weighed to boilers, pds.	299473
Water entrained in steam, pds.....	10077.266
Steam per pound of coal from temperature of feed, pds...........	7.00485
Steam per pound of combustible...	8.2453
Steam per sup. foot of heating surface per hour, pds............	4.1342
Coal per sup. foot of grate surface per hour, pds..................	23.7439

"The duty terms of contract include a steam pressure (presumed to mean at engine) of 60 pounds by gauge. The steam pressure actually held as a mean for trial was less than 60, but not enough less to account for the discrepancy in duty by either method of computation. It was urged during the trial by the contractor's representative, Mr. Johnston, that the variations in steam pressure were calculated to diminish the duty. After a careful examination of the column of pressure I am quite certain the variations were too small to have any material effect upon the duty. It is doubtful whether, with unlimited boiler capacity, the steam pressures could have been held much closer to the standard, except with a quality of firing rarely obtained from men employed for this purpose.

"Recapitulating the results of the trials for convenience of reference: The daily actual delivery during the capacity trial was 14,701,635.3 gallons at a piston speed in ft. per minute of 93.772, corresponding to a delivery per diem of 24 hours at contract piston speed (110 feet) of 17,246,938.13 gallons, or 2,246,938.13 gallons in excess of contract requirements.

"The duty by the method usually employed and designated as duty A, was 67,812,169.996, and by the exact language of contract designated as duty B, 61,968,-284.22.

"The engine while complying fully with the terms of the contract for capacity—indeed furnishing an unusual excess of delivery within the limits piston speed—does not comply with the guaranteed duty by either method of estimate, and it is entirely a matter of judgment, which an intimate 'knowledge of the public requirements can best supply, whether the increased capacity of the engine shall compensate in whole or in part for the loss in duty.'

"Referring to the question of construction, the engine is an absolute specimen of its kind, every detail having received a degree of attention seldom given to work of its class. The writer has examined many 'Worthington' and other pumping engines for public water supply, some at work and some at rest, and hazards the opinion that none excel this in solidity and exactness of construction.

"Excepting so much of the detail as is necessarily hidden from view in the steam and water cylinders, * * * the engine complies in all respects with the terms of the detailed specification, and by inference * * * * that which is not seen is equal to that which is exposed."

DESCRIPTION OF THE MORE RECENT METHODS OF TRANSPORT ON RIVERS AND CANALS AND THEIR EMPLOYMENT IN GERMANY.

By J. KLETT.

From "Zeitschrift für technische Hochschulen," for Abstracts of Institution of Civil Engineers.

In the earliest experiments made on the Elbe in 1720 a hempen rope was fastened on shore, the other end wound up on board and vessels propelled in this way, and nothing better than this rough system obtained for a hundred years when, in 1820, Messrs. Tourasse and Courteaut designed special flat-bottomed tugs 75 feet long and 17 feet wide, with a horse capstan for winding up the rope, and suseqently (on the Seine) a 6 HP. steam engine was substituted for the horse capstan.

Chains next took the place of hempen ropes, and between 1820 and 1830 many chain tugs were employed on French rivers; but the first systematic service was carried out in 1846 between Paris and Montereau (65 miles) with tugs designed by Mr. Dietz, which in their essential parts of construction, are similar to those in use at the present day. These tugs drew 18 inches of water, and were fitted with engines of from 35 to 40 HP. actuating the drum on which the chain was wound, two sets of gearing being provided for going up and down stream respectively. The boiler pressure was 5¼ atmospheres, and the expenditure of fuel 5½ lbs. per HP. per hour. Subsequently the chain was laid further up the Seine and also applied to rivers in France.

In Germany in 1866 chain tugs were running on 200 miles of the Elbe; and in the next ten or twelve years this system was in use on the Saale, the Brahe and the Neckar.

The Elbe tugs are 138 to 150 feet long, 24 feet wide, with 18 inches draught. On the other rivers they are somewhat smaller. The sides are of ¼ inch iron plate, and formerly the bottoms were of ⅛-inch iron, but now they are built of 4-inch pine planks, as suffering less from abrasion on dragging over a rough bed. There is a rudder at each end, the wheel being amidships. The engines are from 60 to 70 HP., and work with a pressure of 5 to 7 atmospheres, with expansion and partial condensation. In slight currents a single drum is sufficient, the chain being kept pressed against it by rollers. and the drum nicked to prevent the slip of the chain, but ordinarily there are two drums to which the engine power is transmitted by two sets of gearing with different rates of speed—one for working up stream with great power and small speed; the other, down stream with less power and greater speed. Projecting over each end of the tug are booms furnished with guide-rollers for the chain; these booms increase the facility of steering.

The chains are from ¾ to 1 inch thick. When fractures occur, which is seldom, it is generally at the moment of the chain being first would round the drum. Each drum is fitted with a brake, and at the ends of the booms are clips to prevent a running out of the chain in case of the brake failing to hold.

Chain towing has so increased on the Elbe, that in 1874 there were twenty-eight tugs running regularly between Hamburg and Aussig (420 miles). On the Neckar, at the same period, five tugs were employed on 56 miles of chain, and this was to be extended for 30 miles more, from Heilbronn to Cannstatt. Experience has shown that chain-tugs have great advantages over paddle-tugs even in smooth water; for in the latter 60 to 70 per cent. of the power is lost in slip, and another great advantage of chain-towing is that there is no wash or swell.

The charge for transport averages about ¼*d.* per ton per mile, which is twice as cheap as the lowest railway charges.

In 1865 Mr. de Meseil, a Belgian, introduced a system of transport where a wire rope was substituted for the chain; this was taken up and improved by a German engineer, Max Eith of Wurtem-

berg, and worked with great success on a 40 mile section of the Maas (from Namur to Liege). It was subsequently employed on canals in Holland and Belgium, and also on the Rhine; and resulted in 1870 in the formation of a wire-rope tug company under the management of Mr. T. Schwartz. Extensive trials were also made on the Danube about the same time. with very satisfactory results, the useful effect being 77 per cent.

In 1873 the above company laid down the line from Bingen to Rotterdam, but worked the upper section only themselves, viz.: from Bingen to Ruhrort (155 miles). From Ruhrort downwards a concession was granted to a Dutch company, who employed a special kind of tug in which the rope passed over drums inside the vessel similar to the chain-tug system; but the usual arrangement of having the rope outside the tug is most convenient, as it enables it to be easily cast off and taken up again when two tugs meet.

The rope used on the Rhine is formed of forty-nine wires 0.189 inch thick, is 1.7 inch in diameter, and weighs 4¾ lbs. per yard. It costs 10*d.* per foot, which is about one-third the weight and cost per foot of an iron chain of equal strength.

Wire-rope tugs are also employed on the Oder and the Neva, and on the Erie Canal they have almost superseded the celebrated "Baxter" steamboats.

The first wire-rope tugs at work in Holland and Belgium had a 20 HP. engine for the driving-wheels, and another 10 HP. engine to work a screw when going down stream clear of the rope. At each end, outside the tug, are guide wheels to keep the rope clear of the vessel, and at the center are two large wheels which lead the rope on to a Fowler's clip drum against which it is kept pressed by small rollers. (To pick up the rope and pass it over the wheels and drum takes a quarter of an hour.)

The Danube Company's tug "Nyitra," to which the Rhine tugs are very similar, is 140 feet long, 24½ wide, and draws 3½ feet of water; the clip-drum is 10½ feet, and the adjoining wheels about 9 feet in diameter. Against a current of 4¼ feet per second it can draw eight barges with a total load of over 2,000 tons at a speed of 3 miles an hour, with useful effect of 75 per cent. In chain-tugs this percentage is higher on account of the greater flexibility of the chain. Fractures of the rope seldom occur, in spite of the rocky bottom in certain sections, and the life of a wire rope may be taken at from four to six years.

Wire-rope tugs cannot work in less than 3 feet of water, or only with difficulty, whereas chain-tugs can work in half that depth. As regards steering facility they are equal. The delay caused by fractures is an important item in the comparison, and repairs to chains occupy considerably less time than repairs to wire ropes. To sum up: chain-tugs in depth under 3 feet and in sharp curves are preferable to rope-tugs; in moderately strong currents and larger curves they are about equal; but in canals, and in large deep rivers rope-tugs are best and both are superior in ordinary circumstances to paddle-tugs.

In canal tunnels, as in the 4 mile section (Monts to Paris), where steam cannot be used on account of the smoke, chain-tugs worked by a horse-capstan tow a barge through in one-third the time and at one-fourth the cost of the former system when men were employed for towing.

On the Rhine and Saone, where particularly strong rapids are met with, special steamers called "grapins" are employed. The "grapin" is an iron wheel of about 20 feet in diameter and 17½ tons weight, furnished with projections or picks, fixed in a well-hole at midships, and worked by a chain attached to the paddle-shaft. On ascending a river the "grapin" is lowered till the picks grip the bed on which the wheel slowly turns, and the paddles working at the same time in this way tow barges over the strongest rapids. Huet's water locomotive and Busquet's tug are worthy of mention; the latter works on a chain. though it is similar to a wire-rope tug, The Baxter steamboat mentioned above as being in use on the Erie Canal, was the outcome of a competition invited by the State of New York for a prize of £20,000 for the steamer which best fulfilled the following conditions, viz.: a mean speed of 3 miles an hour with a load of 200 tons, small cost, and no wash or swell. This steamboat is 100 feet long, 17½ feet wide, and about 9 feet deep, with flat bottom and vertical sides, and in-

cluding engines and coal weighs 52 tons, carries a load of 200 tons, with a draught of 6 feet of water, and has an average speed of about 4 miles, but can work up to 7½ miles an hour.

On the Saar Coal Canal Jacquel's steam tug system is in use, where the screw is within the body of the vessel and surrounded by a cylinder, and is fed with water by two large channels leading from the sides of the vessel to the front of the screw.

THE DEFLECTIVE EFFECT OF THE EARTH'S ROTATION.

By W. M. DAVIS.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN an article by Rd. Randolph, C. E., in a recent number of this magazine (Feb. 1883, p. 117-120), on the "Influence of the earth's rotation on derailments," the lateral impulse is given only half of its true value because two of its components are neglected, namely: the effects of diurnal centrifugal force and of the tendency to the "preservation of areas." The impulse, as estimated by Mr. Randolph is the third component, which may be called the whirling table effect.* His conclusions that the deflective orce is equal in all directions, that it varies with the sine of the latitude (or as in his formula, cosine÷cotangent), and that it is insufficient to determine the direction of derailment, are correct.

Although it is some twenty odd years since the true estimation of this force was first published by Ferrel (*Cambridge Math. Monthly*, i., 1859), there is still too frequently a misunderstanding of the matter, and it is often stated that its effects are only seen in motions having some meridional element in their direction as Hadley first explained it in 1735. This is entirely incorrect; the deflective effect is the same on motions in all directions from a given starting point. The following brief statement gives a comparatively simple version of the explanation of this fact, which at first sight is not at all apparent.

The total deflective effect of the earth's rotation may be resolved into three components: A, that arising from the partial daily rotation of any small area of its surface about a local earth-radius as an axis; B, the effect of centrifugal force; C, the tendency to the preservation of areas. The following analysis is worded for the northern hemisphere.

A. If we consider the daily motion of any small area of the earth's surface of which the latitude is φ, it may be regarded as resulting from a motion of translation around its circle of latitude, and a complete left-handed rotation about an axis through its center parallel to the axis of the earth. The effect of the translation will be considered under B and C; the effect of the full rotation on an oblique axis as above stated is the same as that of a partial rotation on an axis at right angles to the surface, that is on an earth-radius; and the partial rotation is to the full rotation as $\sin\varphi$: radius. The surface is therefore in the condition of a whirling table, which rotates through $2\pi\sin\varphi$ degrees in twenty-four hours; or representing the earth's angular velocity per second by ω, the rotary velocity of the surface $= \omega\sin\varphi$. A body moving from the center of such a rotating surface in any direction with a velocity of a feet per second will be continually left on the right of its original direction by $a\omega\sin\varphi$ feet per second.

B. A body at rest on the earth's surface moves eastward at a velocity $v=R\omega\cos\varphi$, and its centrifugal force$=\dfrac{v^2}{R\cos\varphi}=v\omega$. This being resolved along a tangent to the earth's surface $= v\omega\sin\varphi$, and the flattening of the earth at the poles produces just enough up-slope toward the equator to neutralize this force. If it be now supposed that the body moves eastward relatively to the earth with a velocity a, its angular velocity will be increased to

* The earliest application of this whirling table effect in the explanation of the rotary action of storms was made by Mr. Charles Tracy of New York in 1843. I have lately called attention to his original article (*American Journal Science*, xlv., 1843, 65-72) in *Science*, No. 4, p. 98.

$\omega+\frac{a}{R\cos\varphi}$, and its centrifugal force will equal $(v+a)\left(\omega+\frac{a}{R\cos\varphi}\right)$. For all ordinary motions, except in very high latitudes, $\frac{a}{R\cos\varphi}$ may be neglected, and the tangential component of this new centrifugal force becomes $(v+a)\omega\sin\varphi$. As the body was relatively at rest before, it will now be urged to the right by the force $a\omega\sin\varphi$.

C. In virtue of the law of preservation of areas, if a body in latitude φ, rotating with a velocity $R\omega\cos\varphi$, move northward with a velocity a, its rotary velocity will increase as its radius of rotation decreases, and so become

$$Ra\cos\varphi\frac{R\cos\varphi}{R\cos\varphi-a\sin\varphi}.$$

It will thus be carried to the right of its original path by a force equal to the difference between its original and its acquired rotary velocity=

$$\frac{a\omega\sin\varphi\cos\varphi}{\cos\varphi-\frac{a}{R}\sin\varphi}.$$

For all ordinary velocities the last term of the denominator may be neglected, and the expression becomes $a\omega\sin\varphi$.

The combined effect of B and C on an oblique motion a, making an angle θ with the meridian, will be the resultant of the deflective forces on its northward and eastward components. The deflections of these components are $a\omega\sin\varphi\cos\theta$ and $a\omega\sin\varphi\sin\theta$, and their resultant= $a\omega\sin\varphi\sqrt{\cos^2\theta+\sin^2\theta}=a\omega\sin\varphi$, which is independent of θ. Combining this with A, we have $2a\omega\sin\varphi$ for the total deflective force in latitude φ on a body moving with a (moderate) velocity a in any direction on a planet of any size* with the angular velocity ω. This force tends to the right in the northern hemisphere but to the left in the southern. The simple relation existing between the several parts of the total deflecting force is not a little surprising and interesting.

Among the effects arising from this force that have been suggested by various authors, there may be mentioned: the change of the plane of oscillation of a free swinging pendulum; the oblique motion of the trade winds and the general easterly motion of winds in temperate latitudes; the high barometric pressure about the tropics and the low pressure of the polar regions, especially the antarctic; the constant direction of rotation and a certain share of the strength of cyclonic storms; the regular turning of the great oceanic eddy-currents, and the formation of sargasso-seas at their centers; a possible but problematic effect on the cutting of river valleys; and finally the very doubtful control over railroad accidents and more plausibly the displacement and faster wearing of the right-hand rail on a double track road.

The last named effect having a special interest in connection with engineering, the following extracts from *Das Ausland* (a weekly geographic journal published at Stuttgart), 1876, 334, may be worth quotation.

The Hamburg-Harburg double track railway opened in 1872, crosses a flat marsh between the northern and southern arms of the Elbe, and its road-bed here rests on an elastic subsoil. After several years of running, it was found that the rails were somewhat loosened and moved forward from their original position, and that the right-hand rails on each track (looking in the direction of running), had been moved most, as was shown by the displacement of the joints from their original position opposite those of the left rail. This excess of right-handed pressure is ascribed to the effect of the earth's rotation, and is according to the usual explanation improperly regarded as possible only on north and south railways, such as this one is.

If the effect is properly referred to so small a force as the deflection arising from the earth's turning, it must be on account of its constancy; it should be apparent on similar roads in any direction, and would be most perceptible on roads in high latitudes. Can any other examples of such an effect be given?

CAMBRIDGE, MASS., March, 1882.

* On minute planets like the asteroids, where a moderate surface velocity might be a large part of the velocity of rotation, the terms neglected above would become appreciable.

DAMP WALLS.

From the "Builder."

In a recent issue of the *Badische Gewerbe-Zeitung* appears an article on this subject, the importance of which induces us to reproduce it in our columns, with such additions and remarks as seems to us desirable. The writer, Professor Meidinger, observes that, if in an old house wall-paper turns mouldy and peels off, the cause is dampness in the walls. The walls of an old house may turn damp from various causes:

1. The rain sickers through a defective roof into the wall, or it penetrates a badly-pointed wall.

2. In the cold season vapor produced from special causes is deposited on cold walls.

3. The wall contains aphroniter (nitrate of lime. Chloride of calcium, which occurs less frequently, displays the same behavior; quarry-stones which remain wet contain probably one of these salts). The wetting of walls in consequence of hygroscopic salts is caused only rarely by the formation of saltpeter; most frequently sulphates of sodium, and especially of magnesia, show themselves, which are contained either in the mortar or in the bricks. Mortar of dolomitic limestone, which has been burned with fuel containing sulphur, especially has caused very frequently to the formation of wet places. Even the presence of sulphuric acid in a damp atmosphere, in districts where much coal is consumed, has given rise to the formation of sulphate of magnesia in quarry-stones, and consequent damp walls, as was proved some time ago in London on facades of limestone containing magnesia (Portland stone).

4. The underground water reaches so high that it rises in the wall of the basement.

5. The house is built on a slope, so that the rain-water running down enters the wall of the basement.

Firstly. The cause of the dampness named first may be removed by repairing the roof, or by repointing the wall. A coating w th oil paint of the outer fact of the wall may also be recommended in certain cases. If the outer walls consist of timber and bricks, which cannot be joined closely, the method adopted, especially in mountainous districts, is very serviceable; that is to say, of covering in the walls of the weather-side with boards or shingles, and for greater durability painting the latter in oil. On the Lower Rhine, slate, as used for roofs, is also employed. A cover with metallic slates is likewise to be recommended, and quite recently pressed plates have been used which have the character of shingles. All perfectly water-tight coatings and coverings on vertical surfaces of course keep off driving rains, and prevent the accumulation of dust and the growth of moss and lichen; but they form, at the same time, an impermeable layer, arresting ventilation through the walls, on which account they may prove injurious to health, under certain conditions, in overcrowded houses not provided with artificial ventilation. The duration of such coatings and coverings likewise is not very great, for oil varnish becomes gradually humid, liable to form emulsions with water, and, when in that state, persistently retains damp, which penetrates also into the interior of the wall. It is evident that walls already damp cannot be dried by such means; that, on the contrary, their drying is prevented. On the other hand, the shingle walls in use in Switzerland, or the protective walls of Solingen plates employed in the country along the Weser, and the plates used on the Rauhe Alb in Württemberg, besides the slate coverings, are quite to the purpose.

Secondly. The precipitation of damp and water on cold walls is observed principally in kitchens and in large rooms filled on occasions by large assemblages. In the former case, the deposition of damp arises from the steam generated in cooking; in the latter, it is caused by the breath of the people collected together. If the walls are painted with oil color, drops of water are formed which collect on the walls, and in some cases even wet the floor. If the walls are coated with size-color, the water pen-

etrates and makes them darker; as soon as the generation of steam is arrested for a time, they become perfectly dry again. If the walls are papered, the paper-hangings become wet and dark to dry, however, again very soon and completely. The paper does not turn mouldy, but the paste will probably be destroyed in course of time, and the paper itself discolored. Under such conditions, the outer walls are chiefly exposed to the precipitation of water, especially if they are built of quarry-stones, which are good conductors of heat. Brick walls are less exposed to such a deposition, and walls of tufa and wood not at all. If it is intended to protect an outer wall exposed to such precipitations, the simplest way is to board it. The boards, of a thickness of 0.4 in., are nailed to flat beading ¾ in. to 1.1 in. thick, and secured with holdfasts to the wall, the intervening space being filled with straw. Thus a very bad conductor of heat is placed nearest the wall, upon which water will not be deposited. The warming of the room is also greatly facilitated by this means; for this reason, such a covering may be recommended in many cases, but especially for north or east walls, and more particularly for bedrooms. The boards are either covered with shirting, upon which the paper may be hung, or they are nailed with reeds (much used in place of laths in Germany) and plastered with gypsum, after which the wall may be treated in any manner desired. The cost of such a boarding is in Germany about 1s. per square yard, the shirting 7d., the reed and gypsum coating 1s. 5d.

Thirdly. Aphroniter is most frequently the cause of permanently wet walls, or such as become always wet in damp weather. It is observed principally in the lower stories. Its origin is due to organic substances containing nitrogen, especially exhalations of man and animals, which lodge in the walls and form nitric acid during their decomposition; the latter, in combining with lime, forms nitrate of lime. Its appearance is therefore most frequently met with in water-closets, in stables, and in the country very often on walls near accumulations of liquid manure. Nitrate of lime is a soluble salt (it is hygroscopic); that is, it absorbs water from the atmosphere, more or less according to the humidity of the air. In dry weather part of the water absorbed during damp weather passes back into the atmosphere. If a wall contains little saltpeter, it becomes light in color and dries in dry weather; during damp weather, on the contrary, it turns dark and wet. Should the wall contain much saltpeter, the wall is permanently wet, as in stables. Paperhangings on a wall containing saltpeter appear dark during damp weather, and may easily be pulled off. The paste, kept damp for some time, gradually decomposes and thus loses its adhesive property, the paper hangs loosely even in dry weather, and is held in its place only at the permanently dry spots. The adhesive ingredients of colors are likewise destroyed, the colors fall off as dust, mould is formed, and the whole appearance is totally deteriorated. The aphroniter possesses the property of spreading to a certain distance from the spot where it originates over the porous wall, through stone and mortar. It thus penetrates the whole thickness of the wall, and arrives, although generally found only on one side, at the other surface of the wall. Quite apart from its ugly appearance, a damp wall possesses other disagreeable properties. The adhesive ingredients of paper and color develop an unpleasant odor during their decomposition; the same is observed in mouldy paperhangings, and in the timber in contact with damp walls. Effects injurious to health have consequently been often attributed to damp walls, although it would be difficult to prove such an assertion. Various means have been proposed for preventing damp in walls from aphroniter, or at least obviating the disagreeable consequences attendant upon its formation. We select some of the principal means suggested.

(*a*.) The evil cannot be remedied by simply removing the mortar coating, as far as it shows damp, even between the stones, and subsequent fresh plastering. After the mortar has become thoroughly hardened and dry, the wet places appear again after a little time during damp weather, although not quite so large as before. The saltpeter still in and between the stones gradually penetrates part of the new plaster, until it shows itself on the outside. There is no doubt that, even after removing this second

plastering and putting on a third, the latter would show wet places, although of smaller dimensions. It might be possible to gradually extract the whole saltpeter from the wall, just as it is possible to remove oil spots from wood by repeated applications of wet pipe-clay. This method of drying a damp wall—although it would effect a radical cure, as long as no fresh formation of saltpeter takes place, and although it would be most advisable from a sanitary point of view—will not find much favor, on account of its inconvenience, tediousness and expense. In the rare cases where actual formation of saltpeter is the cause of the dampness in walls, in stables and closets, a coating of dolomite cement, to which some phosphate of magnesia has been added, has proved a very efficient means for preventing the further formation of saltpeter. As the formation of ammonia always preceds that of nitric acid, the ammonia combines rapidly with the magnesia contained in the mortar to insoluble phosphate of ammonia-magnesia, and the carbonic acid of the decomposing urine contained in the ammonia with the lime to carbonate of lime. As it is, the not inconsiderable quantity of phosphoric acid contained in urine by itself causes the formation of the insoluble magnesia combination.

(*b.*) It is stated by practical builders that half cement mortar, that is, ordinary lime mortar mixed with the same quantity of Portland cement, is the best means for drying the walls of water-closets; no penetration of damp has been observed several years after the new coating has been applied. In Germany, instead of ordinary Portland cement, Erdmenger's Portland cement of dolomite is considered the most suitable for the purpose.

(*c.*) For some time past it has been tried to prevent the penetration of the saltpeter still remaining in the stoves into the fresh plaster by coating the stones and the joints between them with an isolating layer impenetrable by water. Asphalt, either by itself or mixed with linseed oil, has been used; pitch, common rosin, and tar have likewise been recommended, the latter, however, less, on account of its liquid state and its powerful smell. The mass must be melted, and applied hot with a brush. It is imperative that the surface of the stones be completely covered, and the joints between them perfectly closed up; the saltpeter will percolate through the smallest crack, and thus produce wet places on the wall. Before applying the isolating layer, the room must be artificially and very highly heated for several days, to make sure that the exposed stones and joints have been perfectly dried. The asphalt or its mixture with linseed oil must penetrate to a certain extent into the stones to insure a perfect adhesion; this is not possible if the stones are damp. Small places may also be warmed and dried by holding a charcoal pan close to them. Timber which may be in the damp wall must be treated in the same way; in this case it would be advisable to remove the stonework round the piece of timber as far as it shows damp, to dry the latter well, and then coat it on all sides with asphalt. Upon this isolating layer, which should be from 0.2 in. to 0.4 inch thick without interruption, after hardening, ordinary plaster or gypsum is applied. In carrying out the above, the plaster should be removed, not simply where wet places show themselves, but from one ft. to 2 ft. round them, the isolating mass being applied to the same extent, so as to prevent the saltpeter from penetrating sideways and causing damp spots round the edge of the new plaster. It is not to be expected that the latter should combine closely with the isolating mass; it receives its support sideways from the old plaster still sound. This would be no objection where only small patches of plaster had to be renewed; but large wall spaces would sound hollow, and probably might become detached unless a close junction of the plaster with the stones were effected by driving in here and there holdfasts coated with asphalt, before the plaster is put on. A putty of asphalt and mastic, also, was successfully employed at the Allgemeine Hospital of Vienna.

(*d.*) A few years ago tinfoil was recommended as an isolating material. It was put on the wall with paste, after every vestige of the old paper had been removed, the fresh wall paper being then put on the tinfoil. Although the latter is very cheap (the cost of pure tinfoil in Germany is only 3s. 7d. per kilogramme, or 1s. 9d. per lb., with which quantity a space of about 12 square yards may be

covered), and the process is very simple. It was soon found that it cannot be put on a damp wall, on account of the paste decomposing. It was next tried to secure the tinfoil with tacks, but the latter soon began to rust; tin tacks might perhaps be more suitable. No case has been recorded in which tinfoil has been pasted upon a wall previously well dried either by natural means or artificial heat. If the wet places are not too large, it might be advisable to paste the tinfoil first on the paper, and, after drying, to put the paper on the wall, care being taken to use paste only for the part of the paper free from tinfoil. The paper would thus lie hollow against the wet place on the wall, where the tinfoil acts as a protector against damp. Lead-foil, in place of tinfoil, is not to be recommended, as lead is attacked by damp saltpeter.

(*e*.) Asphalt paper has sometimes been nailed on damp walls; but in such cases a covering of shirting is necessary for receiving the wall-paper. The cost in Germany is about 10d. per square yard. The protection, however, is not permanent, for the asphalt paper lasts only a few years.

(*f*.) There is no record as to the effect of painting damp-walls, but it is well known that such a coating after some time blisters and finally peels off. It would be worth while to examine more closely into the question whether this always takes place, or only under certain conditions. It is thought that several coatings of paint put on a wall well dried by artificial means would penetrate the same, and unite so closely with the plaster as to prevent a peeling off taking place. Three coats of paint would in this case cost about 9d. per square yard. The paper would have to be put on before the last coat of paint is thoroughly dry, as otherwise the paper would not stick.

(*g*.) If wet places in walls assume large dimensions, it is recommended to face the wall with a brick (or tufa stone) wall or boards. The first is expensive and takes up room; joints with the old wall must be made with asphalted bricks to prevent a transmission of saltpeter. The second remedy is that to be adopted in most cases. The process is similar to that applied in boarding walls outside; filling in with straw, however, is omitted. For greater protection, the boards, as well as the beading fastened against the wall, are coated with silicate paint. Such a coating on both sides of the boards costs about 2½d. a square yard. The whole expense for fixing such a boarding, including the covering with shirting, but excluding the wall paper, is at most 2s. per square yard. It appears unnecessary to the author to provide for ventilation between the boarding and the wall by leaving openings at top and bottom, as it is not intended to dry the wall. A consequence of induced ventilation would be simply to cause the covered wall to absorb more or less moisture according to the state of the atmosphere, just as if the boarding had not been put, while with sluggish circulation the wall gets damp and drys more slowly, it being impossible to cut off the access of air entirely. By others, the necessity of thorough ventilation between wall and boarding is insisted on, it being pointed out that rapid circulation must tend to decrease dampness, and at the same time prevent the otherwise inevitable formation of mould or fungus in the boards.

(*h*.) Quite recently wood hangings have been introduced in Germany, serving as isolating layer between ordinary wall-paper and damp walls. These hangings are made in the form of webs or wickerwork of strips of wood or shavings of North Swedish or Finnish pine, 0.04 in. thick, and 1.17 in. to 1.56 in. wide, which are said to resist the effects of damp for a number of years. They are manufactured in lengths of 22 to 33 yards, of a width of 2 ft. 6 in. to 5 ft., and sold at 1s. 4d. per square yard. The wood hangings are fastened to the wall with galvanized nails, the nail-heads being covered with pieces of shavings slipped in, at a cost of about 6d. per square yard. A covering of shirting is also in this case applied before putting on the wall-paper. This wickerwork may be directly used for panelling; the panels are produced by beading, and by leaving the whole in that state, or applying coatings of varnish, or painting the several stripes with various oil colors. Patterns may in this manner be made at a cost of 6d. per square yard; one coating of varnish at 2½d.; of oil-paint, at 6d. to 1s.

Fourthly. When underground water is the cause of dampness in walls, aphroniter, as a rule, always co-operates. Water

alone does not rise so high, as we know from the behavior of cellars, the sole of which very often is only just above the level of underground water, and the walls of which are nevertheless quite dry. The same means of prevention as above mentioned may be employed; they are, however, only palliatives, which do not dry a damp wall. A radical cure may be effected only either by a perfect isolation of the wall from the source of damp,—which may be done in existing walls by draining at intervals, and isolating from the ground below by the insertion of sheets of asphalt felt,—or by completely eradicating the damp from the wall. This may be done by stamping in between the damp wall, which must be previously stripped of its plaster, and a provisional planking, a layer, about 2 in. in thickness, of fresh quicklime powder. For outside walls the planking may be dispensed with; all that need be done is to dig a trench along the foundation and fill it with lime. The damp may also be got rid of by heating the rooms with coal-baskets, and by drawing the heated air from the interior by means of a suction-pump connected with a box provided with India-rubber packing, which is pressed against the other side of the wall.

Fifthly. When a building is erected on a slope, and the higher wall becomes saturated with percolating rain-water, a cure can only be effected by cutting a deep trench, and thus draining off the water. If substances containing nitrogen and conducing to the formation of saltpeter have been introduced into the wall through rain, the latter will continue to be damp, and the only palliatives against their injurious effects on the inside faces of walls are those already pointed out.

Since the above was written, the *Badische Gewerbe-Zeitung* has published a few additional remarks by Dr. Meidinger. It is stated that in several cases of dampness in walls a coating of oil paint, upon which subsequently tinfoil has been pasted, has been found efficient. The two substances combine very closely, and permit of the hanging of paper afterwards. The paint must, however, be put on only in dry weather, or after artificial drying of the wall, and the wet places have entirely disappeared. With regard to boarding of damp walls, it is added that it should not be neglected to asphalt the beading to which the boards are nailed, to protect them against the absorption of water and subsequent destruction. Moreover, the boards must not be too far away from the wall, on account of mice. The introduction of airholes is also to be recommended, experience having shown that in their absence the wood becomes fusty. It has already been pointed out that it is advisable to coat the boards with silicata paint, to prevent rotting. This little extra expense should not be spared, for it is by no means yet proved whether airholes alone will preserve the boarding; moreover, the introduction of openings for ventilation may be inconvenient. In any case, the naked boards must not touch the wet wall, as otherwise saltpeter would enter them and make them damp also. It would, perhaps, be advisable to remove the plaster wherever damp shows itself, before nailing down the boards. The wall would then absorb less moisture from the air, and would lose it quickly again in dry weather; under these conditions, the space between boards and wall would contain damp air for a shorter time, the presence of which is injurious in any case.

Finally, with respect to the introduction of an isolating layer between stones and mortar, we learn from a prospectus lately issued that a special putty, called Weissang jointing putty, has been introduced in Germany which appears to answer the purpose well. The mass, of the nature of asphalt, but without smell, is boiled with an equal weight of linseed oil, and put on as hot as possible. It is stated that about 2 lbs. of mixture covers one square yard of wall space. The mass is sold retail in Germany at 1.80 mark per kilogramme (11d. per lb.). As the price of linseed oil there is about 6d. per lb., to coat one square yard would cost 1s. 5d. The mixture is applied in a peculiar manner. The wall is stripped of its plaster, the joints being picked out deeply. The latter are then freshly set with mortar. After drying, the hot mixture is put on, and the wall is once thinly rough-plastered. When the latter has dried, plastering is proceeded with as usual. Under these conditions, a close connection of the plaster with the isolating mass is effected. The latter is recom-

mended also for the protection of gable-walls on the weather side against the penetration of damp; as a substitute for reeds (laths) in plastering on wood; for painting timber and ironwork in new buildings; for preventing the growth of fungus on wainscoting and other wooden linings; finally, for coating hoardings, garden-rails, barriers, posts, tree and vine stakes.

THE STORAGE OF POWER.

From "The Engineer."

PROBABLY at no previous period has there existed such a demand for what we may term portable motive power as that which manifests itself now. Power is wanted to propel street cars and trycicles, to drive washing machines and small lathes, to blow organs, to turn dynamo-electric machines, to propel small boats. We might extend our list considerably, but it is unnecessary, for to every reader of these pages a new field for the exertion of power will suggest itself, varying with the reader's tastes, habits, and ingenuity. There can be no doubt that the inventor who could supply in a really portable form a machine or apparatus which could give out 2 or 3-horse power for a day would reap an enormous fortune. Up to the present, however, nothing of the kind has been placed in the market. Sir W. Thomson startled the world—apparently a long time ago, for events move quickly—with the announcement that he had carried a million foot-pounds in something not much larger than a hat box. But it is really very doubtful if this has been approached in practice. The secondary battery is still a baby. What it may become no one can quite tell. Putting it on one side, we may proceed to consider what are the obstacles which stand in the way of the production of portable power; and, first, it will be well to define precisely what it is we are speaking about. A locomotive or marine engine supplies an example of portable power in the fullest sense of the word; but we do not refer to locomotives or marine engines, but rather to something partaking in its nature of a watch—a something which may be wound up or charged with power in one place, and discharged or run down in another. Thus, for example, in the case, let us say, of a small ivory turnery, a cart would bring to the door every morning a box which would contain power for the day, and would take away the run-down or discharged box to be refilled. The patron of trycicles would but have to send to the nearest place where they sold power boxes to procure what he wanted; and thus equipped he might make a trip of twenty or thirty miles, only using his legs for the sake of exercise, and permitting his power box to do all the drudgery. Here, we think, is a sufficently attractive picture; why is it incapable of realization? Shall we never see such things done? Shall we never see such an announcement as this in the daily papers?—"Messrs. Dyne, Erg & Co., Power Merchants; power boxes of the most approved construction wholesale and retail. Messrs. Dyne, Erg & Co. solicit the particular attention of tricyclists to their new invincible drivers, guaranteed to supply half a horse-power for ten hours at the cost of one penny per hour; weight 30 lb. The lightest for the power in the market."

We may point out to begin with that there is absolutely no difficulty whatever in storing power. Every time a watch is wound power is stored. The difficulty lies then not in storing power but in storing enough of it. In the present day we have learned to talk so glibly of horse-powers that we fail to realize the magnitude of that about which we speak. It is not probable that a tricycle or other road vehicle to carry, say, four persons, could be made which will weigh less than 4 cwt. If we add an equal weight for the power box, and 6 cwt. for four persons, we have a total of 14 cwt., and this cannot be successfully propelled at, say, seven miles an hour on a good road with less than half-horse power net. This means 16,500 foot-pounds per minute, 990,000 foot-pounds per hour, or in six hours, in round numbers, 6,000,000 foot-pounds. The power expended on a trip of some forty

miles would suffice to lift 2,678 tons a foot high; it would carry 7 tons to the top of St. Paul's Cathedral. All this power could be got out of 12 lb. of coal; but, unfortunately, not under the conditions. Yet it will be seen that to stow away 2,678 foot-tons of power in a box weighing not more than 4 cwt. is not an easy matter. Three different methods of doing what is wanted at once suggest themselves, but they all depend on the same principle—elasticity. We may wind up springs; or we may compress air; or we may use heated water. There is a fourth way, concerning which we may say something presently. For the moment we shall content ourselves with dealing with the obvious, leaving the more recondite for future consideration.

We have the conditions of our problem laid down. Wanted 2,678 foot-tons stored in an apparatus which, with all the gear necessary to cause the revolution of the wheels, shall not weigh more than 4 cwt. Springs suggest themselves in an instant —springs to be wound up by a stationary engine. A company has been formed in the United States for supplying spring power for tramcars. Why not extend the principle? Let us see what figures can tell us on this point. It is tolerably clear that our spring must be strong and therefore heavy, and it may be conceded perhaps that the gearing to drive the road wheels cannot weigh less than 1 cwt. If, on the other hand, the spring were attached directly to the driving axle, the moment the brake or other stop was released the vehicle with its occupants would begin a headlong course, not unlike that of the American dog who was tied by his master at the tail end of an express train, "because he was used to being led." Some equalizing arrangement must be introduced analogous to the fusee of a watch. A brake could not be employed because it would simply waste power. We have, then, let us suppose, 3 cwt. of spring. But if a spring is to last for some time without breaking it must have no more work stored in it than would suffice to lift it about 60 ft. Let us strain a point, however, and suppose that our 3 cwt. of spring could lift 3 cwt. 100 ft. high; then we have 33,600 foot-pounds, or just power enough to propel our vehicle for two minutes instead of six hours. If we look at the problem from another point of view, we find that no less than 27 tons of spring would be required to supply half a horse-power for six hours. Springs of steel are quite out of the question.

Next comes compressed air, and this is more promising. Indeed, compressed air has been used by several engineers with comparative success, in propelling vehicles; but it is open to question whether in this way we can obtain what we want. We must have an engine to enable the compressed air to do its work. This, with its appurtenances will weigh, say, 1 cwt., leaving us 3 cwt. A steel cylinder 6 ft. long and 2 ft. diameter could be kept under 3 cwt., and yet be strong enough for our purpose. If it were filled with air compressed 31 times, it would contain 4,500,000 foot-pounds. But unfortunately this only represents the power which would be absorbed in compressing 580 cubic feet, or about 45 lb., to 500 lb. or thereabouts on the square inch, and during the compression the air would lose much heat, which would have to be restored, or the loss during expansion would be enormous. It would not be safe to reckon on more than a three hours' run from our air spring, and, considering the difficulty of losing air at such an enormous pressure; the cost of pumping it up to the stated density; and the risk inseparable from its use, it is tolerably evident that compressed air will not supply what is wanted.

We have next to consider what may be done with water on the system devised by Lamm, among others, and used for some time in the United States with success. The system would have to be materially modified for use on a small scale. We may take it for granted that at each stroke of the engine a few drops of highly-heated water would be injected into the cylinder, where they would flash into steam. Now, water under an absolute pressure of 270 lb. on the square inch has a sensible temperature of 408 deg., and if we relieve the pressure, part of the water will be converted into steam, the water falling to the temperature normal to the new pressure. Thus if the new pressure were 50 lb. absolute, the temperature would be 281 deg., and $408-281=127$ deg. units per pound of water; and each pound of steam would require 916 units

for its formation; and $\frac{916}{127}=7.29$. That is to say, for each pound of steam produced we must caary 7.21 lb. of water. But half a horse-power could not well be got from a small non-condensing engine for less than 30 lb. of steam per hour, or for six hours 180 lb., and 7.29 + 180 = 1,297.8 lb. of water. If the pressure were greatly augmented the quantity of water would be diminished; but the strength of the containing vessel and its weight would have to be enormously increased. Under the conditions, it will be seen that it would not be practicable to obtain a run of more than about one hour or, say, seven miles. But there would be very considerable advantages on the side of steam, or rather hot water storage. The pressure would not be more than half that of air, and there would be practically no difficulty in keeping the water reservoir hot for three hours or more by enveloping it in suitable coatings of felt. We have, as far as possible, avoided the use of figures or recondite reasoning of any kind, our purpose being to show in the simplest and briefest way that it is apparently impossible to devise any mechanical system of storage which will give out half a horse-power for six hours and weigh less than 4 cwt. or 5 cwt.; and it will be conceded that half a horse power is a very small thing and not competent to effect much. It probably represents the actual work performed per day by a London omnibus horse weighing 9 cwt.; the horse during a part of his working time does much more, but for the remainder he does less. A horse weighing 9 cwt. to 10 cwt. will plough steadily, if well fed, for ten hours a day, and probably develops during that time about 21,000 foot-pounds per minute, or, say, two-thirds of a horse-power; but it does not appear that in this direction we can at all rival nature in providing portable power.

One method of storing power, and only one, remains for consideration, and it is worth it. At first sight, no doubt, many of our readers may reject the idea as representing expedients too full of danger to merit notice. Without further preface we may say that we refer to the use of explosives under proper conditions. A gunpowder engine was made and worked many years ago, and with some success—and another gunpowder engine is now being tried in Germany—but chemistry has made enormous advances since then, and various compounds exist which, while weighing little, and capable of exerting an enormous force, may be burned slowly and without danger. The power stored in gunpowder is very great, amounting to about 70 foot-tons per pound, so that about 40 lb. would suffice to give out half a horse-power for six hours; and by using the principle of subdivision it may not only be burned without danger, but even when carried in moderate quantities, rendered quite safe from exploding; but we do not propose the use of gunpowder as a means of providing portable power. The chemist may help us to something safer, cheaper, and quite as powerful. Gun-cotton, for example, may be burned without exploding, giving off enormous volumes of gas. It remains to be seen, however, whether compressed gas and air may not be burned, as in the existing gas engine, to supply what is wanted. We believe we are correct in stating that gas engines have been tried, and with success, for driving tramcars, although very little has yet been said on the subject. It will be seen, however, that gas cannot supply all that is wanted, nor, indeed, does its use come quite within the scope of this article. Gas is laid on to most houses now, and gas engines are plenty enough, yet they do not quite meet the want which a storage battery may yet, perhaps, be made to supply; and nothing has yet been devised in this line which would be suitable for the propulsion of light vehicles, such as that to which we have given prominence in all that we have just said.

The high percentage of phosphoric acid in the cinder obtained in the basic Bessemer process has suggested the possibility of using it for agricultural purposes in the place of phosphate. A German contemporary gives the results of some investigations made at a large steel works in Westphalia, the cinder from which contains, of silica, 6.20 per cent.; carbonic acid, 1.72; sulphur, 0.56; phosphoric acid, 19.33; iron, 9.74; manganese, 9.50; lime, 47.60; and alumina, 2.68 per cent. The result of tests was, that this cinder would do well as phosphate manure, and that it will not be necessary for this purpose to treat it with sulphuric acid, because a considerable portion of the phosphoric acid is in a form which will allow it to be assimilated readily.

STEEL FOR STRUCTURES.

By EWING MATHESON, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

During the last ten years the use of steel has increased so continuously, and experience in its manufacture and application has accumulated so fast, that the time seems to have come when the precise position arrived at may be stated with advantage, and some apparent anomalies cleared up. Keeping strictly to the subject of the present paper—steel for structural purposes—it is worth while to inquire why, seeing that the objections and uncertainties that hindered the use of steel for shipbuilding and boilers have been entirely removed, it is nevertheless only used for bridges in exceptional cases, and hardly at all for roofs and buildings.

It is not within the scope of this paper to deal with the preliminary question as to what steel is. It is sufficient to say—paraphrasing a similar clause in Mr. Hackney's paper in vol. xlii. on "The Manufacture of Steel"—steel for the purposes of the present paper, is any variety of iron or alloy of iron, which is cast, while in the liquid state, into a malleable ingot, and, to go further, which will, when rolled into a plate or bar, endure from 26 to 40 tons per square inch before fracture.

As a convenient way of drawing attention to the subject and of eliciting opinion, the author has attempted, in the following series of propositions, to summarize what, in his opinion, is the present state of the case, and in the remarks that follow to elucidate them.

1. Rolled plates, and bars of the various forms required for structures, are now manufactured in steel with as much certainty in regard to quality as iron of the first class.

2. Advantages, in regard to size and weight of pieces, can be obtained in steel which in iron are either impossible or can only be secured at considerable extra expense.

3. Steel has a superiority in strength ranging from one and a half to twice that of iron, and at the same time a more than proportionate superiority in ductility and elasticity.

4. Steel can be manipulated in the factory; bent, straightened, cut, planed, drilled, and punched with the same tools and processes as are used for iron, and for the most part without extra force.

5. Protection against rust is of more importance for steel than for iron, but if treated in the same way as is usual with iron, steel is less liable to waste by rust.

6. Owing to the above advantages, structures of steel are superior to those of iron; but economically it is only in some instances in regard to ships, and in still fewer cases in regard to bridges, that there is at present any pecuniary advantage in using steel.

7. This limit to the application of steel is due partly to official rules which restrict the working strains on steel, and partly to exigencies of design, which hinder the reduction in size and weight of members to the extent which superior strength might otherwise allow.

8. Although for the above reasons steel structures may cost more than iron without any immediate compensation, yet, if measured by actual units of strength and durability, steel is cheaper as well as better for all but very small structures.

9. The employment of steel may be encouraged and extended by a fuller knowledge among users of its qualities, by facilities for verifying these qualities, by exercising a wider choice of the kind of steel suited to the purpose in view, and by such a liberal alteration of the present official rules as will allow fuller advantage to be taken of steel than is usual or permitted at present. The simplicity of manufacture, as compared with that of rolled iron, renders almost certain a nearer approximation in cost, if by a wider permission the demand for steel should increase.

Taking the foregoing propositions in their order, they may be further elucidated as follows:

1. All the plates and bars of various sections used in iron structures are obtainable also in steel; and if, either in large or in small sections, there is a limit to the variety obtainable, it is due only to a limited demand which restricts the number of rolling mills. In a new industry like that of steel, there is naturally the disadvantage that there are fewer makers of it than there are of iron; and if a small quantity of any section is wanted there would be more difficulty in obtaining it in steel than in iron.

In both there are some forms which are more difficult than others to roll, and this especially applies to sections wide as well as deep. The wide deep bulb beams, of which large quantities are made for shipbuilding, require skill and care; a deep and wide channel bar still more; and perhaps the worst section for rolling is a deep wide H section; it being probably for this reason that steel joists, which would be extremely useful, and of which large quantities could be employed in buildings, have not yet become an ordinary article of commerce. Even in iron, deep channel and H sections, if wide as well as deep, demand special iron. The difficulties in rolling have sometimes to be met by a combination of side rolls, or by building up separate bars into a form approaching that of the desired section, the pieces being welded together as they pass through the rolls, and finished by other rolls of the final form. Belgian iron seems peculiarly adapted by its plastic nature to wide and difficult sections. Hence it is that so many joists and channel bars of iron, inferior in all other respects, are imported into this country. As H beams of steel have, however, been made in this country as well as on the Continent, it would be interesting if the makers could tell what probabilities there are of these forms being commercially successful.

The uncertainties and accidents which occurred in the earlier days of steel-making have left prejudices in the minds of some persons who, by not having occasion to use, or notice the use of, steel in recent years, have not acquired experience in its quality. But the vast quantity of steel which, during the last few years, has entered into the construction of ships and boilers, and the small proportion of faulty pieces—a proportion much smaller than is usual in iron—has proved incontestably that steel may be relied on. It may be asserted that, at the present day, out of a given number of steel plates by experienced makers, there would at most be no more, and probably there would be fewer, defective ones than among a similar number of Yorkshire plates of the highest quality. It would, however, be a great mistake to suppose that defective steel plates are now unknown. In this, as in other trades, competition and low prices may tempt makers to use inferior material, or to cheapen too much the processes of manufacture, the evil results showing, not necessarily in bad or weak steel, but in steel of a wrong kind. If an engineer's design depends on steel of a certain strength and elasticity, his plans will be disarranged and the margin of safety in the structure reduced if the special qualities he is reckoning on be not maintained throughout; or, in other words, if successive parcels of steel sent from the rolling-mill differ. The structure made of them will be unequally strained in consequence. But while unremitting watchfulness is as necessary as ever, the mystery which at first attended the manufacture and behavior of steel hardly any longer exists, and the precautions necessary to success are well known. Every stage must be properly watched; the use of suitable ore and of pure fuel, a period in the converter appropriate to the kind of iron, the casting of sound ingots, and examination during the reduction of the ingot to see that no part of the metal having cracks or flaws is allowed to go forward to the rolling-mill.

Among other points it now appears that the quality of steel depends, like that of iron, upon the amount of working it receives, and that the thickness of ingot must, therefore, be proportioned to the thickness of the plate or bar to be rolled from it. With this proviso, there is room for much difference in regard to the manner of reducing the ingot, and hammering is not considered necessary by some makers. The supposed brittleness of steel, its liability to injury from punching, and the risks of damaging it in heating and welding arose when, in the first possession of a novel material, steel-makers and engi-

neers alike endeavored to use steel of a strength and hardness associated with name, or, not being acquainted with the nature of the material, damaged it by erroneous treatment.

2. The process of steel-making, by which the masses of metal to be dealt with are produced in the first instance simply by casting, allows the production of plates and bars of weight and size much greater than are usual in iron, where the piling and intermediary treatment of large masses are troublesome and costly. Thus, in the case of iron plates, extra prices begin and increase at a rapid rate after an area of 30 feet, or a weight of 6 cwt. is reached, although this is a rule which varies in different districts, the limit being 8 and even 9 cwt. in some districts. But a steel ingot as usually made in England for rails weighs from 30 to 35 cwt.; and although for bars and plates much smaller ingots are manufactured, the full net weight which can be rolled from a 25-cwt. ingot is obtainable at ordinary prices, unless extreme thinness or great area be demanded.

Thus a plate, 18 feet long, 3 feet wide and ¾ inch thick, is within the ordinary limits, and if a slight extra price be paid a ¾-inch plate 48 feet long by 4 feet wide can be rolled; or in the case of ½-inch plates a length of 16 feet, and a width of 4 feet 6 inches can be given without extra price. That is to say, the limit in area is 70 feet in steel as against 30 feet in iron, and the limit of weight is 15 cwt. as against 6 or 9 cwt. in iron.

In the sizes of T, L, and other bars, there are differences similar to those in plates. In iron, the limit of section obtainable at the ordinary price is 8 united inches, and the limit of weight for any one bar is generally 4 cwt., the price per ton rapidly rising as the weight is increased. In steel the limit of section for such bars ranges from 9 to 10 inches, and beams as heavy as 15 cwt. are supplied at ordinary price. When long and heavy iron bars are required, the piling and rolling become very troublesome. For round bars an extra price per ton is incurred when a diameter of 3 inches, or a weight of 6 cwt., is exceeded. Therefore, solid piles of wrought iron are expensive; those 5 inches in diameter can only be rolled in short lengths, and larger sections have generally to be prepared under a steam-hammer and welded, while in steel the limit of length would probably be as high as 40 feet. If solid screw-piles for a pier or bridge be required, they would be about as cheap per ton in steel as in iron, and if not quite so cheap it would not be because the steel cost more, but only because there are few makers who have rolls of the desired size, and the competition would be limited. And if, for a particular purpose, long and large pieces are required, piles 7 inches in diameter and 40 feet long, weighing 2½ tons, can be made in one piece without welding, though at some extra cost.

To the engineer there is the obvious advantage in having long and large pieces at command, that the number of joints is diminished and the weight of material, the cost of workmanship, and the inconveniences in design, which must arise from jointings, are also reduced. Indeed, the exigencies of transport rather than difficulties in manufacture seem to limit the area and weight of pieces in steel.

3. In comparing steel with iron in regard to strength and elasticity it is sufficient, for the present purpose, to accept those results which have gradually been obtained during the last few years, and which form the basis of the rules now established by the various official bodies, although the rules themselves may be modified with advantage. The larger share of the experiments by which these results have been elicited in this country have been connected with shipbuilding and boiler-making, and the desingers of bridges or roofs have ready to their hands the experience thus acquired.

The ductility or capacity for elongation, which, in mild steel of 30-tons breaking strain, is, on a test piece 8 inches long, about 25 per cent. on the original length, rapidly declines when that strain is increased, and when a resistance to fracture of 40 tons is reached the ductility is only about 12 per cent. For boilers and, to a somewhat less extent, for ships, where, as will be presently referred to, ductility is particularly necessary, very mild steel is used. When the choice has to be made between iron and steel for fixed structures, the low tensile strains which are permitted for steel, and which are appropriate to soft

steel, so far neutralize the advantage obtainable, as at present to hinder the adoption of steel in many cases where it could be used with advantage.

The Admiralty specification for plates and beams requires:

"Strips cut lengthwise to have an ultimate tensile strength of not less than 26 and not exceeding 30 tons per square inch of section, with an elongation of 20 per cent. in a length of 8 inches.

"Strips cut crosswise or lengthwise 1½ inch wide, heated uniformly to a low cherry-red and cooled in water of 82° Fahrenheit, must stand bending in a press to a curve of which the inner radius is one and a half time the thickness of the steel tested.

"The strips are all to be cut in a planing machine, and to have the sharp edges taken off.

"The ductility of every plate or sheet is to be ascertained by the application of one or both of these tests to the shearings, or by bending them cold by the hammer.

"All steel to be free from lamination and injurious surface defects.

"One plate or sheet to be taken for testing from every invoice, provided the number of plates or sheets does not exceed fifty. If above that number, one of every additional fifty or portion of fifty. Steel may be received or rejected without a trial of every thickness on the invoice.

"The pieces of plate or sheet cut out for testing are to be of parallel width from end to end or for at least 8 inches of the length."

The Admiralty tests for bars and beams of various sections are the same as for plates, with the addition of some forge tests, and the strips are all cut lengthwise.

Lloyd's rule is slightly more liberal than the Admiralty rules, as the minimum and maximum strains are each raised 1 ton, the range being from 27 to 31 tons. The rules of the Liverpool Underwriters' Registry give a range of from 28 to 32 tons. Under these conditions steel, rejected by the Admiralty as being too hard, may be utilized for vessels built under the Lloyd's or Liverpool rules.

The rules of these three official bodies are alike in this, that they make no difference between plates, T or L bars or beams, and they assign maximum as well as minimum limits of tensile strength. In both these respects there is a notable difference in the rules of the French Admiralty, which graduate the minimum strain according to the thickness of the pieces and sectional profile of bars, and which prescribe no limit of maximum strength, provided that a certain ductility is assured. The French rules demand higher minimum strength than any of the three English bodies, and it is only for plates more than ¾ inch thick, that so low a limit as 28 tons per inch is prescribed, while for plates between ¼ inch and ¾ inch which include the majority of those used in structures, the limit is about 28½ tons per inch. That is to say, it is only when ¾-inch thickness is exceeded, that the minimum limit is as low as the highest of the three English rules just given, namely, the 28 tons of the Liverpool registry. An elongation on an 8-inch test piece of 20 per cent. before fracture is prescribed by the French rules, and manufacturers have free scope to give the highest strength which such ductility will allow. As a matter of fact, the steel used in the French navy is stronger than that in the English navy.

The steel as above described is superior to ordinary good iron in all its known qualities, namely, in strength, elasticity (the proportion which the limit of elasticity bears to its strength), and ducility. Referring to the shipbuilding steel made according to English rule, it will be seen that its average strength is 28 tons, elasticity of $\frac{18}{28}=0.64$, and ducility$=0.25$; while the strength of good wrought iron is 21, its elasticity about 0.52, and ductility about 0.15. The capacity of this steel to resist fatigue is not clearly known, but may be inferred with much probability from what has been already ascertained. In this respect, fatigue may be defined as the diminished resistance to fracture which comes after repeated application of strain, especially after strains varying within a wide range. Thus to take an extreme case, the strains on a railway axle tending to bend it backwards and forwards have a wide range; so also have those on a piston-rod being first in compression and then in tension; and in some parts of continuous girders there are fluctuations from plus to minus strains.

The bridge engineer in this country is

under very different rules to those which control the shipbuilder; for while the latter has the quality of material very carefully prescribed for him by the Admiralty or Lloyd's, the Board of Trade, which controls railway bridges, restricts the working strains apparently without any regard to the wide range of difference possible in steel. At present there is a hard and fast rule that steel shall not be strained in tension to more than $6\frac{1}{2}$ tons per square inch, or an increase on the 5 tons prescribed for iron of 30 per cent. Although at first sight this seems an arbitrary regulation, it must be acknowledged that there are difficulties in the way of more elastic rules, but this acknowledgment only points the way to overcoming the difficulties. It should be remembered that the Board of Trade officials have never taken upon themselves the function of verifying the quality of the iron used in bridges, and in allowing a working strain of 5 tons upon iron it is on the assumption, doubtless, that this will be safe even with the worst iron that can be made. The much wider range of possibilities which exists in regard to the quality of steel is one of the difficulties hindering the use of steel.

The improvements in manufacture of late years in the case of rolled steel have been accompanied by improvements equally great and import in the making of steel castings. Steel is more difficult to cast than iron, for the ordinary risks of the ironfounder are increased. A head of metal is necessary even more than in iron to ensure soundness, but this alone will not suffice unless the steel be of the right mixture, or of the proper temperature, or if the composition and condition of the mould are not suitable. Moreover there is the difficulty that the steel may become chilled before it has filled the mould; and in cooling from the high temperature of molten steel there is a contraction which, in articles of certain shape, may draw the castings entirely or locally asunder. There is also the risk that where absolute soundness is demanded a little extra silicon in the mixture will help to give it, but at the expense of ductility. Indeed, if strength and ductility be assured, a user will do well in not objecting to trifling specks in the casting which would be wanting in iron. But while these difficulties are mentioned, experience has taught that within certain limits of form trustworthy steel castings may be obtained. The advantages afforded are great, though they benefit the machinist more often than the builder of fixed structures, but there are cases where cast steel girders are extremely useful.

Till recently an objection, insuperable in many cases to the use of steel castings, was their want of ductility; but now, while they afford all the advantage of shape which a casting alone can give, as well as a tensile strength four times that of cast iron, there is also the toughness and ductility of wrought iron. That is to say, following nearly the proportions found in rolled steel, castings up to 10 tons weight will have a tensile strength of 28 tons, a limit of elasticity of 13 tons, with an elongation before fracture of 20 per cent.; and when the steel is hardened in oil to a strength of 40 tons, a power of elongation in a test piece 2 inches long still remains of 15 per cent. If the soundness and toughness of steel castings be thus assured, they become available for the compression members of trussed girders, although some reduction in the present price will be necessary to admit of their adoption in this way. There is a dislike, it may almost be said a prejudice, among engineers against the employment of cast iron for this purpose, but when properly disposed it is very useful. The author may, as an example of this method, refer to a bridge of three 150-feet spans, designed about fifteen years ago by the late Sir Charles Fox, M. Inst. C.E., for carrying a single line of railway and a carriage road over the River Bremer in Queensland. In this bridge the upper members of the girders are each composed of a cast-iron tube, and it can hardly be doubted that in girders composed mainly of rolled steel, cast steel if it could be obtained at moderate cost would be preferred to cast iron. The same method is available for roof trusses. One of the strongest and yet cheapest roofs known to the author is that over the railway station at Amsterdam, designed by Mr. R. M. Ordish. The span of this roof is 120 feet, and the compression member of each truss is composed of cast-iron tubes.

4. The experience of the last few years in the building of iron ships and girders has shown that very little alteration is

necessary in the appliances of a factory to work steel instead of iron. Indeed, as compared with iron of ordinary quality, which is often brittle, mild steel works more "sweetly" under the cutting tool, and with less risk of spoiling the material. It has been incontestably proved that the mild steel used in shipbuilding may be safely punched, and that where the thickness does not exceed ½ inch there is no necessity for after annealing. If the steel be thicker than ½ inch it is desirable to rimer out the hole after punching, and beyond ¾ inch the holes should be drilled. Much depends upon the arrangement of the punching-machine, and it is probable that, by the improved methods now being introduced, the limits within which punching may be safely performed will be extended. The experiments of Mr. Parker, engineer-surveyer of Lloyd's Register, and Professor Kennedy have shown that the punching of steel, though it reduces the ducility of the metal round the hole, does not reduce its strength; and if the ductility be restored, or if—what is generally more convenient—the hard metal be rimered out, the rest of the piece is unaffected. But while these experiments explain the facts in a most interesting way, the facts themselves depend, not on laboratary tests, but on the use of thousands of tons successfully applied to ships and boilers. It is only necessary to state that at the great shipyards of this country punching is permitted by the Admiralty and Lloyd's inspectors within the limits named above. For hydraulic smithing and flanging in dies the steel not only allows of the operations usual in iron, but also admits of the more difficult shaping into cupped forms, which can only be endured by Yorkshire iron of the highest quality, costing twice as much as steel.

The steel rivets used in steel structures much resemble the finest charcoal iron in their ductility. They can be readily upset and made to fill the rivet-hole, and there is less risk than in iron of cracking the head in closing.

In steel as in iron, welding should be avoided as much as possible, especially when a direct tensile strain is to be borne, as in the tie-rod of a roof-truss or on the tension link of a girder. But in the T or L frames of a girder, or in the frames of a ship, welding is often unavoidable, and may be adopted without risk. While, however, steel has proved to be workable in the operations above mentioned, there is undoubtedly a difference in behavior between steel and iron. If steel be unnecessarily heated, or if one part of a plate be cooled suddenly while another part of the same piece is kept hot, initial strains may be set up which will tend to the starting of fractures from holes afterwards made in the piece. Steel demands a difference of treatment; this remark especially applying to the smithing and welding of steel. It may indeed be said, that in dealing with a new and superior material, a modification of old methods is necessary, which is soon learnt by intelligent workmen, who readily adapt their methods to the steel just as a smith or other workman who has been accustomed to manipulate one class of rolled iron has to alter his practice when another class of iron is presented to him. A smith accustomed to work inferior iron is almost as much perplexed when set to work on good iron as in the opposite case, and it is obvious that a diminution in strength arising from improper treatment is of consequence proportioned to the original strength of the material, and therefore a more serious matter in steel than in iron.

5. The liability of steel to rust was dealt with by Mr. D. Phillips, M. Inst. C. E., last session; but as the subject was there almost exclusively confined to ships and boilers, the behavior of steel when exposed to the weather was hardly touched on. The protection of steel from rust is of more importance than of iron, because of the higher value of the material to be protected, and the greater loss of strength which the wasting away of a certain thickness implies. For instance, if the floor of a bridge be made of ¼-inch steel-plates instead of ⅜ iron-plates, the wasting away of $\frac{1}{16}$ inch will in steel reduce the strength 25 per cent., while in iron the same waste will only reduce its strength 16⅔ per cent. The preservation of iron from rust is not in this country sufficiently considered. It will be an advantage if, owing to the importance of the question when steel is to be treated, the methods of protection are more carefully considered for steel and iron alike. Modern mild steel has not yet been used long enough to afford any precise knowl-

edge of the subject; but it is probable that, in regard to corrosion, steel is less vulnerable than cast iron.

The author touched on this question in the discussion on Mr. Phillip's Paper, and ventures to draw attention to it once more. Leaving out the few examples where iron or steel are protected from rust by being built into brickwork or concrete, paint is the universal protector. Proper painting in the first instance and accessibility for painting afterwards are the two essential conditions: unfortunately in iron structures both are very often unobserved. The accessibility for future painting is frequently ignored in a design, and narrow crevices or spaces are left into which the paint-brush can hardly enter; painting is not often enough repeated, and the scraping before the new paint is applied not effectually performed; the railway bridges and roofs in the neighborhood of London afford numerous examples of this neglect.

It cannot be too forcibly stated that it is impossible to protect rolled iron properly by paint without the removal first of the black oxide scales on the surface, and it is interesting to know that this preliminary process is an advantage also in steel. At Crewe, all plates used for boilers and other parts are washed with sal ammoniac. In ships the necessity for preliminary treatment of the steel is even greater than in boilers, as in salt water a galvanic action sets up between the black oxide on the surface and the iron within, which would soon result in corrosion. Therefore it is now the custom in all shipyards carefully to remove the scales from the steel plates before they are painted.

The occasional peculiar pitting or rusting into cup-shaped holes of plates of steel as well as of iron has never been satisfactorily explained, and the chemical composition of the plates to which this is due demands investigation. The author will not further repeat here what he has already stated in regard to the proper preparation for painting, but would point out that steel, having been cast into an ingot with a skin much resembling that of cast iron, the subsequent rolling, while it does to some extent substitute for the original skin the scales which form on rolled iron, yet possesses to the last a greater homogeneity than piled wrought iron. Moreover while rolled iron contains from 3 to 4 per cent. of slag and other impurities, steel contains only ½ per cent., and while in iron the rust eats into the laminations of the pile and leaves room for more moisture to enter, in steel there is a homegeneous substance, presenting a more solid front, which has to be eaten away gradually, and the granular rust which forms on steel is itself somewhat of a protection against a further advance; at any rate, the rust probably advances more slowly; in this respect being like zinc, which is protected by the oxide formed on its surface when exposed to the weather. The naked appearance of iron is well seen when a piece that has long been submerged in the sea is recovered, somewhat resembling iron which has been pickled in dilute acid.

6. Steel having been proved to be stronger, more ductile, and more elastic than wrought iron, and probably also being better able to endure the fatigue of repeated strains, as in a railway bridge, it is *per se* better than iron for bridges. But as in practice the relative value of two things can only be arrived at by comparing the cost also, it will be found on investigation that at present, and under the existing order of things, steel can, as a matter of strict economy, replace iron in comparatively few cases. Before analyzing this further, attention may be drawn to the difference between ships and stationary structures in this respect. Ships are at times subjected to strains so peculiar and so severe that it is almost impossible to calculate their extent beforehand. A ship supported at one moment upon all sides by water, and at the next, spanning like a bridge (and with an enormous load) a chasm between two waves, has to withstand strains of a kind never imposed upon bridges. It is not improbable that the loss of iron ships from leakage is often due to their having been strained in some of their parts beyond the limits of elasticity, whereby they have been permanently distorted. In the case of collision and leakage, the internal bulkheads may have to do duty as the walls of tanks to resist the pressure of water. The hull is often bumped against other vessels or against piles, to say nothing of rocks, and the plates and

beams are subjected to concussion which would break up iron, and yet may be withstood by ductile steel.

Ships are built of steel for three principal reasons. There is the simple economical reason that a vessel built of steel saves so much in weight that she has an additional weight-carrying capacity of about 10 per cent. Therefore, even though the capital expenditure be greater than for an iron vessel, the gain in revenue more than compensates for it. Then there are vessels where reduced immersion is of importance; and vessels of this kind being frequently the property of high-class mail companies have the further advantage of greater strength and elasticity. Finally, there are the warships of the country; and here the justification for steel is simple, the very best is wanted without regard to comparative cost, or, at any rate, within the limits of cost which modern steel allows.

For steam-boilers the advantages obtained by the use of steel are even greater than in ships This has been most manifest in marine boilers, where the greater strength allows a steam pressure one-third higher than formerly, thus affording greater power with less proportionate consumption of fuel.

The circumstances are very different for bridges. The maximum strains on a road or railway bridge can be calculated with much greater accuracy than those on a ship; and if, for the discrepancies which must always arise between actual and calculated strains, for the wasting by rust and for fatigue, the ordinary margin be left, no more seems necessary, and bridges, not being liable like ships to indenting blows, need not be of such ductile material.

At present steel costs, in proportion to iron, just about as much more, say as 13 to 10, as the strains permitted by the Board of Trade on steel bear to the strains allowed on iron, namely, as 6½ to 5, though this difference in price becomes less where, as already described, large or heavy pieces of steel are required, this naturally occurring oftenest in large or heavy bridges. There is the advantage, of course, in steel, of less weight for transport, but the cost of carriage, not only in the country of production but by sea to distant countries, is so cheap that this is of little consequence, except where land carriage is expensive, as in South Africa, or over mountainous country. There is, again, an advantage in the case of swing or movable bridges, where, if there be an overhanging structure to counterbalance, the saving in weight is of great importance, as well as the saving in force required to move the bridge.

A further consideration is that the weight of metal in a bridge has itself to be carried, and that a saving in the weight allows again a saving in the weight of material required to sustain it. This sort of compound saving makes itself felt according as the proportion which the metal bears to the moving load plus the road material, becomes greater. If, for example, the weight of iron in a 50-feet span single-line lattice bridge is 14 tons, and the moving load plus road material 77 tons, the proportion is as 1 to 5.5, and the proportion of iron to the total load 1 in 6.5. If, on the other hand, the weight of iron in a single line bridge of 500-feet span is 1,000 tons, and the moving load plus the road material is 600 tons, the proportion is as 5 to 3, or the proportion of iron to the total weight 5 to 8. Making, now, these bridges in steel with the Board of Trade limit of 6½-tons strain per square inch, and assuming for a moment that the sectional area of every part could be reduced in proportion to its greater strength, and in proportion to the strain, then the weight of steel in the first case would be 10.3 tons, and in the second case 555 tons.

In the first case there would be a saving in weight of 24 per cent., and in the second case one of 44.5 per cent. The cost of workmanship per ton may be taken at rather more for the steel than for the iron because divided over a less tonnage. If, therefore, the price for the ironwork for the 50-feet bridge, including transport and erection, be taken at £18 per ton, and for the steelwork £22 per ton, the cost would be

	£
In iron	252.0
In steel	226.6
Or a saving of	10 per cent.

The cost of very large bridges is generally more per ton than of small bridges, and, therefore, if the price of the 500-

feet bridge be taken at £21 for ironwork as against £27 per ton for the steelwork, the cost would be

	£
In iron	21,000
And in steel	14,985
Or a saving of	31.5 per cent.

But while the foregoing figures may be useful as a starting-point, the saving there represented cannot be realized in practice. Taking, first, the 50-feet span, if the types of iron girder-bridges usually adopted in this country be dealt with, and a saving attempted by reducing the sectional area of the parts, it will be found that no advantage in cost can be obtained on the basis of the foregoing rates per ton till a span of about 150 feet be reached. Not only have the direct strains to be provided for, but the compression members must be stiffened against collapse, and, therefore, if less sectional area be given to an upper flange it will be found necessary to provide extra bracing. Then, again, the rivets cannot be reduced in their total area because steel rivets are little, if any, better than the high-quality iron rivets used in iron bridges, and the group of rivets must remain nearly as large. In small spans, moreover, where the thickness of parts is small even in iron, a further reduction for steel makes the question of rust important. For instance, an engineer would hardly like to make any part thinner than ¼ inch. From this point of view an increase in the working strains from 6½ tons per inch to 8 tons would be of no service. It will become necessary, if the advantages of steel are to be utilized, that the designs of girders must be modified, even though the cost of workmanship be somewhat increased. In iron girders of small span no benefit is gained in adopting the forms which theory might prescribe as best. That is to say, parallel girders with flanges formed of plates and L bars are adopted, because the simplicity of design more than compensates for an excess of weight over more symmetrical designs; or, in other words, because sufficient metal can be given to them at less cost, for instance, than in bowstring girders of less weight with tubular or trough-shaped members. Probably because iron is so cheap in this country there has been less tendency than in Germany and in the United States to save weight by elaborate designs. When large spans are in question the more appropriate shapes are demanded and given to iron bridges, and the advantage of doing this for small spans also with such a superior material as steel may become apparent. Then perhaps trough-shaped or box, or even cylindrical, members will be used so as to give stiffness with steel of moderate thickness. In the bridges built by Mr. Brunel, still to be seen, these principles were adopted as far as the limited appliances and shapes of iron then at command allowed, but the heavier and less symmetrical forms have proved cheaper. It can hardly be doubted that the adoption of steel will encourage such modifications in design as will allow that reduction in weight which is impossible in small girders of the ordinary kind. Instead of 150 feet being the limit at which the saving could commence, probably while a slight reduction in weight was possible, even for spans shorter than 50 feet, the saving in money would begin at 100 feet, the most favorable opportunities occurring when heavy loads had to be carried.

Taking, now, the two examples again, it will be found that while some parts can be reduced to the full extent, others can only be partially altered, and some not at all; and that the portion affected is smaller for the short span than for the long span, because it is easier in the latter to give to the parts exactly the sectional form and area which they require than in the short span, and as has been mentioned, large pieces are more readily obtained of steel than of iron, and the saving in connections can be fully realized. In the 50-feet bridge the proportion of the total weight in which the reduction can be made will probably be at most 50 per cent., and on this basis the weight of steel would be 12.15 tons, and the cost £276.3, or an excess as compared with iron of 6 per cent.

The case of the 500-feet bridge is different, as the parts are larger and there is more room for economy without excessive thinness anywhere, and by the use of long pieces metal may be saved in the joints. Still there would be parts like the cross-girders, floor, and wind-

bracing where the full saving could not be effected, so that probably 80 per cent. only of the parts could be reduced; the weight in steel then would be 700 tons, and the cost £18,900, or a saving over the iron of 10 per cent.

But in the case of a large bridge of this sort the designer would be much hampered by the present rule of the Board of Trade, that 6½ tons strain per inch must be the maximum, because it is obvious that in some parts the steel might with advantage be harder and stronger than in others, and, if free scope in this respect were allowed, a greater economy than 10 per cent. could be effected.

The comparative weight of iron and of steel for any bridge, and the cost in iron and steel, depend therefore on four principal points:—First, the span and the imposed load, which together determine the proportion which the material of the bridge bears to the total load; secondly, the mode of construction and skill in design which adapt the forms of the parts to the material; third, the working strains permitted by the Board of Trade or other authority; and, fourth, the prices of iron and steel charged by the manufacturer. Various considerations arise in regard to all these points, and the author disclaims any intention of defining the limits within which either iron or steel may be used. He only wishes to show the present position of steel as against iron, and the effect of the present restrictions in excluding structures which might otherwise be made with advantage of steel.

8. Although structures made of a strength to satisfy official rules, or the conventional rules adopted by engineers, can in many cases be made more cheaply of wrought iron than of steel, there still remains the question whether it is not worth while to pay a slightly increased price for much greater advantages. For, under the present rules, there is no option but to adopt a margin of strength which would be considered excessive and unnecessary in iron. Taking even the mild steel as prescribed by the Admiralty for ships, the breaking strength of such steel justifies 6⅞ tons instead of the 6½ tons allowed by the Board of Trade; while, if the limit of elasticity be taken as the basis, the difference is still greater. There is nothing even now to hinder engineers from using steel equal to 35-tons breaking strain, though they are forbidden to strain it more than 6½ tons per inch. Therefore, without waiting for any such alteration in the rules as will presently be referred to, the question arises whether it is not worth while to pay 10 or 20 per cent. more for a bridge to gain 20 or 40 per cent. in quality. It must be remembered that the history of wrought-iron bridges under heavy, quick and continuous traffic is still a short one; and the question of renewals and strengthenings looms in a future not too distant to affect present proprietors. Moreover, the tendency of traffic to become heavier and more frequent is likely to continue, and what may appear an excessive margin of safety now may prove no more than sufficient in a few years' time. Although the circumstances differ very much from those of ships and boilers, it is surely suggestive to learn from those best able to judge that, at the rate steel is superseding iron, probably five years hence iron will be no longer used for either of these important purposes. If this be the case, doubtless, the manufacturer of steel will be so extended as to cheapen and improve it for bridge-builders also.

9. There are several means by which the use of steel may be extended. In the first place, the experience already gained as to the certainty of quality and the facility with which it can be worked, has not yet spread to all these concerned, especially to engineers in distant countries who naturally await some authoritative affirmation to set against the records of early mistakes, misapplications and failures. Some ready means of verifying quality is essential to the free use of steel, the precautions at present adopted where steel is employed in large quantities not being feasible in ordinary cases, and by their minute investigation making those engineers who cannot enforce them disinclined to try steel. The Admiralty, who use thousands of tons, have a complete organization at command. Every piece is tested, and the treatment and behavior of the steel during the building of a ship are watched in every stage. So also for "Lloyd's Register of Shipping," the number of vessels is great, and a regular code of laws can be enforced with

precision. If an engineer has a big bridge to build, and requires 1,000 or 5,000 tons of steel, he can and probably will have inspectors to watch the manufacture of the steel; nay, if need be, even to investigate its origin and the ore from which it is made, and may prove the qualities of every piece. But steel will never supersede iron in ordinary cases if precautions such as these are considered necessary; nor can it be expected that the Board of Trade will increase the limits of strain till some certainty of quality is assured. And yet at present to judge by the inspection which takes place, tests are deemed more necessary for steel than for iron, and are not so easily made. In the first place, the principal brands of iron have, in the course of many years, become so well established that they can be safely accepted without any test whatever, not only as affording sufficient guarantee of excellence, but of the kind of excellence also. Where tests are demanded they are mechanical tests only, easily made, and if one piece out of one hundred be proved, all the bars or plates in that parcel may be safely accepted if they are found free from defects easily to be detected by the eye. More than this, even if the iron be not branded or tested in any way, the mere fact that it has been produced by rolling, that it has borne the treatment of punching and fashioning into shape, indicates a certain minimum of quality which will suffice for many purposes, if only it be of ample substance, and this ample substance is sufficiently assured by the Board of Trade rule prescribing 1 square inch of sectional area for a strain of 5 tons; and for structures outside the jurisdiction of the Board of Trade the same rule has been generally accepted. But to judge by the precautions adopted in regard to steel, all steel-makers seem to be looked upon with suspicion. The Admiralty, it is true, always demanded high quality even in iron, but it is interesting to note that Llyod's still admit, as they have always done, iron of a low class, while enforcing such strict rules about steel. The iron plates rolled in this country may be broadly divided into three grades of quality, the best being boiler-plates, the next best bridge-plates, and the next ship-plates. Therefore while, as has been seen, ships are exposed to strains far more severe than bridges, to strains demanding ductility as well as elasticity of the metal, Lloyd's admit to their register ships made of iron which has not a high character in either respect, while setting up so high a standard for steel. In regard to the iron used for ordinary building purposes in this country the case is even worse. It is probably no exaggeration to say that three-fourths of the wrought-iron used by builders in London is about the worst that is made, the contract system as carried out by architects and builders rendering this result almost a matter of certainty. But no great harm comes of it; such iron is not often used for railway structures, and no disaster happens. It is true that such iron may only endure 17 tons before fracture instead of the 20 tons which the architect has specified, or be somewhat brittle; but the limit of 5 tons per inch provides for all, at any rate for the service of many years, the earliest failures being most likely to occur in those cases where the structures are exposed to vibration, and are not properly protected against rust. But in regard to steel the case is very different. While the range of quality in the iron is only one-sixth, say, from 17 tons to 21 tons, the range in steel may be one half, for if it has been improperly made it may be inferior to wrought iron. And the ultimate buyer of the bridge, or girder, or roof, especially if he reside in a foreign country, may naturally desire some evidence that he is getting steel at all; for while, in an iron structure, he has the evidence of his senses that it is nothing worse than rolled iron, it is not so with steel. If a steel bridge be wanted weighing 50 tons, and if this be made of several shapes and sizes, the testing adopted in the case of larger structures will hardly be feasible. True that the word steel may be impressed on every piece, but some further sign or hall-mark seems to be needed which shall tell more clearly and positively what the steel is. Just as on a silver fork there are numerous letters which notify not only a quality but date, locality, and other particulars, so it might be suggested that on steel there should be three marks, one signifying steel, and a second the kind of steel as in regard to an Admiralty, or Lloyd's, or a bridge-builder's

standard. These two marks might be common to all makers, and then, as a third mark, if the brand or trade-mark of the maker were added, it would, in the forming of contracts, be considered as a warranty of considerable value. These marks would form lasting evidence, and, years after the bridge had been erected, its designer perhaps unknown, no invoice or specification available, if an engineer were examining into the safety of the structure to bear a certain load, the mark would give strong *primâ facie* evidence of a useful kind. Whether such a plan be feasible or not, the author wishes to point out that it is far more necessary with steel than with iron, and that the absence of such security will retard greatly the use of steel, especially in commerce with foreign countries through intermediaries; some mark or evidence actually pertaining to the structure having in such cases the greatest value. It is true that at present all steel makers have marks of their own to denote kind, such as S for soft steel, M for medium steel; but what is wanted are signs common to all makers, leaving them their trade-marks only as particular to themselves.

It may of course be said that a remedy should be sought in the more frequent employment of private testing machines. At present the great cost of such machines, and the special care necessary to their use, disinclines the makers of steel structures, or even the engineers who control them, from testing steel; but as the necessary knowledge is more widely known, such testing will probably become more common.

It is not suggested that there should be any Government interference, for such a system is neither popular nor useful in this country; but conventional rules may be established by a concurrence of makers and users, which would form the basis of agreement. Now that steel structures of importance are being made in this country the standard of quality which, after careful investigation, is arrived at for these might be conveniently adopted for less important structures, and be summarized in a mark common to all who chose to warrant it.

But while some authoritative verification and stamp of quality seem thus desirable if steel is to be generally accepted instead of iron, this is not all that is needed. So long as the maximum strain permitted is only $6\frac{1}{2}$ tons per inch, steel is artificially burned in its competition with iron, and can only be used in a small portion of the cases where it would really be preferable. The experience of the last two years proves undoubtedly that steel capable of withstanding 35 tons per square inch before fracture can be produced with the greatest certainty, and that with ordinary care it can be manipulated without any of those risks which formerly were supposed to accompany its use. Leaving on one side the question of steel for ships, which, as has been seen, differs materially from the question now under consideration of steel for fixed structures, rolled plates and bars equal to a strain of 35 tons can be made which will elongate 20 per cent. in 8 inches, and allow a strain of 20 tons before sensible permanent set commences. Compared with the soft steel used for ships it has a higher limit of elasticity, which is just what bridge-builders require. This is gained by a reduction in ductility, which bridge-builders can well afford to lose even if it involves the necessity of drilling the rivet-holes instead of punching them.

With, then, a resistance to fracture of 35 tons as compared with 20 and 22 tons in iron, and with a limit of elasticity of 20 tons as compared with 11 tons in iron, a maximum working strain of 8 tons could as safely be allowed as is the present limit of 5 tons in iron. The advantage would be immediate; steel could then be economically applied to smaller spans than the present rules admit of, especially, if as is likely, the wider use caused lower prices. The author is aware of the difficulties which arise in dealing with this matter. The present rule of the Board of Trade for steel is evidently made to conform to the existing factor of safety for iron. and is based on the breaking strain of the mild steel used in ship-building. If stronger steel be allowed, certain restrictions would be necessary or expedient in regard to the working strains in the various parts of bridges in which it is to be applied; but after all, something must be left to the discretion and ability of the engineer. How the change will come it is

impossible to predict, but as the superiority of steel has now become so manifest, it appears as if its adoption must be made unquestionable either by more liberal rules of the Board of Trade, which will induce bridge-builders and purchasers by self-interest to use it, or by the prohibition of iron altogether. The difficulties which stand in the way of official bodies granting more liberal rules have already been alluded to, and if the quality-denoting stamp on the steel, accompanied by the brand of the maker, as here proposed, be not considered sufficient or feasible, there is at any rate a middle path which would go far to satisfy engineers and at the same time the Board of Trade. This is to allow 8 tons strain where the purchaser bore the expense of official tests. There is plenty of experience available for testing, and the Admiralty and Lloyd's surveyors afford an already constituted body whose certificates might confidently be accepted by the Board of Trade. And if a Commission is necessary to elicit facts and to prove the correctness of the views here stated, then let one be granted; for it is ten years since the last, and the importance of the subject makes it high time there should be another.

It does not follow that, if 8 tons strain were permitted by the Board of Trade, the maximum would always be adopted by engineers. It is one of the advantages of steel that it can, within a wide range, be made of any desired hardness and strength, thus allowing exactly the right kind to be chosen for each particular duty, in a way practically impossible in iron. It can hardly be doubted that the accumulated experience in steel-making and using must ultimately, and it is to be hoped soon, be expressed in regularly tabulated classes of steel. Such choice of kind would facilitate the method of varying the factor of safety according to the proportion of dead load to moving load, to which attention was drawn in Mr. B. Baker's book on long and short span bridges, or according to the nature of the strains, as alluded to by Dr. Weyrauch and by Mr. Am Ende, in 1881. That is to say, while a factor of safety of 4 would amply suffice for the main members of a large girder where 70 per cent. of the strain was constant from the weight of the structure itself and only 30 per cent. arose from the passing load, a factor of 5 or even 6 might be appropriate for cross-girders, the strains on which fluctuate from *nil* to the maximum, and where, therefore, the fatigue was more rapid. Or, as another example, hard steel equal to a strain of 40 tons per square inch before fracture might well be chosen for the supporting columns or piers of a bridge, while the milder steel might be taken for the girders.

The use of steel would undoubtedly be encouraged and extended by a reduction in its price. Since the introduction of the Bessemer and other cheap methods of making steel, there has been a continual reduction in the cost of manufacture, and the example afforded by rails shows that an increased demand brings with it the competition of many makers who are stimulated to the invention of labor-saving processes. Really, the process of making steel is simpler than that of making iron, though this is to some extent balanced by the greater capital expenditure in the plant of the steel works and the greater cost of material. While the ore used for ironmaking is abundant in Great Britain, that for steelmaking is found but in few districts, and in insufficient quantities: and the necessity of importing ore from Spain, Italy, and Africa, of course, enhances the price. The fuel has also to be of special kind, specially prepared. These expenses bid fair to disappear before the basic process, which is to render phosphoric ores—of which in this country there is practically an unlimited supply—available. At present, however, the fact remains that, notwithstanding the cheap price to which steel has been brought, it is still dear for bridges, and the quality which renders steel so valuable for rails—its resistance to abrasion—affords little compensation.

One great advantage, therefore, that would arise, if the limit of 6½ tons were extended, would be an increased demand, which would extend the manufacture, increase the output, and reduce the price. The whole calculation would then be altered, and when steel may not only be worked to 8-tons strain per square inch, but the difference in price over iron is less than it is now at 6½ tons, then, and

not till then, will the area of steel structures have arrived. The history of manufactures in this country shows that the more Government interference and restrictions are reduced, the more does trade flourish. The removal of Excise limitations in the making of glass, paper, bricks, and other commodities, was the starting-point in each of these trades of a sudden growth almost inconceivable before. The introduction of such a period for steel is greatly to be desired.

RECENT HYDRAULIC EXPERIMENTS.

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Minutes of Proceedings of the Institution of Civil Engineers.*

I.

I. Introduction.—This paper is for the most part a short general account of some extensive experiments on the flow of water in the Ganges Canal in Northern India, lasting over four years (1874–79), of which a detailed report was published in 1881.* All experimental and argumentative details are here necessarily omitted.

The main object of the undertaking was to interpolate something between Mr. Bazin's experiments on small canals and the experiments on American rivers, chiefly with a view to discharge-measurement on large canals, the proper measurement of such discharge being of great practical importance, but hitherto attended with much uncertainty. For any such work there are good opportunities in India from its system of canals both large and small, pre-eminent among which is the Ganges Canal.

The extensive scale of the operations can be judged from the following abstract:

565 Sets of vertical velocity-curves, each set containing velocities measured thrice at every foot below the surface.
548 Rod-velocities taken with above, each measured six times.
133 Sets of surface, mid-depth, and bed velocity-curve work, each set containing velocities measured thrice at from twelve to seventeen points on the transversal.
581 Sets of mean velocity-curves, each set containing velocities measured thrice at from ten to twenty-one points.
318 Central surface velocities, each measured forty-eight times.
440 Surface-slopes (about 150 taken on both banks).
90 Silt-collections.
40 Evaporation measurements.

The total number of velocity-measurements was thus about 50,000. Besides these there were many occasional special experiments which together form an important addition.

Nearly all the observations, various, or even unpractical as some of them may seem, were strictly subordinate to the great practical end in view—viz. discharge-measurement: thus the first two items above were the earliest steps directed, at the suggestion of the Mississippi experimenters,* to discover some rapid means of measuring the mean velocity past a vertical, and actually led to showing that the tube-rod is well suited for this.

An important feature in this work is the great range of conditions and data, and therefore of results obtained, this being essential to the discovery of the laws of complex motion. Thus the velocity-work was done at thirteen sites differing much in nature, some being of brick, some of earth; in figure, some being rectangular, some trapezoidal; and in size, the surface breadth varying from 193 feet to 13 feet, and the central depth from 11 feet to 8 inches. At one of the sites the ranges of some of the conditions and results were: central depth, from 10 feet to 8 inches; surface-slope, from 480 to 24 per million; velocity, from 7.7 feet to 0.6 foot per second; cubic discharge, from 7,364 to 114 cubic feet per second.

II. History.—This undertaking was initiated and planned throughout by the author: the field work (lasting from 1874 to 1879), reduction (finished in 1880), and publication (1880–81) were all carried out under his personal superintendence.

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* "Roorkee Hydraulic Experiments."

* "Report upon the Physics and Hydraulics of the Mississippi River, p. 222."

Every precaution which seemed possible was taken to secure accuracy, of which full explanation is given in the detailed report. The experiments and reductions were made for, and at the expense of, the Indian Government, at a total cost of about £5,000, including publication. This may seem a large sum, but such work is necessarily expensive from a cause to be explained hereafter (Section VI.).

III. SITES.—The Ganges Canal abounds in long, straight, fairly uniform reaches 200 feet wide and under. It has the peculiarity of being laid out in reaches of several miles in length, with masonry falls of about 8 feet drop at the tail of each. The original bed-slope was found to give too high a velocity, and to cause injury to the banks and bed. To meet this the falls were built up several feet after 1863, so that they are now all obstructed falls.

Although these raised crests must have diminished the velocity, the longitudinal sections of the reaches show that there has not been much silting up above them. As the canal is often full of silt, this shows that the water is in pretty rapid motion close to the actual bed, and disproves the idea, sometimes advanced, that an obstruction across a channel causes a stillwater pool above it roughly flush with its crest. There are means of temporarily raising still further the crests of the falls, thereby affording great control over the velocity and depth at any site due to a given quantity of water admitted into the reach, so that the mere gauge-reading at any site is no indication of the velocity or discharge through it. The great range of data and results obtained resulted from the exercise of this control.

The systematic observations were made at seven sites of different kinds—viz. at four sites in earthen channels of trapezoidal section, with surface-breadths of 186 feet, 189 feet, 193 feet, and 66 feet and under, and with central depths of 11 feet, 10 feet, 9 feet, and 6 feet and under respectively; also at one site of quasi-trapezoidal section, with a bed of clay and boulders, and banks consisting of flights of masonry steps, with a surface-breadth of from 171 feet to 150 feet, and central depth of from 11 feet to $1\frac{1}{2}$ foot; also at a pair of sites in two similar rectangular masonry channels side by side, each 932 feet long, and 82 feet broad, with a depth of from 10 feet to 8 inches. These three last named are situate in the famous Solání embankment $2\frac{3}{4}$ miles long, and Solání aqueduct 1,112 feet long, and are extremely favorable for experiment.

Some minor measurements were effected at six other sites—viz.: at two large sites in the Solání embankment similar to the above, and also at four small sites in small distributaries of trapezoidal section in earth, with surface-breadths of 25 feet, 14 feet, 14 feet, and 13 feet and under, and with central depths of 4 feet $3\frac{1}{2}$ feet, 2 feet, and $3\frac{1}{2}$ feet and under.

IV. VELOCITY.—A short discussion on the use of floats will now be given; the following short terms are used:

Float.—Any freely floating instrument for measuring velocity.

Run.—The measured length through which a float is timed.

Float-course.—The intended course of a float within the run.

In fair course.—Close to the laid-out float-course.

Forward velocity.*—The resolved part of the actual velocity at any any point, taken parallel to the current-axis.

Note that the forward* velocity is the only velocity of much use in practical hydraulics, and is therefore the quantity really sought in most practical velocity-measurement; hence also the single word velocity is commonly used in hydraulics in the limited sense of forward velocity, and (for shortness' sake) will be so used in this paper; the context will show when actual velocity is meant.

Objection has been taken† to the use of floats, that they do not measure velocity at all, and also that they move quicker‡ on the whole than the fluid particles about them. These objections are first met in the "Roorkee Hydraulic Experiments" by an argument showing that, in spite of the ever varying and confused motion of the water, and of the consequent irregularity of the paths of floats, nevertheless "very small floats do measure fairly an average of the forward velocities of the fluid particles successively in contact with them throughout their run;" and this measure (styled for shortness a float-velocity) must—for want of better

* A useful term adopted from Prof. James Thomson.

† "Annual Report of the Chief of Engineers," U.S.A.

‡ Dubuat's "Principles d'Hydraulique;" Bélidor's "Architecture Hydraulique;" Weisbach's "Mechanics of Machinery," &c.

means—be accepted as a measure of the forward-velocity at the middle of the float-course.

The following appear to be the true criteria of a good float observation, viz.: that the float should be moving in relative equilibrium with the fluid just before entering the run; and that it should pass through the run in fair course and in the same state of relative equilibrium. A float-observation satisfying these conditions should be accepted as good, without any reference to its timing proving longer or shorter than others. Great stress is laid on this, as the practice of some persons is to select from a number of observations those which agree nearly in timing, and to reject the rest; this practice appears wrong in principle. Next, no float which does not satisfy the above should be recorded; this saves useless records in the field book.

In observing the following precautions are important: the ropes defining the run should be strained at the lowest possible level, and the pendants defining the float courses should graze the water. The run should be the shortest compatible with accurate timing; and, in using short runs, accuracy is essential both in laying out the run and in timing. Of these precautions the first and last are essential. The second is of great practical importance in saving time, for in consequence of the unsteady motion floats will seldom move throughout a long path in sufficiently fair course to be worth recording. The following experiment was tried: forty-eight surface-floats and forty-five rods of various lengths were timed, each one through four adjacent runs of 25 feet, 50 feet, 25 feet, and 100 feet. The results from the 50 feet and 100 feet runs were so nearly alike that the former was adopted as the standard run. Shorter runs of 25 feet and 12½ feet were used only close to the banks, where the irregular motion causes undue waste of time with a longer run.

The following advantages are claimed for floats: 1st, they interfere little with the natural motion of the water; 2d, they measure velocity directly; 3d, they can be used in streams of any size; 4th, they are not much affected by silt or floating weeds, &c.; 5th, they measure "forward-velocity"; 6th, they can be made up and repaired by common workmen; and 7th, they are very cheap. Fixed instruments fail in all these points. These reasons were held to justify the exclusive use of floats for all systematic velocity work at Roorkee. The floats used were of three kinds: namely, surface-floats for surface velocity; double-floats for sub-surface velocity; and loaded rods for mean velocity past a vertical. The surface-floats consisted either of a 3 inches by 3 inches by ¼ inch pine disk, or of a 1 inch by 1 inch by ⅛ inch cork disk; the other two instruments will be described hereafter.

V. Details.—Some details about observing the water level, depth, and wind will now be shortly described.

Water level.—The water surface is in a state of constant slight but rapid oscillation, so rapid that it is impossible to note any but the highest and lowest water level. The practice here was to note the highest and lowest water level occurring in about one-half minute, and accept the mead of these as the free water level of the time. By comparing the level so given by an adjacent still water gauge, it was found that the free water level stood, as a rule, slightly higher (even as much as 0.07 foot) than the still water level. The reverse occurred only six times in sixty-three trials. This is an interesting confirmation of the law known from theory, that the pressure in running water is less than in still water. The difference is, as a rule, very small; still for accurate investigation the water level should always be taken in the same way.

Again, to determine with the precision small differences of water level at different places, the surface level must be taken, not only in the same manner, but also at the same time at each place. Great attention was paid to this. It was found that in calm air the free water level of opposite banks differs only slightly; but that in a high cross wind it stands markedly (even as much as 0.07 foot) higher on the lee shore. These conclusions are based on thirty-six trials in calm air and sixteen trials on a high cross wind. Hence all depths depending on a gauge reading, and also all quantities such as discharges computed from them, are liable to slight over or under estimation in a high cross wind, according as a gauge stands on the lee or weather shore.

In all experiments lasting over any time the mean of the water levels taken

as above at the beginning and the end was accepted as the mean water level of the experiment.

Average Depths.—In all cases of irregular beds average depths were obtained by sounding along each float course in six or eight places, so as to give six or eight cross sections about 25 feet apart. The average of these gave an average cross section, from which all wet borders, areas, and hydraulic mean depths were computed; also the mean of the depths along each float course was always taken for the depth thereon. Some such procedure seems an essential in all irregular beds, as the velocities through any site depend on the bed just above and below the site as well as at the site. The great labor thereof is a drawback, but not a justification for omission.

Wind.—The direction and velocity of the wind were observed at the beginning and again at the end of every experiment. The mode of recording this in the printed tables is worth notice, from its conciseness and convenience. The direction of the wind relative to the current (and not its real cardinal direction) is alone of importance as affecting the motion of the water; the direction of the wind was therefore always entered relative to the current axis taken as the working meridian or N. S. line. Thus a wind from up stream is entered as N. (north), a cross wind from the right bank as W. (west), and so on; one of variable direction is entered V. The velocity deduced from observing the number of seconds occupied by one revolution of the hand of an anemometer was entered in feet per second: a wind too light to move the anemometer was denoted as *l* (light). The mean of a number of such wind data was obtained by finding their resultant by the theorem of the polygon of forces by a graphic construction, and dividing it by the number of data.

The wind results can only be looked on as a rough indication of some cause of disturbance of the motion of the water, as there is no known way of making any quantitative allowance of it. It is questionable whether the wind data obtained were the best for the purpose. The highest wind, and also the total, or mean wind during each experiment, are also important data.

VI. Unsteady Motion.—One of the most important conclusions from modern experiments is that the motion of water, even when tranquil to the eye, is extremely unsteady, so that there is no definite velocity at any point; but the velocity varies everywhere largely from instant to instant. The evidence of this is purely experimental; the variability of direction and magnitude may be studied separately.

Variability of Direction.—In any tolerably clear silt bearing stream the motion of the particles of silt will be seen to be most confused; some hurrying up, some down, some cross ways, in apparently ever variable disorder. The paths of floating chips cross each other very irregularly. This transverse motion forms one great difficulty in the use of floats. Again, weeds, strings, &c., fixed at only one end, sway about irregularly; such motion is unfavorable to the use of current meters. The inference is that "the stream-lines interlace irregularly from instant to instant in all directions."

Variability of Magnitude.—This is well seen in the use of floats. It is a common thing for floats passing in succession under a rope, say in the order A, B, C, &c., at 2 inches or 3 inches intervals, and running nearly in one float course, to pass under a parallel rope 50 feet lower down stream in a different order, say, B, A. C, &c., or C, B, A, &c. It was found in the Roorkee experiments that the range of velocities deduced from a number of similar floats run in rapid succession over nearly the same float course was commonly 20 per cent. of the mean. In some of Harlacher's experiments a current meter was fitted with electric connections, so as to record every revolution; the variations amount to from 20 per cent. in surface velocities to 50 per cent. in bed velocities in a few seconds. These rapid changes are certainly not due to faulty experiment, but to the variation of the motion itself.

Extreme variability in both direction and rate (technically unsteadiness) must be accepted, then, as a fundamental property of the motion of water. It is analogous to the well known unsteady motion of the wind, which is well shown by the swaying of the wind vane, and by the fluttering of a pennon. This conclusion

has an important practical bearing on both theory and experiment. First, as to theory, many formulæ and investigations are based on two hypotheses of parallel motion and of steady motion, neither of which states exists even approximately. Next, as to experiment, any single velocity measurement is clearly an accidental value, possibly the maximum or minimum; non synchronous velocity measurements at different points are therefore commonly incomparable. Hence in most cases the average values of velocities are the only comparable ones, and therefore the only ones of much practical use. With current meters this averaging can be done by letting them run for a considerable time. With floats it might be thought sufficient to increase the length of run; but this has been explained above to involve too great waste of time; it can then be done only by repeated measurement of each velocity sought. As to the number of repetitions necessary, twenty-seven instances are quoted in the Roorkee experiments of a velocity measurement from twelve to one hundred times repeated, from which it appears that the means of twenty-five and of fifty repetitions do not differ more than 0.05 foot per second. The twenty-seven cases are of very varied kind; they include eighteen cases of central surface velocities, each forty-eight times measured in calm air at eight widely different sites; and nine cases of central velocity measurements at 5, 6, and 9 feet depths, ascertained with various instruments, repeated from twelve to one hundred times. From this evidence it was concluded that a fair average might be expected from about fifty repetitions, and the number forty-eight was chosen as the standard to be aimed at. This is very laborious work; so many repetitions could seldom be done in less than half an hour, and they sometimes took an hour. Such experiment is necessarily tedious and expensive; as it would take many hours to obtain average values at only a few points.

Again, a serious practical difficulty arises in that the state of the water is itself very varying in a canal, chiefly from the regulation of the supply into, and withdrawal from, each reach; whereas, it is essential that observations intended to be combined should be made in nearly the same state of water. The system adopted, to secure this condition throughout a number of velocity measurements at many points on any one vertical or on any one transversal, was to measure the velocity thrice at each point thereof, in turn from end to end of the vertical or transversal in question, as quickly as possible. The mean of each trio is accepted for permanent record. Such observations, together with all data collected with it, *e g.*, gauge readings, surface slope, state of wind, &c., are briefly styled a "set." When one set was done, a second was undertaken, then a third, &c., each complete in itself, as long as the working hours, weather, and state of water permitted. Each set is thus a complete group of data collected within a short time, and therefore in as nearly the same state of water as practically attainable.

Afterwards all sets of similar character, namely upon the same vertical or the same transversal, at the same site, and nearly in the same state of water, were collected into groups shortly styled "series," and the means of the several data taken. The proper criterion of similarity of the state of water seems to be a close similarity of gauge readings throughout the reach and of the control at the head and tail of the reach. The actual practice was, however, to combine all sets with nearly the same mean velocity and nearly the same water level, a range of 0.3 foot of water level being admitted. The irregularity of the wind forced the combinations to be made almost irrespective of wind. The fairness of these combinations in forming averages is a matter of great importance; in other experiments on a large scale such strict rules have not been observed, combinations having been used of work in very different states of water, and even of dissimilar work when judged as above. The means of the several data in a series evidently form a set of mean data taken under nearly the same average conditions. The field work was, when possible, repeated so as to obtain about sixteen sets in a series; each mean of velocities would thus be the mean of $16 \times 3 = 48$ repetitions, and therefore a fair average value. But from canal exigencies certain states of water occurred very seldom; thus the series for such states contain only a few sets, some only one set; this was unavoidable. Except for illustrating special points, such as un-

steady motion, only the mean results of series have been used in discussion.

On plotting the velocity ordinates so as to form curves showing the forward velocities at all points of a vertical or of a transversal, to be styled for shortness vertical and transverse velocity curves, it is evident that curves formed from a single set, or from only a few sets, are very irregular, and that they become more and more regular in outline with increase of the number of sets in the series represented. Curves formed from many sets, each ordinate of which is therefore an average velocity, may be styled average curves; these are the only ones from which geometric properties can be readily traced, and are the only ones worth discussing. They exhibit the following general properties:

"They are very flat, and are mostly everywhere convex down stream (exceptions being traceable to irregularity of bed or banks)."

"The maximum velocity is furthest from the resisting margin."

Taking the evidence as a whole it would seem that, though the motion is very unsteady in detail, yet there is nevertheless an average steady motion.

VII. Surface Slope.—One of the most important hydraulic data is the surface slope of a stream; this, from its extreme smallness and from the oscillation of the water, is difficult to measure. Great care was taken to secure the best results; it must suffice to say here that the water levels at the two points concerned in finding any one slope were always taken at the same time by signal; for shortness these will be called the slope points, and the distance between them the slope length.

First, it was found from twelve trials of slope lengths of 2,000 feet and 4,000 feet symmetrically situate about the same site, that the deduced slopes are liable to differ by 25 per cent. This shows that surface slope is probably a quantity not admitting of proper measurement, as different slope lengths give such very different results. It seems clear that the slope length should be the shortest, compatible with accuracy in measuring the surface fall therein; also that to give comparable results a standard slope length should be adopted, and that the same slope points should always be used at any one site. The standard slope length on this work was 2,000 feet.

Next, by taking thirty-one pairs of slope measurements in pairs at the same time at three sites 1 mile to 2 miles apart, it was found that the surface slope may be very different at different parts of the same reach.

Again, from one hundred and eighty-one pairs of slope measurements on both banks at six sites, the measurements on the right bank being taken two or three hours after those on the left bank, it appears that the surface slopes of opposite banks may differ 50 per cent. Hence it would seem that a surface slope should always be deduced from simultaneous water levels on both banks, two on each bank, thus requiring four skilled observers. This course could not be adopted from want of observers. The general conclusion from over five hundred cases was that surface slope measurement is so delicate a matter that the results are of doubtful use.

From numerous data it was found that the surface-gradient at different parts of a reach depends partly on the depth, but much more on the control at the tail. Also, that the figure of the free surface along a reach depends chiefly on the control at the tail; thus, during high supply with constant obstruction at the tail, the free surface sinks in nearly parallel lines in the upper sub-reach and in converging lines with decreasing gradient in the lower sub-reach, and therefore becomes a concave surface; and the concavity is greatly increased by increase of the obstruction at the tail.

VIII. Surface Convexity.—Since fluid pressure decreases with velocity, there is some ground for expecting that the surface of a stream should be convex, *i.e.*, stand highest about the middle or where the motion is quickest. The experimental evidence of this is very small.

In the atlas illustrative of Darcy and Bazin's hydraulic experiments there are forty-six carefully drawn cross sections of channels less than 6½ feet wide; these include nine cases of central elevation, and eight of central depression above or below both banks, and twenty-nine doubtful cases. On the large scale the author knows of only two cases in point. In the "Annales des Ponts et Chaussées" for 1848, Mr. Baumgarten

states that once the surface of the Garonne was $\frac{4}{10}$ foot and $\frac{1}{10}$ foot above that at the banks when the river was rising 5 feet in a day, and was another time nearly plane when the river was falling 8 feet in a day. Again, General F. H. Rundall states that on the Godavery and Mahanuddy, when in flood, the surface used to present to the eye the general appearance of being convex, plane, or concave, according as the river was rising, stationary, or falling, "so plain as to be unmistakable to all who were eye-witnesses of it." All this evidence together is very little; the last is, indeed, only a note of the observer's mental impressions. Now the mind is singularly liable to be deceived in impressions of slight convexity of large areas; *e.g.*, to an observer on a high mountain or in a balloon the distant plain or sea always appears to rise to the level of his eye, so that the earth's surface seems concave to him.

The question is of such high interest that an attempt was made to test it by taking at the same instant the free water levels at the center and at both banks in a stream of 171 feet surface breadth, and depth exceeding 10 feet at the center and under $\frac{1}{10}$ foot and $\frac{3}{10}$ foot at the two banks, with a surface velocity of about $4\frac{1}{2}$ feet and $\frac{1}{2}$ foot per second at the center and the banks respectively. The oscillations, amounting to 0.07 foot at the center, rendered the experiment an extremely difficult one. The center was found to oscillate above and below the water level at either edge as much as $\frac{1}{10}$ foot; but the means of twelve trials on one day and twenty-four trials on another, both in calm air and with water gently rising, both gave the very trifling central depression of less than 0.01 foot, so that the fair conclusion seems to be that the water-surface is probably level across on the average.

IX. Sub-surface Velocity.—For all systematic sub-surface velocity measurement the double float was exclusively used. Such strong objections have been urged to the double float that a very full discussion is given in the text, and the adverse opinions are freely quoted. It must suffice to say here that the Connecticut experimenters, having tried both double-floats and current-meters together, decided that the former were "most reliable." The effect of the several inherent faults upon the deduced velocities is also discussed at length. It must suffice to state the principal one, viz.: that, in consequence of the current action on the connector joining the surface to the sub-float, the efficiency of a given double-float decreases with the depth of the sub-float so that there is a limit of depths at which it ceases to be useful. The resultant effect of all the faults is, that the sub-float moves at a depth higher than that indicated by the length of connector, so that the velocity-measurement is attributed to a depth greater than the real depth, and is further affected by current-action on the surface-float and connector.

The double-floats used in the systematic work were of two patterns. One had a spherical wood sub-float of 3 inches diameter, loaded with lead, connected by a brass wire 0.012 inch thick to a 3 inches by 3 inches by $\frac{1}{4}$ inch pine disk as surface-float. The other had a spherical copper shell $1\frac{5}{8}$ inch diameter as the sub-float, loaded with lead, connected by a silk thread $\frac{1}{150}$ inch thick to a 1 inch by 1 inch by $\frac{1}{8}$ inch cork disk as the surface-float. Calling the areas of the sub-float exposed to direct and lateral current-action 100 each, the areas exposed by the surface-floats and connectors at the greatest immersion, 10 feet, were as follows:

	Surface-float.		Connector (10 ft. long).	
1st pat.	11 direct,	30 lateral,	20 direct,	10 lateral.
2d "	10 "	14 "	48 "	24 "

The tension of the connector of the first pattern was ample; that of the second pattern was only 30 grains. It is clear that at the greater depths (say over 6 feet) the connector of the second pattern had too much influence, and that it was not well designed for use in deep water. Mr. Robert Gordon, M. Inst. C.E., the experimenter on the Irrawaddi, describes these floats, after actual inspection, as "models for analysis on a clear-water regular canal;" on the other hand, Mr. Ellis, the experimenter on the Connecticut, reports that they "were not what would be considered of the best form by such American engineers as have had most experience."

X. Vertical Velocity-Curves.—There were in all five hundred and sixty-five complete sets of velocity-measurements with surface and double-floats, at the sur-

face and at every foot of depth below, upon five hundred and sixty-five verticals at three sites. These have been combined into forty-six series (only two containing less than four sets), upon forty-six verticals, viz.: twenty-eight central, and eighteen variously situate non-central, one of which was only 7 inches from the edge.

The range of conditions and results was as shown below:

Work.			Conditions.			Results.
Vertical.	Series.	Sets.	Sites.	Depth.	Surface-breadth.	Mean velocity.
				Ft. Ft.	Ft. Ft.	Feet per sec.
Central	28	344	3	11.0—3.9	170 — 82	6.58—2.54
Non-central, at 12¼ feet to ⅝ foot from edge..	18	221	2	9.6—2.5	169 — 82	4.27—2.20

The whole is believed to be the most important collection yet published. The mode of combination into series has already been explained; the mean results of series are alone used in discussion. From these means forty-six vertical velocity-curves have been drawn and published. The following are their salient properties:

"The curves are generally convex down stream (except near an irregular bank), and are all very flat; their flatness decreases from the center towards the banks."

"The maximum velocity line is usually below the surface, and sinks in a rectangular channel from the center outwards to about mid-depth at the banks."

"The mid-depth velocity is usually greater than the mean (on the same vertical), and the bed velocity is usually the least."

These properties agree closely with those of the curves illustrating Messrs. Darcy and Bazin's small-scale experiments.

The effect of the use of the double-float upon the curves is fully considered in the "Roorkee Hydraulic Experiments," and is shown to be to give them undue flatness, especially near the bed, where the curve is worst determined.

The mid-depth and bed velocities were computed by simple interpolation for every set, and the means taken out for every series. From the five hundred and sixty-five cases, it is at once seen that the mid-depth velocity is by no means constant, as had been supposed by the Mississippi experimenters, but varies nearly as much as any other velocity. The mid-depth velocity-measurements above have the disadvantage of having been made at various times, sometimes at long intervals. To test this point better, therefore, two special experiments were made; the same mid-depth velocity was measured forty-eight times running with the double-float, and twelve times running with a current-meter, the repetitions being as quick as possible; the ranges of the results were 16.9 and 12.6 per cent. of the mean values, thus fully confirming the above. The statement in the report on the Mississippi is shown in the "Roorkee Hydraulic Experiments" to be based, not upon actual mid-depth velocity measurement, but upon an argument involving at least two doubtful assumptions.

XI. Vertical-Curve Figure.—The figure of the vertical velocity-curve is a question of such high theoretic interest that great pains were taken to investigate it. It is a very delicate inquiry, inasmuch as the available velocity-ordinates are not good data for the purpose; for the figure of the curve depends on its curvature, and therefore only on the "second differences" of the velocities, which are always very minute compared with the velocities themselves, so that a trifling error in the latter involves enormous distortion of the curve. The curves indeed are so flat that probably almost any geometric curve could be fitted pretty close to them. In such uncertainty, the only way of dealing fairly with the case, and especially of obtaining fair

values of the parameters involved, appears to be to use the method of least squares. This method was adopted for computing the most probable parabolæ to fit the forty-six observation-curves, and also the probable errors; an allowance was made for the decrease of efficiency of the double float with deep immersion, by varying the "weights" of the velocity-data with distance from the bed, the deeper having the lesser weight. This was a work of great labor, occupying the author and two skilled computers upwards of a month; much of the labor consists in certain preliminary steps, which can be done once for all; the results of these steps have been recorded in a form which can be used by any good computor.

The general result is that, except near an irregular bank, the vertical-curve is approximately a common parabola with horizontal axis; but the "probable error" of the computed parameteris often very large.

This last result is of great scientific importance, for it shows that, though a parabola may be formed to fit the observations well enough by other and simpler methods, such as by "trial and error," no confidence can be placed in values of the parameter not formed by the method of least squares, as their probable error is enormous. Similarly formulas for the parameter depending on such values are probably wrong in form, although they may suit well enough for finding a curve to fit the observations, as great changes in the parameter will not unduly displace the curve. A full discussion is given in the text of the parameter-formulas proposed in the Mississippi and in Bazin's experiments; they are shown to be derived from parabolæ formed by a process of trial and error, and to fail when applied to new data. After many attempts to construct a new formula, the conclusion is drawn that the data are too uncertain to admit of it.

XII. Depression of Maximum Velocity.—A lengthened discussion is given as to the cause of the depression of the maximum velocity-line—a point of great interest. It is shown that neither the actual nor the proportionate depression thereof depend much on the depth of water, surface-slope, velocity, or state of wind. This last result is opposed to that of the Mississippi report.

The primary effect of wind appears to be the production of wave-motion, and it causes translation of the water only when long continued; were it not so, every wind would produce a current on a lake or at sea. It seems probable, then, that the short duration of marked up-stream or down-stream wind on canals and rivers prevents it from prominently affecting the sub-surface water, and with it the depth of the quickest stream-line. All modern experiment shows that forward velocity decreases with approach to a resisting margin. It appears to the author that the air itself must be looked on as an efficient upper resisting margin in all open channels; and if air be resisttant at all, then it is an ever-present cause of retardation of forward surface-flow. If it be so even in a small degree, the maximum velocity line must necessarily be everywhere depressed, and in a rectangular channel this depression would increase towards, and be greatest (but above mid-depth) at the banks, because the resistance of the wet border, namely, sides and tops, would increase towards the banks. These conclusions agree with the results shown in both Bazin's experiments and in the Roorkee Hydraulic Experiments.

XIII. Discharge past a Vertical.—From the five hundred and sixty-five sets of velocity-measurements at each foot of depth, the five hundred and sixty-five superficial discharges past the forty-six verticals were computed down to the level of the lowest velocity-measurement, by the best approximation-formulæ available, including the trapezoidal, Simson's cubic, and Weddle's, the velocity-ordinates being obviously equi-distant; a correction was applied for the lowest spade just above the bed. These are considered to be the best values obtainable from the data.

XIV. Mean Velocity past a Vertical.—This quantity is of great practical importance as a step towards computing cubic discharge; its rapid measurement was pointed out in the Mississippi report as the most useful object of present research. Great attention was accordingly given to this. The mean velocities past the forty-six verticals were computed as the quotient of the superficial discharges by the depths for each of the five hundred and sixty-five sets separately. These are

considered to be the best values obtainable from the data ; and, seeing the number of data from which each is derived, namely, three measurements at each foot of depth, each result may be looked on as a fair average. These are accepted as the fundamental values for testing other proposed approximations. It is important then to show the error thereof due to the use of the double-float. The discussion leads to the following simple rule, very easy of application:—

"The double-float mean velocity past a vertical exceeds or falls short of the true value, according as it is less or greater than the surface-velocity."

From comparing the above values and also approximations found by use of loaded rods, described below, arranged in daily groups, it was found that the mean velocity past a vertical is subject to some temporary variation, less than that of the individual velocities.

For rapid approximation, admitting that the average vertical-curve is nearly a common parabola, the properties of the parabola should aid in finding the mean velocity-ordinate by measurement at only a few depths, and therefore far more rapidly than by the tedious process above. It is at once seen that three, and perhaps fewer, ordinates will suffice. This is fully investigated, and several new formulas are given, involving only three velocities, and a more useful new set involving only two velocities. These last are—

$$\mathrm{U}=\tfrac{1}{4}(v_o + 3v_{\frac{2}{3}\mathrm{H}})=\tfrac{1}{7}(3v_{\frac{1}{6}\mathrm{H}}+4v_{\frac{3}{4}\mathrm{H}})$$
$$=\tfrac{1}{7}(4v_{\frac{1}{4}\mathrm{H}}+3v_{\frac{5}{6}\mathrm{H}}),$$

wherein U is the mean velocity sought, v the velocity at the depth shown by the subscript, and H the actual depth on the vertical. Another, since discovered by the author, by pursuing the same investigation, is:

$$\mathrm{U}=\tfrac{1}{2}(v_{.211\,\mathrm{H}}+v_{.789\,\mathrm{H}}).$$

The investigation shows that these are the simplest formulæ obtainable. The first is one of the best for practical use, inasmuch as the velocities are measured at the highest levels possible in such a formula, and are therefore the most easily measured.

It is important to inquire whether a single velocity would suffice. This would be possible if the velocity at any definite depth were equal to the mean; but this depth is found to depend upon the position of the maximum velocity line, and is therefore variable. From the flatness of the curve an approximation is, however, possible. It is shown that the velocity at $\frac{5}{8}$-depth, or at $\frac{7}{10}$-depth is a fair approximation according as the maximum velocity-line is above or below $\frac{1}{4}$-depth; the former case usually obtains, the latter only near a vertical bank. In the Mississippi report it is proposed to use the mid-depth velocity for this approximation. Much attention was therefore given to this. It is easily shown that, the average curves being very flat and convex down-stream, the mid-depth velocity must, whatever be the curve, exceed the mean velocity by a small quantity; in the forty-six average curves of this work there is only one marked exception to this. In the Mississippi Report, the ratio of the mean to the mid-depth velocity is said to be a "sensibly constant quantity for practical purposes." Were this true it would be an important result, but the experimental evidence now available shows that the ratio varies from 1.082 to 0.918, or about 16 per cent., a quantity not fairly negligible.

The first formulas above have the disadvantage of requiring measurements at two points by two operations. In the last and newest formula, however, only an arithmetic mean is needed. This permits of the two velocities being measured together by a suitable instrument, a great practical advantage; this being a new and important result requires fuller explanation here. It has been shown by the author that, if a double-float be made up of two sub-floats of equal size, similar shape, and similar surface physically, which move at different depths, say λ H, μ H, in strata whose velocities are $v_{\lambda\,\mathrm{H}}$, $v_{\mu\,\mathrm{H}}$, then the velocity, say u, of such an instrument will be—on the usual theory of current-pressure and friction on immersed solids — simply the arithmetic mean of those velocities. Hence, if the sub-floats be sunk to depths of 0.211 H and 0.789 H, the velocity (u) of the instrument will by the last formula be actually the mean velocity (U) required, which is thus obtained at one operation with a single instrument; this effects a great saving of time and labor. But another great advantage accrues; the upper

sub-flat may be made very buoyant, and the lower heavy, so as to throw considerable tension on the connector between them, thus getting rid in great part of one of the worst faults, namely want of stability, of the ordinary double-float; so that the results should be improved in accuracy. The author would recommend that the sub-floats should be thin copper shells not less than 2 inches in diameter, connected by a fine silk thread; the surface-float a small slice of cork joined by silk thread to the upper sub-float. For shortness this might be called the "twin balls." This instrument appears to the author the best yet proposed for depths exceeding those suitable for loaded rods.

All approximations by measurements as above at only one or two selected points require, of course, frequent repetition to bring out average values, as, in consequence of the unsteady motion, it is only these averages that can be expected to approximate to the required mean velocity of the average curve.

Lastly, an attempt was made to find an expression for this mean velocity in terms of the depth, surface-gradient, &c.; it was found to depend much more on the latter than on the depth, but the actual relation could not be traced. It was also found that the direct measurement of almost any velocity was far more likely to give an approximation to this mean velocity than any expression yet known not involving velocities.

XV. Rods.—The use of a loaded rod or float-pole has often been recommended for rapid approximation to the mean velocity past a vertical. It obviously gives some sort of mean of the velocities at all parts of its immersed length, but the degree of approximation has not been hitherto sufficiently investigated. This point was taken up pretty thoroughly at Roorkee.

Out of the five hundred and sixty-five sets above mentioned, thirty-six sets were specially done with a complete double equipment of both instruments, double-float and rod, of 1 foot, 2 feet, 3 feet, &c., to 7 feet depth of immersion. The two instruments were used together in pairs of like lengths, so as to secure the same state of water for both; and every velocity was thrice measured as usual. The results were grouped into six series, three for each instrument; lastly, the average rod-velocities of the several lengths were compared with the average mean velocities past the upper 1 foot, 2 feet, 3 feet, &c., to 7 feet of each vertical, computed from the double-float work; there were in all eighteen pairs of comparable results.

Again, along with five hundred and forty-three of the above-described sets of double-float observations, five hundred and forty-three rod-velocities were measured each six times with rods of nearly full immersion. These rod-velocities are printed set by set along with the five hundred and forty-three mean velocities past each complete vertical computed from the double-float work. The great range of the conditions and data in that work, already described, gives high value to these results.

The grand result was that, after making due allowance for the known inaccuracies of the double-float, the rod-velocity is most probably a closer approximation to the mean velocity past its vertical than the value deduced from the double-float. Observe that this result is purely experimental.

Rods of two patterns were used, namely, 1-inch cylindrical wood poles loaded with lead at the foot, and 1-inch tin tubes made of stout sheet tin, loaded at the foot with a short length of rod-iron, and closed at the ends. The latter was found to be by far the best.

The following are the theoretical advantages claimed for the rod, over the double-float, for measurement of mean velocity past a vertical: It is free from the uncertainty attending the instability and lift of the sub-float; and the result is a closer approximation than that given by the double-float. The practical advantages are, that the result is obtained much more quickly; that the rod is more easily handled, and that it is simpler in construction, less delicate, and cheaper. These advantages are so great that it seems to the author that it should supersede all other instruments for the purpose in conditions favorable to its use. The necessary favorable conditions are: A reach of nearly uniform cross-section and average bed-slope for a great length; that the bed and banks should be pretty even lengthways near the site; and that the depth should not exceed 15 feet. It would often be worth while to prepare a

site, by dressing the bed to the average bed-slope, and the banks to a uniform side-slope for at least 250 feet length; and the banks would be better if revetted with masonry.

XVI. ROD-MOTION.—The experiments did not show directly whether the rod-velocity was greater or less than the mean velocity past the vertical. This was supplied by a mathematical investigation of the motion, but so complex and so long that it can merely be indicated here. It is based on the assumption that the resultant forward force on any part of the rod varies as the square of the relative forward velocity of that part and the fluid adjacent, that the resultant force on the whole rod is zero when in relative eqilibrium; and that the figure of the vertical velocity-curve is a parabola. Upon this it has been found possible* to solve the equations of motion, with the practical result that the rod-velocity is always somewhat less than the mean velocity past its own immersed length, and that, finally, to measure mean velocity past a vertical of depth H, the rod should be immersed only about 0.94 H. As practical necessity also obviously involves the use of a rod immersed decidedly less than the full depth, this result is of great importance, as it removes one of the chief objections hitherto urged to the use of rods, namely, that in consequence of not reaching into the slack water near the bed, they move quicker than the mean velocity.

XVII. TRANSVERSE VELOCITY-CURVES.—There were in all seven hundred and fourteen complete sets of velocity-measurements at from eleven to twenty-one selected points on seven hundred and fourteen transversals. The transversals were the surface, mid-depth, bed, and a quasi-mean, the last being so-called because mean velocities past many verticals scattered across the channel were measured. The instruments used were surface-floats for the surface, double-floats for the mid-depth and bed, and rods for the mean transversal respectively. The whole have been grouped into one hundred and fourteen series on as many transversals at thirteen sites.

Observations.			Conditions.				Results.	
Transversal.	Series.	Sets.	Sites.	Central depths.	Surface breadths.	Surface slope per million.	Mean velocity.	Discharge.
				Ft. Ft.	Ft. Ft.			
Surface.....	10	109	4	10.8–7.5	169–82	238–178	4.61–3.60	708-306
Mid-depth..	2	17	1	10.1–9.1	84–82	210	4.54–4.10	378–348
Bed..	2	7	1	10.0–8.7	84–82	—	4.24–3.42	361–291
Mean.......	100	581	11	11.2–0 7	193–13	480–24	4.87–0.69	7,364–25

The rod-velocity work was considered by far the most important, as from it the cubic discharge, the final object of the whole work, was to be computed; and it will be seen that five hundred and eighty-one sets of observations wexe of this sort. The range of the external conditions, and, therefore, also of the results, was very great. The whole is believed to be the most important collection yet published. The mode of combination into series has already been explained. Owing to canal exigencies low water occurred so seldom, and lasted so short a time, that it was impossible to repeat such work often in the same state of water; thus in the rod-work there are twenty-nine series containing less than five sets each, besides twenty-two of only a single set; so that the low-water work at each site is of less weight than the rest. The mean results of series are alone used in discussion in general.

The spacing of the float-courses was fixed so as best to exhibit the figure of the transverse curves; and so to be convenient for discharge-computation. To meet the former the float-courses were spaced widest where the curve was known to be flattest, viz. throughout the central portion, and closest where the curvature was known to be sharpest, viz.

* Now done for the first time.

near the banks; to meet the latter the spacing was made equidistant throughout the central, intermediate, and side spaces.

From these series one hundred and fourteen curves have been drawn and published; these are the transverse velocity-curves. The following are their more salient properties:

"The curves are generally convex down-stream over a level or concave bed, are nearly symmetric in a symmetric cross-section, and are all very flat."

"The velocity is generally greatest near the center (or deepest channel), decreases very slowly at first towards both banks, more rapidly with approach to the banks or with shallowing of the depth, and very rapidly close to the banks."

"The figure of the curve is determined by the figure of the bed, increased depth producing increased velocity, and *vice versa*, so that a convexity in the bed produces a concavity in the curve, and *vice versa*; also these effects are more marked in shallow than in deep water."

The decrease of forward velocity is so rapid close to the banks, as to make it clear that the forward velocity at the edges must be very small, possibly zero. It does not admit of direct measurement. The employment of floats close to the edge is, of course, impossible when the edge is irregular: but even with artificial straight banks their use is very difficult in consequence of a prevailing transverse surface-current from the edges, which is so marked that in a float-course only 7½ inches from a straight vertical bank, occasionally one hundred surface-floats were run before three were obtained in fair course over a 12½-feet run. This transverse surface-flow is supposed by some to be caused by the reduction of pressure at the center consequent on the higher central velocity. To keep up the water-level at the edge, this surface-flow from the edge clearly involves a sub-surface-flow towards the edges; the experiments showed this indirectly, in that the deeply-immersed double-floats and rods moved without any general bias.

XVIII. TRANSVERSE CURVE FIGURE.—The geometric figure of these curves is a matter of high scientific interest. The unsuitability of the data (just as in Section XI.), and their extreme flatness makes it a delicate matter; hence the velocities near the banks, where alone there is any marked curvature, are the most important for this purpose; in fact, omitting these, almost any geometric curve might be made to fit. This is well shown in the trials of the late Mr. Darcy and Canon Moseley, who proposed a semicubical parabola and an exponential curve respectively, from certain theoretical investigations; and, though using the same experimental data, Darcy's Results in Pipes, for their verification, were each satisfied with their own results: Darcy's curve, however, is convex, whilst Moseley's is concave down-stream; but the experimental test is very poor, as Darcy's velocity-measurements were solely in the middle two-thirds of the stream, thus omitting the critical portions near the edges.

One interesting general result is deduced in the discussion, that "Curves of like kind with same water-level at same site, but with different general velocities, are nearly parallel projections of one another." This is deduced from a consideration of five pairs of curves of the same kind, the two curves of each pair being obtained at nearly the same water-level at each site, and each containing fifteen or sixteen measured ordinates for comparison; the cases, five in number, are few for so broad a generalization, but they are each good in that the general velocities in the two curves of each pair are very different, their ratios being as 3.4, 4.7, 1.7, 2.3, 1.9 to 1 respectively.

Two other important conclusions are drawn. No single curve will suffice, as the flatness of the curve varies with the water-level in such a way as to show that the exponents of the abscissæ should probably vary with the water-level. And no single species of curve will suffice, inasmuch as it seems that the figure of the curve is, for given states of water, determined by the figure of the bed. It follows that it is almost hopeless to seek the figure of the curve from mere experiment, without some help from a rational theory.

XIX. AREAS AND DISCHARGES.—From the average soundings, and from the above-mentioned sets of velocities past a transversal, the whole of the cross-sectional areas required and the corresponding discharges, thus including five hun-

dred and eighty-one cubic discharges, were computed by the most accurate formulas available (viz. the trapezoidal, Simson's cubic, and Weddle's) for a curved area or volume divided by equidistant ordinates or planes. For the cubic discharge, the most important of all, which is represented by the volume of the velocity-surface, the preliminary step was to multiply every rod-velocity (u) by the average depth (H) in its float-course, the products (Hu) being the superficial discharges past each vertical, *i. e.* the areas of the equidistant plane sections of the velocity surface; these areas being known, the above formulas were at once applicable. It might be supposed that the use of these formulæ is very troublesome; they are, on the contrary, very simple, and their use was readily acquired by the ordinary overseers of the Indian P. W. D. It is shown in the text that on account of the usual convexity of the curves with concavity of the bed, all simpler formulæ err in defect in the long run, and in the case of the cubic discharge the error is not necessarily very small. A curious source of errors occurs in that in these formulas different ordinates carry different coefficients, and have therefore different weights in the result; thus in Simson's formula, the middle ordinate being multiplied by 4, an error in it is four times as important as a similar error in the end-ordinates; to avoid this it would seem necessary to have all the ordinates of equal weight, by making the number of repetitions of each measurement proportional to its weight in the formula; this would be so troublesome that it would only be done to secure a high degree of accuracy. The present results are considered to be the most accurate obtainable from the data.

The surface, mid-depth, and bed-discharges being of little interest will not be further alluded to here. The cubic discharge is so important that it seems well to recapitulate the process of measurement used. This contains three distinct steps: 1. Obtaining the average depths by sounding along a number of float courses 2. Rod-velocity measurements in each float-course. 3. Computation. The time required depends on the number of float-courses, and on the number of repetitions of the measurement of each depth and of each velocity. Closer approximation to average results is obtained by increase of these numbers. At Roorkee the details were: 1. Sounding along about sixteen float-courses in eight cross-sections; time, three to four hours. 2. Rod-velocity measurements in fifteen to twenty-one float-courses, each thrice repeated; time, two to four hours. 3. Computation, about two hours. The chief advantages of this process are that it is direct and purely experimental, and therefore independent of any as yet uncertain theory; that the data are taken from many parts of a site, so that the result is certainly some sort of average; and that the whole velocity work is done within a short time, *i. e.* in as nearly a constant state of water as is practicable. This last point is of extreme importance.

From the mode of computation it is evident that breadth, depth, and velocity all enter as factors into the result. In wide streams the breadth varies but little, so that the cubic discharge varies chiefly with the other two factors. The experiments show clearly that the discharge increases and decreases with the rise and fall of the water level, but depends in a far greater degree on the velocities. This shows that a discharge table for a given state of water must be a table of at least double entry, showing the discharge as dependent on both gauge reading and velocity, or some equivalent data, *e. g.* gauge reading and surface-slope, or gauge readings throughout the reach; this result, which seems felf-evident, is of great practical interest. The official tables in use on the Ganges Canal have hitherto indicated a definite discharge for a given depth, but the power of control on this canal is so great that the gauge alone is no indication of the discharge; witness the following results:

	Soláni R. aqueduct.				Soláni embankment.		
Gauge reading.	4.6	4.0	3.6	3.6	3.6	3.6	2.9
Cubic disch'ge.	482	1,623	212	1,124	643	483	1,142

On comparing the new results with the official canal tables, the new results were found to be unexpectedly higher than the official at all the higher depths. This was at first supposed to be due to an inherent defect in the use of rods which had hitherto been believed to register unduly high velocities, in consequence of not reaching into the slack water just over the bed; but if this were so, the ex-

cess in question would be relatively largest at low water, because a small lift of the foot of the rod above the bed is of greater relative importance in shallow than in deep water, whereas at low water the new results were found to be less than the official. The supposed defect of the rod has, however, been shown above not to exist. The mode of preparation of the official tables was examined, and is demonstrated to be such as to lead naturally to under estimation at high water, and to over estimation at low water.

One chief aim was to compare the experimental discharge measurements with those given by various discharge formulas; most of these formulas give the mean velocity; the comparison of mean velocities is in these cases most convenient, and will be discussed later. A somewhat complex discharge formula was proposed by the late Canon Moseley,* and was shown by him to agree pretty well with Bazin's small scale experiments; it is interesting as having been deduced from a rational, but probably incorrect, theory of fluid motion. On comparison with the present large scale results, it was found to give results of only from $\frac{1}{4}$ to $\frac{1}{14}$ of the observed, so that it is clearly useless. †

XX. Mean Velocity.—From the discharge measurements above mentioned, the corresponding mean velocities were computed separately for each set by dividing each cubic discharge by the area, thus giving five hundred and eighty-one mean sectional velocities. These are considered to be the best values obtainable from the data, and seeing the number of data from which each is derived, namely three measurements at from eleven to twenty-one points on each transversal, may be looked on as fair averages. The mean surface, mid-depth, and bed-velocities, being of little interest, will not be further alluded to here; the very important mean sectional velocity is, for shortness, called "mean velocity" below.

It seems clear that the cubic discharge is constant from instant to instant, so that the mean velocity must be so likewise, *i. e.* both are technically steady. The measurements of both these quantities, however, indicate marked and seemingly capricious variations in apparently the same state of water; no doubt due to their being simply an average from a number of incessantly varying data. The cubic discharge and area both vary with change of water level, but the effect of this disappears in part from their quotient, so that the mean velocity must be less variable than the discharge.

The mean velocity V is a quantity of such great practical use for immediate computation of the all important cubic discharge, as the product $D = A\,V$, that immense labor has been given at various times in the endeavor to find some rapid approximations to it. These have taken two principal forms, one involving only velocity data, and the other only surface-slope and cross-section data. The most important modern trials of the latter, by Mr. Bazin and Kutter, are based on the old Chézy formula, involving $\sqrt{RS}$.

On this work data were collected on an extensive scale for trial approximations in three ways. In fact, all the experiments were directed to this. Measurements were made of three quantities, viz.:

Central * mean velocity U_0, central surface velocity v_0, surface slope S,

the approximations being intended to be made by using certain "reduction-coefficients" (α, β, C,) thus:

$$V=\alpha\,U_0;\quad V=\beta\,v_0;\quad V=C\,w,$$

where for shortness † $w=100\sqrt{RS}$. The experimental values of the coefficients are computed by inversion of these formulas. The experimental values of the coefficients are computed by inversion of these formulas.

The use of the central mean velocity U_0 in this way was an after-thought; it had been measured as a part of the regular work of each of the five hundred and eighty-one above-mentioned sets of rod work, thus giving five hundred and eighty-one values of both U_0 V. Those of U_0 are poor averages, having been only thrice repeated; the reduction coefficient $\alpha = V \div U_0$ has therefore been taken out only for the one hundred mean values of U_0, V in the one hundred series

* "On the Steady Flow of a Liquid," *Philosophical Magazine.*

† The author has not seen this formula challenged before.

* A short term denoting "mean velocity past the central vertical."

† This important quantity w is conveniently treated of as a quasi-velocity.

into which the rod work was grouped (*v supra*). The central surface velocity v_o, and surface-slope S, were measured along with many of the five hundred and eighty-one sets of rod-work just mentioned. They are both so much affected by wind that they were taken, as a rule, only when wind and weather were favorable; thus only three hundred and thirteen values of v_o, and three hundred and sixty-three of S, were obtained. These were grouped, along with the corresponding values of V, into special series, namely, seventy-six of v_o, V, and eighty-three of S, w, V; the reduction coefficients $\beta = V \div v_o$, $C = V \div w$, were taken out for each set separately, as well as for the means of series. The central surface velocity was always measured forty-eight times, so is a good average; the surface slope was measured once only with each set on one bank in two hundred and twelve sets, and on both banks in one hundred and fifty-one sets. The range of conditions and results was very great, nearly the same as that of the rod work, *q.v.* The whole form, it is believed, one of the most important collections of such data yet published. The "weights" of V, U_o, v_o, w, will be seen to be very different, and the series containing them do not correspond; this could not be helped. As a general rule, only the serial means of the four velocities (V, U_o, v_o, w), and of the three coefficients (a, β, C) have been used in discussion. From these data diagrams have been pubilshed to show the relations of these quantities to each other, and to various hydraulic elements. From the tables and diagrams it is evident that:

The four velocities, V, U_o, v_o, w, increase and decrease, as a rule, together, and also increase and decrease in general jointly with the increase and decrease of hydraulic mean depth and surface slope.

"The connection between V, U_o, v_o, seems much closer than between V, w."

"Up or down stream wind markedly decreases or increases surface velocity; its effect on mean velocity seems quite trifling."

Next, as to the coefficients a, β, C, it appears that—

"The ratio C increases with decrease of R, but depends largely on some other unknown elements. The variation of β is irregular (probably partly from wind effect). The ratio C increases with increase of R, and depends also greatly on S, and also on the nature of channel."

After discussing various known formulas for mean velocity, the only ones that appeared worth extended trial were Bazin's formulas for the coefficients β, C, and Kutter's* for the coefficient C. Accordingly the values of these coefficients, from the published Tables, have been printed alongside the experimental mean serial values, seventy-six of β and eighty-three of C. As to Bazin's two coefficients (β, C), the discussion shows that neither is reliable, and that the use of the former with surface velocity leads to under estimation of mean velocity, and that the latter is defective in not containing S. As to Kutter's coefficient C, the discrepancies between eighty-three experimental and computed values were:

Thirteen over 10 per cent., five over 7¼ per cent., fifteen over 5 per cent., seventeen over 3 per cent., thirty-three under 3 per cent.

Now in all the discrepancies over 10 per cent., it was found that the state of water was unfavorable for the slope measurement. Taking this into account, along with the varied evidence in Kutter's work, it seems fair to accept Kutter's coefficient as of pretty general applicability; also that when the surface slope measurement is good, it will give results seldom exceeding 7½ per cent. error, provided that the rugosity coefficient of the formula be known for the site. For practical application extreme care would be necessary about the slope measurement, and the rugosity coefficient could only be determined, according to present knowledge, by special preliminary experiments at each site.

The formula derived from the Mississippi experiments was also tried in nineteen test cases; the agreement with experiment was extremely poor. This has already been shown in Kutter's work.

The experimental results (V, S) of this work have been recently applied by Dr. Hagen to test his proposed new general formula $V = C R^m S^n$; from a selection of forty-three of the series most complete in the data (V, S) he deduces, by the method of least squares, $V = 65 R^{\frac{2}{3}} S^{\frac{1}{2}}$; but the "probable errors" computed

* "The New Formula for Mean Velocity of Discharge of Rivers and Canals." By W. R. Kutter.

therewith appear enormous. Moreover, he had in the same way deduced from the Mississippi experiments the different expression $V=6\ R^{\frac{1}{2}}\ S^{\frac{1}{4}}$. From this he concludes that probably the surface-slope measurements are too inaccurate for use in such a formula.

The general result of trial of these formulas, which are all empirical, shows that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labor of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the author hopeless to attempt further improvement.

In the absence of any true formula, that coefficient which is the least variable would probably be the best for practical use, as likely to lead to least error. Now the range of the experimental values of $C = V \div w$ is far larger than that of either α or β. This result is of great importance, in showing that the connection between mean velocity and other velocities is far closer and more intimate than between velocity and surface-float. The connection is unknown in any case; but the latter is a physical one, requiring the determination of velocity from physical conditions, whereas the former is possibly only a geometrical one, depending on the figure of the velocity surface. Moreover, the surface-slope is extremely difficult of proper measurement, and its use involves a working from the minute to the large. It seems, then, that at present the direct measurement of velocity, such as the central mean or central surface, is more likely to give a near value of the mean velocity than any formula involving surface-slope, and of the two velocities the central mean is to be preferred, as being little affected by wind; but there is as yet no good formula for reducing these velocities to the mean, so that the "reduction-coefficients" required must at present be determined by special trial for each site.

XXI. Discharge-Verification.—It is very important to have the means of testing the accuracy of any process of discharge-measurement. On the large scale any absolute test is impracticable. The only test that seems practicable is that the results should be consistent with each other. Many of the Roorkee results admit of such a test being applied in several ways.

Test 1.—It is clear that discharge-measurements, and therefore also the deduced mean velocities, made at the same site under nearly similar states of water should be nearly equal. The different sets of rod-velocity work from which they are computed were seldom done at exactly the same water-level. The difference of water-level affects the computed discharges directly, but scarcely affects the deduced mean velocities (Sec. XX.), so that the latter are the more fairly comparable.

Test 1*a*.—A series being a group of data, &c., in nearly the same state of water, the mean velocity results within each series can thus be compared together. Rejecting, out of the total of one hundred series of rod-work, the twenty-two series containing only one set each, there remain seventy-eight series containing five hundred and fifty-nine sets in all, which yield two thousand seven hundred and fourteen pairs of comparable results. There were only eighty-eight cases of discrepancy over 10 per cent. in this large number. On examining the details of these, it was found that the external conditions were in a large number not nearly so closely similar as was expected from their collocation within same series, or as is desirable in such a test.

Test 1*b*.—Out of the whole number of five hundred and eighty-one sets, two hundred and fifty-six sets had been done in one hundred and six days, viz. in one hundred and six groups of 2, 3, 4, 5, or 6 within the same day, usually in immediate succession. at the same site. These yield one hundred and ninety pairs of comparable mean velocities, which seem favorable for this test, as the probability of constant state of water throughout one day is considerable. The discrepancies were really very small, being over 10 per cent. in seven groups; over 5 per cent. in two groups; over 3 per cent. in seventeen groups; under 3 per cent. in eighty groups; and in every one of the cases over 5 per cent. the cause was traced to the canal being out of train.

Test 1*c*.—On one occasion two complete sets of rod velocity work were done by timing the whole of the rods through both a 50-feet and a 100-feet run. The

discrepancy of the mean velocities deduced was only $\frac{3}{10}$ per cent.; this extreme closeness is probably accidental.

TEST 2.—It is clear that discharge-measurements at successive sites, between which there is neither influx nor outflow, in the same stream should be nearly equal, if conducted under nearly similar general external conditions in the portion of the stream between the sites. This test can be applied to seventy-five cases, by comparing the discharge at an upper site with that at a single lower site in ten cases, and with the sum of discharges at two lower sites (in two branches of the stream) in sixty-five cases. The sites being of very different natures, widths, and depths, the test is a searching one.

TEST 2*a*.—In the first six trials the field work at the upper and lower sites was done on different days, and, therefore under somewhat different external conditions. A small correction was applied to reduce the work at each site to a common level at the standard gauge; the highest discrepancy remaining was 7 per cent., and the rest were under 5 per cent.

TEST 2*b*.—In thirty-five more trials the field work was done at the same time at the upper and lower sites by three complete field-parties; the discrepancies were, six over 5 per cent. (8.3 the highest); six over 3 per cent.; twenty-three under 3 per cent.

TEST 2*c*.—In thirty-four more trials the field work at the two lower sites was done later than at the upper site, time being roughly allowed for the water to pass from the upper to the lower sites; these may be said, in a rough way, to have been carried out in the same body of water; the discrepancies were, two over 5 per cent. (5.5 the highest); three over 3 per cent.; twenty-nine of 3 per cent. and under.

In very many of the cases of the higher discrepancies in each test certain disturbing causes were found to exist, *e. g.* a rising or falling state of water, high wind, &c.; some of the sites were also unfavorable. The evidence of all the tests together as to the consistency of the results is very great both in amount and in range of data. The application of Test 2 was, of course, expensive, as it involved the use of three complete field-parties for six months; the importance of these tests was well worth the expense.

The conclusion seems fair that "the process of discharge-measurement described yields, under favorable circumstances, results which will probably seldom differ 5 per cent." For such close approximation it seems essential that—1. The sites should be favorable. 2. Fieldwork should be done only when the water is "in train."

XXIII. CURRENT-METERS.—Three current meters, one of Moore's, one of Révy's, and one of Elliot's patterns, were tried for some time, but the experimental difficulties were not got over. A pretty full discussion is given of the disadvantages of current-meters in general and of the means of their improvement. A special lift was contrived for gripping the meter firmly parallel to the current-axis, so as to register only forward velocity, and having a continuous rigid gearing wire. This got rid of at least four bad faults, viz. the uncertainties of orientation and of position of the meter, and of gearing and ungearing it, and the non-measurement of forward velocity which occur otherwise in the ordinary use. A great improvement might be made so as to reduce the mass placed in the water, viz. by separating the recording works from the screw and placing them above water under the observer's eye, with electric connections to cause them to follow the motions of the screw. But at best, there are serious difficulties in their use, some of which seem insurmountable.

XXIV. SILT.—An important course of experiments was made on the distribution of and amount of silt in the channel. A specimen of the water was collected in a brass tube 12 feet long by 2 inches internal diameter, open at both ends. This was thrust vertically from a boat floating freely down stream, until it touched the bed, whereon the lower end was closed by a movable lid worked by a strong spring; by this means a column of water stretching from the surface to the bed of the channel was collected. The silt was separated from the water by decantation and filtration, and was finally weighed on a well-dried filter in a chemical balance. From this the weight of silt in grains per cubic foot of water was computed. This result is called silt-density; it is obviously an average of

the vertical of collection. Two styles of experiment were made, one on the distribution of silt across the channel, the other on the relation of the silt-density to the velocity and depth.

Silt-distribution.—Two sets of silt collections were made at nine points, distributed across two of the large sites, each set being done as quickly as possible. Each resulting silt-density multiplied by the mean velocity past the vertical of collection is obviously the mean silt-velocity past the vertical. Next, the three quantities, namely, the silt-density in, the mean silt-velocity past, and the mean velocity past each vertical, were plotted as ordinates on a common base line, thus giving three transverse curves. These curves were so little alike and so irregular, that it seems evident there is no close connection between the silt and the velocity at different parts of a channel, also that probably the silt-density at any point varies from instant to instant, *i. e.* is, technically, unsteady.

Silt-relations. — Again, seventy-three collections were made on the central vertical at four of the large sites in very varied conditions of depth and velocity at two of them. From these it appears that the silt-density in no way depends on the depth or velocity, at any rate in the Ganges canal. In fact it seems certain that the silt in this canal depends chiefly on the state of the supply-water from the Ganges, which varies from clearness to great turbidity, and on the occasional admission of local drainage which is often very turbid; so that the canal is unsuited for experiments on the relation of silt to velocity, &c. This is a disappointing conclusion, as the labor of the silt-collection and reduction was very great.

XXV. EVAPORATION.—An attempt was made to measure the evaporation from the canal surface, lasting twenty-five months from 1876–79. The evapometer was a zinc pan 12 inches square and 9 inches deep, above which the four sides splayed outwards so as to form a "free-board" 10 inches high and exposing an opening of 30 inches square at top to the sun and air; this rested in a wooden frame buoyed by zinc air-chambers so as to float in water. The experiment was started by pouring canal water into it to a depth of about 6 inches; after carefully measuring this depth, the pan was set afloat at mid-channel, moored in a place where it was not likely to be disturbed; when so floating, the water-level inside the pan was nearly flush with the surface of the stream. After about a week, it was taken out and the depth of water remaining was carefully measured. The loss, if any, was considered to be the evaporation, diminished by dew.

Out of the twenty five months mentioned, measurements during two were lost by an accident to the pan, and eight months of "rainy season" were also unavailable; in the remaining fifteen months about half the trials were vitiated through stress of weather; *e. g.* every trial affected by rain had to be rejected until the erection of a rain-guage at the site enabled an allowance to be made for rain. Thus, finally only forty results were obtained. Along with twenty-eight of these results, the mean temperature, mean humidity, and mean wind at the Roorkee Observatory are recorded; also, in a few cases, the temperature of the canal water.

The most remarkable feature of the results is their extreme smallness, amounting to only about $\frac{1}{10}$ inch per day on the average near Roorkee; whereas $\frac{1}{2}$ inch per day is said to be a common rate in India for evaporation on land. This led at first to the suspicion of introduction of water *ab extra*; but after considering the possible sources of this, namely, leakage, spray, rain, dew, wilful tampering, it still seems that the results may be accepted as substantially correct. The real cause of the small evaporation appears to be the unusual coldness of the canal water *e.g.* on May 22, 1877, at 2.30 P.M., the temperature of the air was 165° in the sun, and 105° in the shade, whilst that of the water was only 66° inside the pan and 65° in the canal; also the highest recorded temperature of the canal water was only 65½°. The canal, in fact, takes its supply from the Ganges, a snow-fed river, at its exit from the hills.

It was, indeed, found that the canal-evaporation increases with distance from the head, *i.e.* from the Ganges. Thus out of the forty results, twenty-eight were taken near Roorkee, and twelve near Kamhera, at distances of 18 and 52½ miles from the head-works; the evaporation at the latter was much the larger,

comparing, of course, similar seasons, being about 0.15 inch against 0.10 inch on an average. This is no doubt due to the gradual heating of the water under the hot sun with increased distance from the head.

Taking the Roorkee estimate of $\frac{1}{10}$ inch per day, the total evaporation from the whole surface of the canal and its branches, about 487 million square feet, amounts to about 47 cubic feet per second, which is about $\frac{1}{150}$ part of the full supply of the canal, or in other words ten minutes' full supply daily.

Little connection could be traced between the evaporation and the meteorological elements; the temperature of the water, which depends chiefly on the amount of snow-water in the Ganges, being probably the governing element.

UNDERGROUND TEMPERATURE.

From "Nature."

I.

THE Underground Temperature Committee of the British Association have presented a summary (drawn up by Prof. Everett) of the results contained in all their reports (fifteen in number) up to the present date, of which the following is an abridgment:

The results are classified under the heads: A. Instruments. B. Methods of observation. C. Questions affecting correctness of observations. D. Questions affecting deductions from observations. E. Comparison of results. F. Mean rate of increase of temperature with depth, and mean upward flow of heat.

A. INSTRUMENTS.—Under this head we have: 1. Instruments for observing temperature. 2. Subsidiary apparatus.

1. The thermometers which the Committee have employed have been of two kinds—slow-action thermometers and maximum thermometers. The present pattern of slow-action thermometers consists of a thermometer having its bulb surrounded by stearine or tallow, the whole instrument being hermetically sealed within a glass jacket, and had its origin in a conference between the secretary and Dr. Stapff in the St. Gothard Tunnel.

Our present patterns of maximum thermometer are two—the Phillips and the Inverted Negretti—both being hermetically sealed in strong glass jackets to prevent the bulbs from receiving pressure when lowered to a great depth in water. Both instruments are used in a vertical position, and they register truly in spite of jolts in hauling up.

References to Becquerel's thermo-electric method of observing underground temperature were made in three of the reports, and some laboratory experiments were subsequently carried out by the secretary, which led to the conclusion that the method could not be relied on to yield sufficiently accurate results. It may be mentioned that Becquerel's observations are only carried to the depth of 100 feet, whereas we require observations at the depth of 1000 or 2000 feet.

2. Under the head of subsidiary (that is non-thermometric) apparatus, plugs for preventing convection-current in a bore or well are referred to. Prof. Lebour's umbrella-like plug, in its final form, appears to be very convenient, as it requires only one wire. It remains collapsed so long as the wire is taut, but opens out and plugs the hole when it becomes slack.

B. METHODS OF OBSERVATION.—These have chiefly been of two kinds: 1. Observations in holes bored to the depth of a few feet in newly-opened rock, either in the workings of a mine or a tunnel, or in a shaft during the sinking. The rock should not have been exposed for more than a week when the hole is bored, and a day may be allowed to elapse for the heat generated by boring to escape before the thermometer is inserted. Very complete plugging is necessary to exclude the influence of the

external air. It is desirable to use about two feet of plugging, of which the outer part should be made air-tight with plastic clay or greased rag. After the lapse of a few days, the thermometer is to be drawn out by means of a string attached to the handle of its copper case, and the reading taken. The slow-action thermometer above described is employed for this purpose, and there is time to read it with sufficient deliberation before any appreciable change occurs in its indication. It is recommended that the thermometer be then reinserted and plugged as before, and a second reading taken after the lapse of a week. The majority of our successful observations have been made by this method.

2. Observations in deep bores of small diameter. The first report contained a successful application of this method to a bore about 350 feet deep, near Glasgow, which gave very regular results in a series of observations at every sixtieth foot of depth; but in the majority of instances in which it has since been applied, there have been marked irregularities, due apparently to the influx of water from springs at particular points. One of the most valuable of our results was obtained by the application of the method to a bore 863 feet deep, executed at the bottom of a coal mine 1066 feet deep, giving a total depth of 1929 feet. The bore in this case was dry at the time of its execution, though full of water at the time of the observation. It was in South Hetton Colliery, Durham. The instrument generally employed in the observations of this class was a maximum thermometer of either the Phillips or the Inverted Negretti construction.

The larger the diameter of the bore, the more uncertain does this mode of observation become. The South Hetton bore had a diameter of $2\frac{1}{2}$ inches. The Kentish Town well, 1000 feet deep, in which Mr. Symons' observations were made, had a diameter of 8 inches, and the well 660 meters deep at La Chapelle, in the north of Paris, had a diameter of $4\frac{1}{2}$ feet (V., VI., VII.). The temperatures in this last were proved to be largely affected by convection, the water at the top being too warm, and that at the bottom not warm enough. The observation of Herr Dunker, in the bore at Sperenberg, near Berlin, with a depth of 3390 feet and a diameter of 12 inches, proved a similar disturbance, amounting at the top and bottom, to several degrees. As regards the bottom, the proof consisted in showing that when a thermometer at the bottom was protected by a tight plug from the influence of the water above, its indications were higher by 3° R. ($=6\frac{3}{4}$° F.) than when this precaution was not employed.

C. Questions Affecting the Correctness of the Observations made might theoretically include questions as to the correct working of the instruments employed, and as to the personal reliability of observers; but the latter topic has not come into discussion, and the former has not arisen since our present patterns of instrument came into use. The questions for discussion are thus confined to those which relate to possible differences between the temperature of the point at which the thermometer was placed and the normal temperature at the same depth in its vicinity.

1. The heat generated by the action of the boring tool will vitiate the observation if sufficient time is not allowed for its escape.

A very full discussion of this subject in connection with the great artesian well at La Chapelle will be found in reports V., VI., and VII., clearly establishing the fact that the temperature at the bottom both on the third and the sixth day after the cessation of boring operations, was $7\frac{1}{4}$° F. higher than after the lapse of four months, though the water had been left to itself during this interval. Further evidence showing that the temperature in the lower part of a bore full of water may thus be raised several degrees, is furnished by the Sub-Wealden bore.

2. The generation of heat by local chemical action is well known to be a powerful disturbing cause when pyrites is present. The observers in the mines of Schemnitz say, "Pyrites and also decaying timber were avoided, as being known to generate heat." The observations in the coal mines of Anzin show a temperature of $70\frac{3}{4}$° F. in shaft IV. (a very dry one) at the depth of 21.2 meters, or less than 70 feet. This must be about 15° F. above the normal temperature. In shaft II. the observer mentions that there was, at a depth of 90m., a seam of coal in which heat was generated by oxidation.

At Talargoch lead mine, in Flintshire' the discrepancies between the temperatures at the six observing stations are suggestive of local chemical action.

3. Convection of heat has proved a very troublesome disturbing cause.

As to convection of heat by air in a shaft or well not filled with water, evidence will be found in the second report both in the case of Mr. Hunter's observations in the shafts of two salt mines at Carrickfergus, having the depths of 570 and 770 feet respectively, and in the case of Mr. Symons' observations at Kentish Town, where the first 210 feet of the well are occupied with air. At the depth of 150 feet the temperature was 52.1 in January, and 54.7 in July.

Convection of heat by water in old shafts which have been allowed to become flooded, is very manifest in some of the observations communicated by Mr. Burns in the second and fourth reports. In Allendale shaft (Northumberland), 300 feet deep, with about 150 feet of water the temperature was practically the same at all depths in the water, and this was also the case in Breckon Hill Shaft where the observations extended from the depth of 42 feet to that of 350 feet. A similar state of things was found in a shaft at Ashburton (Devon) by Mr. Amery, who observed at every fiftieth foot of depth down to 350 feet.

Convection by water in the great well at La Chapelle, 660 m. (2165 feet) deep, and 1.35 m. (4 feet 5 inches) in diameter at the bottom, appears probable from the following comparisons:

Very concordant observations (communicated by M. Walferdin to *Comptes rendus* for 1838) at three different wells in the Paris basin of the respective depths of 263 m., 400 m., and 600 m., show by comparison with one another, and with the constant temperature in the artificial caves under the Paris Observatory a rate of increase of 1° F. in 56 or 57 feet. These data would give, at the depth of 100 m., or 328 feet, a temperature of 57°, and at the depth of 660 m., or 2165 feet, a temperature of 90°; whereas the temperatures actually observed at those depths in the well at La Chapelle in October, 1873, when the water had been undisturbed for a year and four months, were 59°.5 and 76°. It thus appears probable that the upper part of well is warmed, and the lower part cooled, by convection. Further light may be expected to be thrown on this point when the well reaches the springs, and the water spouts above the surface, as it does at the Puits de Grenelle. A letter received by the secretary in July, 1882, states that the engineering difficulties have prevented any deepening of the well since the above observations, but that arrangements for this purpose have now been made.

More certain and precise information as to the effect of convection in deep bores is furnished by the experiments of Herr Dunker at Sperenberg. The principal bore at Sperenberg has a depth of 4052 Rhenish, or 4172 English feet, and is entirely in rock salt, with the exception of the first 283 feet. Observations were first taken (with a maximum thermometer on the overflow principle) at numerour depths, from 100 feet to the bottom, and showed a fairly regular increase of temperature downwards. The temperature at 700 feet was 16°.08 R., and at 3390 feet 34°.1 R. Plugs were then contrived which could be fixed tight in the bore at any depth with the thermometer between them, or could be fixed above the thermometer for observing at the bottom. Convection was thus prevented, and a difference of one or two degrees Réaumur was found in the temperatures at most of the depths; at 700 feet the temperature was now 17°.06 R., and at 3390 feet 36°.15. We have thus direct evidence that convection had made the temperature at 3390 feet 2°.05 R., or 4°.6 F. too low; and this, as Herr Dunker remarks, is an under-estimate of the error, inasmuch as convection had been exerting its equalizing action for a long time, and its effect could not be completely destroyed in the comparatively short time that the plugs were in position. Again, as regards the effect of convection on the upper part of the bore, the temperature 11°.0 R. was observed at the depth of 100 feet in the principal bore when no plugs were employed, while a second bore only 100 feet deep in its immediate vicinity showed a temperature 9°.0 R. at the bottom. This is direct evidence that the water near the top of the great bore had been warmed 2° R., or 4½° F. by convection.

Suggestions for observations in filled

up bores will be found in the eleventh report, but they have not yet taken a practical shape.

D. Questions Affecting Deductions from Observations.—1. In many instances the observations of temperature have been confined to considerable depths, and in order to deduce the mean rate of increase from the surface downwards it has been necessary to assume the mean temperature of the surfuce. To do this correctly is all the more difficult, because there seems to be a sensible difference between the mere temperature of the surface and that of the air a few feet above it.

In the third report some information on this point is given, based on observations of thermometers 22 inches deep at some of the stations of the Scottish Meterological Society, and of thermoneters 3 (French) feet deep at Greenwich and at Edinburgh. These observations point to an excess of surface-temperature above air-temperature, ranging from half a degree to nearly two degrees, and having an average value of about one degree.

Dr. Schwartz, Professor of Physics in the Imperial School of Mines at Schemnitz, in sending his observations made in the mines at that place, remarks on this point:

"Observations in various localities show that in sandy soils the excess in question amounts, on the average, to about half a degree Centigrade. In this locality the surface is a compact rock, which is highly heated by the sun in summer, and is protected from radiation by a covering of snow in winter; and the conformation of the hills in the neighborhood is such as to give protection against the prevailing winds. Hence the excess is probably greater here than in most places, and may fairly be assumed to be double the above average."

Some excellent observations of underground temperature at small depths were made at the Botanic Gardens, Regent's Park, London, for the six years 1871-76, along with observations of air temperature, and have been reduced by Mr. Symons. They are at depths of 3, 6, 12, 24, and 48 inches beneath a surface of grass, and their joint mean derived from readings at 9 A. M. and 9 P. M. for the six years is 49.9, the mean for the 48 inch thermometer being 50.05. The mean air-temperature derived in the same way from the readings of the dry-bulb thermometer is 49.6. Hence it appears that the excess of soil above air is in this case about 0°.4.

Quetelet's observations for three years at Brussels (p. 48 of his "Mémoire") make the earth, at depths less than 1¼ foot, colder than the air, and at greater depths warmer than the air.

Caldecott's observations for three years at Trevandrum, in India, make the ground at the depth of 3 feet warmer than the air by 5°.7 F.

Dr. Stapff, in his elaborate publications on the temperature of the St. Gothard Tunnel, arrives at the conclusion that the mean temperature of the soil on the surface of the mountain above the tunnel is some degrees higher than that of the air, the excess increasing with the height of the surface and ranging from 2° or 3° C. near the ends of the tunnel, to 5° or 6° in the neighborhood of the central ridge.

2. Connected with this is the question whether the mean annual temperature of the soil increases downwards from the surface itself, or whether, as is sometimes asserted, the increase only begins where annual range ceases to be sensible—say at a depth of 50 or 60 feet.

The general answer is obvious from the nature of conduction. Starting with the fact that temperature increases downwards at depths where the annual range is insensible, it follows that heat is traveling upwards, because heat will always pass from a hotter to a colder stratum. This heat must make its way to the surface and escape there. But it could not make its way to the surface unless the mean temperature diminished in approaching the surface; for if two superposed layers had the same mean temperature, just as much heat would pass from the upper to the lower as from the lower to the upper, and there would not be that excess of upward flow which is necessary to carry off the perennial supply from below.

This reasoning is rigorously true if the conductivity at a given depth be independent of the temperature, and be the same all the year round. By "conductivity" we are to understand the "flux of heat" divided by the "temperature-gradient;" where by the "flux of heat" is meant the quantity of heat which flows in one

second across unit-area at the depth considered, and by the "temperature-gradient" is meant the difference of temperature per foot of descent at the depth and time considered.

Convection of heat by the percolation of water is here to be regarded as included in conduction. If the conductivity as thus defined were the same all the year round, the increase of mean temperature per foot of depth would be independent of the annual range, and would be the same as if this range did not exist.

As a matter of fact, out of six stations at which first-class underground thermometers have been observed, five show an increase downwards, and one a decrease. The following are the results obtained for the depths of 3, 12, and 24 French feet:

	3 ft.	12 ft.	24 ft.
Brussels, 3 years	51.85	53 69	53.71
Edinburgh (Craigleith) 5 years	45.88	45.92	46.07
" (Gardens five years	46.13	46.76	47.09
" (Observatory), 17 years	46.27	46.92	47.18
Trevandrum (India), 3 years	85.71	86.12	—
Greenwich, fourteen years	50.92	50.61	50.28

In calculating the mean temperature at 12 feet for Trevandrum, we have assumed the temperature of May, which is wanting, to be the same as that of April.

Omitting Trevandrum, and taking the mean values at 3 and 24 French feet, we find an increase of 6.56 of a degree in 21 French feet, which is at the rate of 1° for 32 French, or about 34 English feet.

3. Another question which it has sometimes been necessary to discuss is the influence which the form of the surface exerts on the rate of increase of temperature with depth.

The surface itself is not in general isothermal, but its temperature is least where its elevation is greatest; the rate of decrease upwards or increase downwards being generally estimated at 1° F. for 300 feet. This is only about one fifth of the average rate of increase downwards in the substance of the earth itself beneath a level surface. If the two rates were the same, the isotherms in the interior of a mountain would be horizontal, and the form of the surface would have no influence on the rate of increase of temperature with depth. The two extreme assumptions that the surface is an isotherm, and that the isotherms are horizontal, lie on opposite sides of the truth. The isotherms, where they meet the sides of the mountain slope in the same direction as the sides of the mountain, but to a less degree. Probably the tangents of the two slopes are generally about as 3 to 4.

Further, if we draw a vertical line cutting two isotherms, the lower one must have less slope than the upper, because the elevations and depressions are smoothed off as the depth increases.

The practical inference is that the distance between the isotherms (in other words, the number of feet for 1° of increase), is greatest under mountain crests and ridges, and is least under bowl-shaped or trough-shaped hollows.

The observations in the Mont Cenis tunnel, and the much more complete observations made by Dr. Stapff in the St. Gothard tunnel, fully bear out these predictions from theory. The discussion of the former occurs in the fourth report, p. 15.

As regards the St. Gothard tunnel, Dr. Stapff reports: "The mean rate of increase downwards in the whole length of the tunnel is .02068 of a degree Centigrade per meter of depth, measured from the surface directly over. This is 1° F. for 88 feet. Where the surface is a steep ridge the increase is less rapid than this average; where the surface is a valley or plain the increase is more rapid."

4. The question whether the rate of increase downwards is upon the whole the same at all depths, was raised by Prof. Mohr in his comments upon the Sperenberg observations, and is discussed so far as these observations bear upon it, in the 9th and 11th reports.

Against the Sperenberg observations, which upon the whole show a retardation of the rate of increase as we go deeper, may now be set the Dukinfield observations begun by Sir William Fairbairn, and continued by Mr. Garside. Taking Mr. Garside's observations, and assuming a surface temperature of 49°, the increase in the first 1987½ feet is at the rate of 1° in 79.5 feet; in the next 420 feet it is at the rate of 1° in 70 feet, and in the last 283½ feet it is at the rate of 1° in 51½ feet.

From a theoretical point of view, in places where there is no local generation of heat by chemical action, the case stands thus:

The flow of heat upwards must be the same at all depths, and the flow is equal to the rate of increase downwards, multiplied by the conductivity, using the word "conductivity" (as above explained) in such a sense as to include convection. The rate of increase downwards must, therefore, be the same at all depths at which this conductivity is the same.

This reasoning applies to superposed strata at the same place, and assumes them to be sufficiently regular in their arrangement to ensure that the flow of heat shall be in parallel lines, not in converging or diverging lines.

5. If we have reason to believe that the flow of heat upwards is nearly the same at all places, then the above reasoning can also be applied approximately to the comparison of one place with another; that is to say, the rates of increase downwards, in two masses of rock at two different places, must be approximately in the inverse ratio of their conductivities. In the cooling of a heated sphere of heterogeneous composition, the rates of flow would at first be very unequal through different parts of the surface, being most rapid through those portions of the substance which conducted best; but these portions would thus be more rapidly drained of their heat than the other portions, and thus their rates of flow would fall off more rapidly than the rates of flow in the other portions. If the only differences in the material were differences of conductivity, we might on this account expect the outflow to be after a long time nearly the same at all parts of the surface. But when we come to consider differences of "thermal capacity per unit volume," it is clear that with equal values of "diffusivity," that is of "conductivity divided by thermal capacity of unit volume" in two places, say in two adjacent sectors of the globe, there would be the same distribution of temperature in both, but not the same flow of heat, this latter being greatest in the sector in which the capacity and conductivity were greatest.

Where we find, as in Mr. Deacon's observations at Bootle, near Liverpool, and to a less marked degree in the observations of Sir William Fairbairn and Mr. Garside, near Manchester, an exceptionally slow rate of increase, without exceptionally good conductivity, it is open to us to fall back on the explanation of exceptionally small thermal capacity per unit volume in the underlying region of the earth, perhaps at depths of from a few miles to a few hundred miles.

6. A question which was brought into consideration by Prof. Hull, in connection with the great difference between the rate of increase at Dukinfield and that at Rosebridge, is the effect of the dip of the strata upon the vertical conduction of heat. Laminated rocks conduct heat much better along the planes of lamination than at right angles to them. If k_1 denote the conductivity along, and k_2 the conductivity normal to the planes of lamination, and if these planes are inclined at an angle θ to the horizon, the number of feet per degree of increase downwards corresponding to a given rate of outflow through the surface, will be the same as if the flow were vertical with a vertical conductivity:

$$k_1 \sin^2 \theta + k_2 \cos^2 \theta.$$

Prof. Herschel finds about 1.3 as the ratio of the two principal conductivities in Loch Rannoch flagstone, and 1.875 as the ratio in Festiniog slate.

The dip of the strata at Dukinfield is stated by Mr. Garside to be 15°, and we have $\sin^2 15° = .07$, $\cos^2 15° = .93$.

If we assume $k_1 = 1.3\, k_2$, as in the case of flagstone, we find for the effective vertical conductivity $k_2\,(.09 + .93) = 1.02\, k_2$, so that the number of feet per degree would only be increased by 2 per cent.

It is not likely that the two conductivities in the strata at Dukinfield are so unequal as even in the case of flagstone, so that two per cent. is a high estimate of the effect of their dip on the vertical rate of increase so far as pure conduction is concerned. The effect of dip in promoting the percolation of water is a distinct consideration, but the workings of the Dukinfield mines are so dry that this action does not seem to be important.

A New Cable.—The London *Times* says a new submarine cable will shortly be laid between France and Senegal.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS. —A regular meeting of the American Society of Civil Engineers was held on the evening of March 7th, 1883.

The ballots for the Amendments to the Constitution were canvassed.

A paper by Hamilton Smith, Jr., M. Am. Soc. C. E. of San Francisco, on the "Flow of Water in Pipes," was read. This paper gave the results of many experiments upon the discharge of water through circular pipes ranging from one-half an inch to 4 feet in diameter, and with velocities from one-sixth of one foot to twenty feet per second.

ENGINEER'S CLUB OF PHILADELPHIA.—Regular meeting March 3d. Mr. Horace See described the S. S. "Mariposa," built for the Oceanic S. S. Co. of San Francisco, Cal., by the Wm. Cramp & Sons S. and E. B. Co., of Philadelphia.

The Secretary presented, for Prof. Mansfield Merriman, a description of a Graphic Solution of Cubic Equations.

Mr. Wm. P. Osler presented a table for Bolts, Nuts and Threads, prepared by Mr. H. C. Slaney, Gas Engineer, and himself.

The Secretary presented tracings of device for holding transit pole vertically, and shelf for holding field instruments in vertical position while temporarily out of use in office, contributed to the Club by Mr. Chas. E. Chandler, Civil Engineer, Norwich, Conn. The former consist of a bubble tube, framed at right angles to the rod, and the latter of a shelf notched out to admit the legs under the tripod head and provided with a slide in front to retain the instrument in place.

The Secretary read a communication from C. René, of Stettin, describing a process for the drying of wood, intended especially for the preparation of wood for musical instruments, but perhaps otherwise useful. It is described as follows:

"The wooden boards are so arranged in a large iron kettle, that gases may freely circulate over their entire surface, and exposed, in the first place, for twelve hours to the drying effects of hot air: after this the kettle is closed, reheated by the apparatus below and the air exhausted, when the kettle is filled with the oxygen ozonized by electrical sparks passing continually between two points of platina, forming the end poles of two wires conducted through tubes of glass into the kettle. The ozone is said to act so energetically upon the heated wood that it consumes the destroying resinous, oily or other parts in from 12 to 24 hours."

REGULAR MEETING, FEB. 17TH.

The Secretary presented, for Mr. J. M. Stewart, the following description of the Delaware Breakwater Harbor Improvement.

The construction of the Delaware Breakwater was commenced in the year 1829, upon plans submitted by a Board of Commissioners appointed by the Congress of the United States.

The project of the Board contemplated the construction, in the concavity of the Delaware Bay, just inside Cape Henlopen, of two massive works on the "pierres perdues" or rip-rap system, separated by an interval or "gap" of about one-quarter of a mile; the greater called the "Breakwater," to be about twenty-six hundred (2600) feet long, and fourteen (14) feet above low water, to afford safe anchorage during gales from the north and east; the other called the "Ice Breaker," to be about one-half the length of the greater work, and of the same height, to protect shipping against northwesterly gales and heavy drifting ice of the bay.

During ten years the progress of construction was such, that by 1839, eight hundred and thirty-five thousand (835,000) tons had been deposited. From this time on, work was done irregularly, until 1869, when the works were completed as they stand at present.

The Breakwater Harbor for many years fulfilled, so far as its capacity enabled it, the design of its projectors in protecting the commerce of the country. But the growth of this commerce, particularly the last twenty-five years, has so far exceeded possible anticipation, as to practically exclude more than a fractional part from the intended shelter. At various times, therefore, projects looking to the enlargement of the protected area have been the subject of deliberation by officers and boards of engineers; and the matter received renewed attention during the fiscal just closed, from the reference by the Chief of Engineers of the U. S. A., to a report of Major Wm. Ludlow, Corps of Engineers, U. S. A., showing by comparison of a recent survey with former ones, that not only was the protected area yearly diminishing relatively to the amount of shipping seeking shelter, but was also undergoing a positive and serious deterioration from a marked decrease in depth. In view of this deterioration, a Board of Engineers recommended as a possible remedy, the closure of the "Gap" existing between the Breakwater and adjoining Ice Breaker, which, it is believed, will, to a greater or less extent, check further deposits within the harbor and remove those existing on the shoals in its vicinity, and also increase the protected area of anchorage nearly four-fold.

It is proposed to close this "Gap" by means of a concrete superstructure resting upon a granite rip-rap foundation, described generally as follows:

A bridge of creosoted timber is first to be constructed completely across the "Gap," with bays of sixteen feet long. Each trestle is to be formed of three piles, the center pile vertical, the others inclined, and forming with the cap a profile for the concrete work. A double railroad track is to be laid on the bridge for the transportation of material.

Upon the completion of the bridge, it is proposed to begin the deposit of the rip-rap (the granite used weighing not less than one ton, the average being three to the block), laying the same uniformly across the "Gap" to prevent scouring of the bottom, and to gradually raise the height of the rip-rap to twelve (12) feet below mean low water, with a width of

forty-eight (48) feet on top, and a side slope of one-half on one.

The concrete superstructure is to begin twelve (12) feet below low water, with a base of twenty-four (24) feet wide, height twenty-four (24) feet, side slope of four (4) on one (1), and a width on top of twelve (12) feet.

The concrete is to be built in blocks of the above cross section and length of sixteen (16) feet, corresponding to the length of the bays of the bridge. For this purpose the bays are to be enclosed with detachable aprons forming boxes to be filled in succession, the boxes containing at each end a triangular re-entrant, which, after the two adjoining boxes have been filled, can be taken down and likewise filled, forming square plugs to bind together the larger blocks. To hold the said aprons in position, tie-rods will be used, passing through pipes, the extremities of which will be flush with the exterior surface of the concrete.

The aprons and tie-rods used in the construction of each block can be removed and used again as soon as the mass shall have attained the requisite solidity.

The closing of the "Gap" between the Ice Breaker and Breakwater will be begun at once, and it is expected that the work will be completed in about five years.

Dr. H. M. Chance reported his experience with the Loiseau Fuel in domestic use. He found the ash low in weight, but high in bulk. His conclusion was that it is a good fuel for open grates and for manufacturing purposes.

The Secretary called attention to a letter, from Mr. E. H. Talbott, Secretary of the National Exposition of Railway Appliances, requesting co-operation in obtaining, or bringing to his notice, any articles, new or old, appropriate to that Exposition.

ENGINEERING NOTES.

THE NEW MOTIVE POWER AND THE ST. GOTHARD RAILWAY.—The opening of the St. Gothard Railway has given rise to a proposal the magnitude of which can scarcely be overrated. At present that new railway, as well as the North Italian lines, labor under the serious drawback of dear fuel, whereas the lines of access to Antwerp and Marseilles obtain their supplies of coals on much more favorable terms. If this difficulty could be overcome, the freight rates on the St. Gothard system might be considerably reduced, to the great advantage of all concerned. The line traverses streams which, properly utilized, would give almost limitless water power, and experiments are being made with a view to transmuting, by means of electricity, this potential power into a force that will render it possible to run trains from the Rhine at Basel to the shores of Lake Maggiore without any fuel whatever. In this way may Switzerland's want of minerals be more than compensated by the abundance of water with which nature has gifted her. The idea is a grand one, but it is not at all out of keeping with the great progress made in electrical science, notably with that branch of it which deals with the conversion of electricity into motive power. It should be remembered that we are as yet only on the threshold of an inquiry which may produce unlooked-for results.

OCEAN CABLES.—The two great cable manufacturing companies, viz., the Telegraph Construction and Maintenance Company, and the Silvertown Company, have between them manufactured and laid something like 7000 miles of cable during the year. The following is, we think, a fairly complete list of the cables: Trieste-Corfu; Malta-Tripoli; Alexandria-Port Said—manufactured by the Telegraph Construction and Maintenance Company for the Eastern Telegraph Company; the Grutsiel-Valentia cable, manufactured by the same company for the German Union Company; the Ceara-Maranham, Madeira-Lisbon, and Lipari-Salina cables, also manufactured by the Telegraph Construction Company. The Silvertown Company has been very busy during the year over the West Coast of America and other cables. The sections of these cables are Chorillos Payta, Payta-St. Helena, Galveston-Brownsville, St. Helena-Buenaventura, Buenaventura-Pearl Island, Salina Cruz-Libertad; Libertad S. Jaun del Sur; S-Juan del Sur-Pearl Island. These are all, as will be seen, from America, as were several others. The company manufactured the Saghalien Tartary cable for the Great Northern Company, the Aberdour Granton cable for the Post-office, and some others. Messrs. Siemens have, it seems, done little cable work during the year, the principal being the Land's End Dover Bay, and the Jeddah-Suakin cables. The total lengths of these cables, as we have said, is about 7000 miles, and the manufacture has been pretty equally distributed between the two great companies. Roughly speaking, we may estimate the cost of making and laying a cable at some £200 per mile more or less, and this would give the capital for the year's work at something like a million and a half. The largest cable company is the Eastern, which, with its connections, has a system comprising about 22,000 miles of cable, and if to this the system of the Eastern Extension is added—for the whole is really one system—the mileage reaches nearly 33,000 miles. During the year some notable repairs have been effected by the ships and staff of the Telegraph Construction and Maintenance Company. The Kangaroo repaired the 1874 Atlantic cable off Newfoundland in the spring; in August the Seine repaired the Lisbon-Porthcurnow cable in 2700 fathoms of water off the coast of Spain. During the laying of the duplicate Madeira-Lisbon cable a new shoal was found and the course of the cable diverted. The shoal rises to within 100 feet of the surface, and the depth of water rapidly increases to over 2000 feet. The shoal is now marked on the Admiralty charts as the Seine shoal. A similar shoal was found along the original line of cable, and is known as the Gettysbury shoal.

IRON AND STEEL NOTES.

CHILLED IRON VS. STEEL.—In the discussion following Prof. Abel's paper on "Hardening of Steel," before the Institution of Mechanical Engineers, Sir Frederick Bramwell made the following inquiry:—

"I should be very glad, sir, if Prof. Abel, in his remarks in reply, would touch upon the question of whether we may look upon the condition of carbon in hardened steel as analogous to that of carbon in chilled cast iron. We know that we have cast iron, which, before chilling, contains—to judge by the eye—carbon intermixed, as it were, with the iron, but if you chill it, you get a uniform structure. I should like him to say, if there is anything in it, whether the same reasoning that prevails as regards the hardening of iron by chilling, under those circumstances, prevails as regards the tempering of steel. In speculating upon this matter, for it has amounted to nothing more, I have often thought that the operation of hardening steel was similar to that of chilling iron, and that it had the effect of preventing the separation out of the carbon from the particles of iron which might take place if the cooling was gradual; and that in that way it was retained within the iron and made therefore permanent, either as a chemical compound or as an alloy, and so causing the particular properties of the hardened or tempered metal. It has occurred to me that if this were so, one might perhaps be able to attain the same end in steel or in molten iron by subjecting it to violent agitation during the whole time the cooling was taking place—that is to say, let the cooling be gradual, but let the agitation go on. I have in my mind that which one does with certain mixed liquids. If you allow them to cool gradually they will separate out, but if you cool them rapidly, and during the time of cooling keep a uniformity or mixture, you prevent the separation. It seems to me, if the effect of chilling molten cast iron or of hardening the steel is to prevent the separation out, that probably—I do not say at all certainly—the same result might be obtained by the agitation which would prevent the quiescent separating out in the case either of the cast iron or of the steel; and that in that way one might, as it were, by another process check the conclusions, arrived at by a chemical analysis.

To which Professor Abel replied:—With regard to the questions raised by Sir Frederick Bramwell, I can only venture to offer a very guarded opinion. I confess that I have always regarded the condition of carbon in chilled iron as analogous, if not similar to, the condition of carbon in hardened steel. So far as a chemical comparison of these two materials go it would appear as if the sudden arresting of the effect of heat, or the sudden bringing down from a highly-heated comparatively cold condition, the mass of the material, be it cast iron or be it steel, has an analogous effect upon the condition of the carbon in the material. There is, of course, this important difference between the two materials which we are dealing with—that in the case of the cast iron we are acting upon the liquid in the fluid condition, whereas in the case of hardening steel we are acting upon the material in a solid condition. Therefore, there is an important element of difference between the two cases. How far this may effect the result I hope the experiments upon which we have embarked, if we have the courage to continue them sufficiently far, may throw some light. It is right that in reference to this I should refer to some interesting experiments with which, no doubt many of the members may be already acquainted, and which have been made by M. Clemendeau, in which he has, by steel raised to a red heat, then subjecting it to powerful pressure, obtained results very similar—so far as regards the physical characters of the products—to those produced by the hardening and tempering process; and he claims for this particular method of treating steel that while he can produce a degree of hardness and variety of temper, just similar to that which is produced by the ordinary hardening and tempering process, he also produces an effect which is permanent, and which is not subsequently affected by considerable variations of temperature. I think the results, as I have seen them, certainly have some promise, and that it is worth while to examine into them more closely; and with this view it is intended by us at the Arsenal to make some experiments —upon which we have already embarked—with a view to ascertain whether M. Clemendeau's results—which, as I say, appear so far to be promising—are fully borne out in actual practice.

ON THE EFFECTS OF COMPRESSION ON THE HARDNESS OF STEEL.—Experiments were made at the ironworks of Saint Jacques at Montlucon confirmatory of the results already presented by Mr. Dumas in the name of Mr. Clemandot, that in compressed steel there was an increased hardness as compared with uncompressed. Further results have been obtained. Steels have been analyzed, compressed and uncompressed, containing different proportions of carbon. The proportion of combined carbon, as regards the total carbon, has already been found to be greater in the compressed as compared with the non-compressed steels. The experiments were made on elongated shot, the samples being taken from four different points in the depth, and the combined carbon was tested by the Eggertz process, and the total carbon by Bossingault's. The comparative results are so constant that one table of results will suffice:

	Compressed.		Uncompressed.	
Carbon, total per cent	0.70		0.70	
Combined carbon at A	0.60	Mean 0.585	0.49	Mean 0 49
B	0.59		0 50	
C	0 55		0.47	
D	0.60		0.50	
Free carbon by difference	0.115		0.21	

Thus the compressed steel has more combined and less uncombined carbon. The same results were obtained by sudden cooling in the

ordinary manner of hardening. Hence compression produces the same physical effects as sudden cooling in steels.

RAILWAY NOTES.

NARROW-GAUGE RAILWAYS IN ALGERIA.—A paper on "Narrow-Gauge Railways in Algeria" was recently read by M. Fousset, before the French Society of Civil Engineers, of which a brief account will be found interesting. The superiority of narrow-gauge to broad-gauge railways for Algeria springs from the nature of the trade of the country. As the products of all the provinces are the same, there is little internal traffic, and everything is sent to the sea for export. Thus the gross receipts of the railway from Algiers to Oran (gauge 4 feet 8½ inches) amounted to 12,-880 francs only per kilometer per annum, and the speed is only 18 miles an hour, which would be low for a narrow-gauge line. For a long time to come the main traffic will be in agricultural products for which lines of 1.10-meter (3.6-foot) gauge will amply suffice; in some cases, indeed, a 0.75-meter (2½-foot) gauge might be laid down with advantage. Further, the soil contains many heavy clay beds, which necessitate wide curves to avoid them; as otherwise the heavy rains and the torrent-like water courses which occur frequently in different localities expose the line to accidents which it is impossible to provide against; slips in this clay cannot be dealt with till dry. M. Fousset, therefore, erected wooden bridges and light viaducts over them, and often made a curve of 80 meters in order to avoid them altogether. The lightness of materials greatly facilitated these works. M. Fousset considers that in making such railways it would be expedient to let to one contractor at least 1,000 kilometers (600 miles) starting from the sea towards the desert in order that he might at once have ample supplies of material, and so organize the management as that all officials of the line should serve by turn on different portions, with a view to the preservation of health. On the present development of narrow-gauge railways, M. Fousset gives the following statistics:

	Kilometers.
Lines in Scandinavia (opened 1880).........	1,656
Lines in New Zealand (working or in course of construction)......	647
Lines in Isle of Reunion.	132
Lines in Senegal (in course of construction)	500
Lines in Brazil (under contract in 1881)......	5,374
Total..............	8,309 or 5,157 miles.

In the last-named country, 4,748 kilometers (2,950 miles) are laid down with a 1-meter gauge, which has been deliberately adopted as the best. The receipts of these lines vary between 7,000 and 22,000 francs per kilometer. Their carrying power is amply sufficient. On the line between Arzew and Mecheria (Oran) goods engines draw the following loads at a speed of from 15 to 20 kilometers (9 to 12 miles) per hour: On the sections with gradients below 1 in 100, 250 tons gross; on long inclines of 2.7 in 100, 75 to 80 tons gross. In July, 1881, Generals Saussier and Delebuque decided that a railway must at once be laid down from the nearest station on the Arzew and Saida Railway to Mecheria, 115 kilometers (71 miles) distant, this being the farthest military outpost stationed to suppress the Algerian insurrection. The Franco-Algerian Railway Company undertook to lay down the first 34 kilometers in 100 days, and to complete the entire 115 kilometers in 250 days. The order was given to M. Fousset on August 6. The 34 kilometers were laid down in 52 days, and 128 days sufficed for the first 76 kilometers, extending to Ben Senia. The winter of 1881–82 was exceptionally severe in Algeria; but although severe floods interrupted the works for 70 days the line reached Mecheria on the 239th day, viz. April 2, 1882. M. Fousset considers such results could not be obtained on a railway whose gauge was 1.45 meter. On these grounds he pronounces in favor of a narrow-gauge system for the whole of the Algerian railways.

DURING the ensuing year, surveys will be made, says the St. Petersburg correspondent of the *Times*, for the following new lines of railway: From Samara through Ufa to Omsk, from Ufa to Ekaterinburg, a continuation of the Ural Railway from Ekaterinburg to join the above Samara-Omsk line, and further on, in the direction of Troitsk, from Tmerioka to Novselsk, the east Donetz line; from Lugan to Millerovo, the branch to Novorossisk, from Wioldou to Toukoum, and from Ryieff to Viazma.

THE new departure of the Midland Railway Company, in the business of hiring wagons, has so far been successful. Many colliery companies are giving up the keeping of their own wagons, and leasing from the Midland, whose terms are certainly reasonable. For fifty miles and under, on its own line, wagons can be had at 6d. per ton, the rate rising to 9d. and 1s. when other companies' lines are travelled over, and the distance is considerable. The Midland is certainly in a good position for doing a business of this sort on conditions which private firms or even limited companies could scarcely offer. It has the labor to its hand, and beyond the mere cost of making the wagons and keeping them in good repair there is very little left in the way of heavy outlay.

ORDNANCE AND NAVAL.

AN AMERICAN STEAMSHIP OF THE FUTURE.—From a Boston paper, whose specia correspondent has just visited the dome-deck ship *Meteor*, recently launched from a shipyard at Nyack, on the river Hudson, we gather that many besides the designer and builder of this remarkable vessel are confident she will revolutionize the carrying trade of the merchant marine service of the world. According to

Captain Bliven, the designer, "a gentleman who has been building ships on the Clyde for upwards of fifty years" visited the vessel the other day, and, after a minute inspection, gave it as his opinion that ships built on the *Metor* lines were destined to supersede the present style of ocean ships. The qualities said to be obtained in vessels of this new type are powers of self-righting, great speed and unsinkableness. Trial of the *Meteor* is to be made shortly, and if the results are such as are anticipated, the building of large ocean steamships on the same principle will be immediately proceeded with. Concerning speed, it is expected the new vessel, which is comparatively small, and intended only as an experimental craft, will attain from 25 to 28 miles an hour. Machinery of an improved type is fitted, differing, it is said, in every material respect from that now in general use. Doubts are entertained by some regarding this feature of the noval craft, but one of the most prominent engine builders of New York has assured a director of the company which has been formed to promote the building of vessels of the *Meteor* type, that should the machinery not come up to expectation, he can put into the steamship "an engine that will certainly drive her from 23 to 25 miles an hour." No apprehensions on this score, however, are felt, and the company, having ample funds, is prepared to meet any emergency of the kind. Meantime, great caution is observed in allowing visitors on board the experimental vessel. Captain Bliven states that "gentlemen in whom I have the greatest reliance warn me to beware of emissaries from English shipbuilders, who I know myself, don't like the idea of an American model superseding the old style of ocean vessel. I keep a watch on the vessel night and day, and after dark the watch is armed. It would not be a very difficult job for evil-disposed persons to row alongside under cover of darkness and adjust a torpedo and sink the *Meteor* just as sunshine is about to brighten the end of my long months of labor!" This, we must say, is a genuine Yankee notion. The idea of our peace-loving British shipbuilders, in despair at their defeat, resorting to the "policy of dynamite" is matchless for its originality.—*Iron.*

SOUND-SIGNALLING IN FOGS AT SEA.—Considering the terrible loss of life occasioned by steam vessels colliding at sea in foggy weather, some systematic means of signalling by sound is imperatively called for. The danger will not diminish, but is sure to increase, from year to year with the increasing number and size of the steamships which crowd the approaches to our coasts and harbors. At present, scarcely any recognized rules and certainly no systematic code of rules obtain, and two or even three vessels, becoming suddenly aware of each other's proximity, in their anxiety to avoid collision, set up a chorus of blasts and screeches with their whistles which only serve to make confusion worse confounded. In stating this, we are simply reiterating what has again and again been urged in our columns and elsewhere, and as persistently ignored or forgotten by those from whom some decided action should be expected. The sinking of the *City of Brussels* and the still more recent calamity befalling the *Cimbria*, with the appalling loss of life attending it, are casualties which have once more forced the subject on people's minds. Will the present movement be of effect? Or must we wait until some morning we read with horror of two of the large Transatlantic mail and passenger steamships telescoping each other, and hundreds of souls being hurried out of existence, with, perhaps, a lord of the treasury, an archbishop or two, and possibly a member of the royal family amongst them, to give to the event the requisite significance. Surely, enough has already transpired to show the paramount need there is for some serious international consideration of the subject of establishing a code of sound signals, which, if made law by all maritime nations, would prevent many of the fearful disasters now so frequent.—*Iron.*

BOOK NOTICES

PUBLICATIONS RECEIVED.

THROUGH the kindness of Mr. James Forrest we are in receipt of the following published papers of the Institution of Civil Engineers:

Current-Meter Observations in the Thames, by Prof. Wm. Cawthorne Unwin, M. I. C. E.

The Sinking of Two Shafts at Marsden, by John Daglish, M. I. C. E.

Track-laying in Canada in 1882, by Henry Purdon Bell, M. I. C. E.

TRANSACTIONS OF THE AMERICAN INSTITUTE OF MINING ENGINEERS.

PERPETUAL CALENDAR. By Pres't Barnard.

THE NEW JERSEY WEATHER REVIEW.

THE ELEMENTARY PART OF A TREATISE ON THE DYNAMICS OF A SYSTEM OF RIGID BODIES. By Edward John Routh, LL.D.; F. R. S. London: MacMillan & Co. Price $3 75.

The first part only of a complete treatise is represented by this volume. The table of contents affords the following guide to the range of topics.

Chap. I. Moments of Inertia. II. D'Alembert's Principle. III. Motion about a Fixed Axis. IV. Motion in Two Dimensions. V. Motion in Three Dimensions. VI. On Momentum. VII. Vis Viva. VIII. Lagrange's Equations. IX. Small Oscillations. X. On Some Special Problems.

The volume is a royal octavo of 385 pages and is beautifully printed.

A CHRONOLOGICAL HISTORY OF THE ORIGIN AND DEVELOPMENT OF STEAM NAVIGATION. By Geo. Henry Preble, Rear Admiral U. S. N. Philadelphia: L. R. Hamersly & Co. Price $3 50.

The author believes this book to contain more facts relating to the progress of steam navigation than have ever before been gathered

into one book. It professes to be nothing more than a mere epitome of history, and as such is of value to a certain class of students, but will hardly prove of interest to the general reader.

PRACTICAL MICROSCOPY. By George E. Davis, F. R. M. S. Second Edition. London: David Bogue. Price $8 00.

This work is arranged on the plan of Quekett's Treatise on the use of the microscope, which has been out of print for some years.

A liberal amount of space is devoted to the preparation of microscopic sections.

The uses of the polariscope and micro-spectroscope are particularly described.

LECTURES ON THE SCIENCE AND ART OF PLUMBING. By S. Stevens Hellyer. London: B. T. Batsford. Price, $2 00.

This is a series of six lectures prepared at the request of the National Health Society, London, and delivered in the hall of the Society of Arts.

The subjects treated by chapters are: joints and pipe-bending; the necessity of traps; traps and trap ventilation; water-closets, soil pipes, and waste pipes; house drainage and ventilation.

The illustrations are good and tolerably abundant.

WATER: ITS COMPOSITION, COLLECTION. AND DISTRIBUTION. By Joseph Parry, C. E. London: Frederick Warne & Co, Price, $1 00.

This brief essay fairly covers a great subject. It is designed as a practical handbook for domestic and general use, and for such purposes it is admirably constructed. Free from technical phraseology, it still contains all information needful for the general reader regarding water supply.

A few plain illustrations, mainly of English devices, illustrate that portion of the book that relates to house distribution.

THE MODERN HOUSE CARPENTER'S COMPANION AND BUILDER'S GUIDE. By W. A. Sylvester. Boston: A. Williams & Co. Price, $1 25.

This is an elementary treatise on drawing plans and making estimates for ordinary carpentry.

The instructions are brief, and for a beginner with but little ambition would probably answer a good purpose.

MISCELLANEOUS.

THE LAY TORPEDO.—Colonel Lay has recently submitted his torpedo to a severe test in the Bosphorus by discharging it over a course of a mile at a target only 60 feet long. The path of the projectile was crossed by three distinct currents, of which two flowed slowly upward, and one strongly downward. In addition to this the sea was very lumpy, especially at the junctions of the currents. Yet, in spite of the difficulties of the course, the torpedo was steered without trouble through the space separating the boats which represented the target, and after passing them was caused to turn round and return to the spot where the examining committee, among whom were Woods Bey, and Frost, Hassan and Hobart Pachas, was stationed. In the Lay torpedo the steering is effected by electricity transmitted through a cable, carried in the body of the torpedo, and paid out as it runs. Thus the line does not require to be dragged along, and forms no hindrance either to the speed or the manipulation of the projectile. The course is followed by means of two small sight rods, which project above the surface of the water, and can be seen for a mile or so by aid of a good glass. These are the only parts of the apparatus that are visible when the torpedo is in motion. At rest it projects about an inch above the surface, but immediately it starts it buries itself completely, and if the sight rods be lost it is difficult to again find them. At night the rods carry lamps that direct the light backwards, and are invisible to the enemy. The torpedo experimented upon is not of the latest pattern; it is a cigar-shaped boat 26 ft. long, and 24 in. in diameter at the largest part, and weighs, when fully prepared for action, with 90 lb. of dynamite, one and a half tons. In the more recent examples the speed has been increased to 12½ knots, and the disturbance of the water lessened by the use of twin propellers, while the change of explosive has been augmented to 150 lb. The results of the trial were so satisfactory that a contract was prepared between the Ottoman Government and Messrs. Lay and Nordenfelt. At the last moment, however, this fell through, owing to the request of the United States minister, that no decision should be come to until the Berden torpedo could be tried also. It is claimed for this latter that it will break through the steel wire netting that is used in the English Navy, and which is believed both here and in Turkey to offer a good defence to both the Whitehead and the Lay torpedoes.

A THERMOSTAT CURRENT METER.—An ingenious adaptation of M. Breguet's well-known metallic thermometer has been made by M. Dubois, mining engineer. It consists of a fine spiral compound wire of platinum and zinc, suspended in a vertical direction and dipping at its lower end into mercury. Midway there is also a connection between the spiral and a mercury cup enclosing it, formed by two arms branching out from the spiral and dipping into the mercury. The upper half of the spiral is kinked in one direction and the lower half in the other, to prevent changes of atmospheric temperature from altering the zero of the instrument by acting on the Breguet spiral. The current to be measured is sent through the lower half and heats it by overcoming the resistance of the compound wire. This rise of temperature causes the wire to turn, and being fitted with an indicator and scale of the deflection of the thermostatic coil is read off. Theory shows it to be strictly proportional to the strength of current.

A NEW IMITATION IVORY.—One of the disadvantages of celluloid is the fact that it burns very readily when a flame is applied,

but a new compound, said to be fireproof, and suitable as a substitute for ivory, is thus made: A solution is prepared of 200 parts of casein in 50 parts of ammonia and 400 of water, or 150 parts of albumen in 400 of water. To the solution the following are added: quicklime, 240 parts; acetate of alumina, 150 parts; alum 50 parts; sulphate of lime, 1,200 parts; oil, 100 parts. The oil is to be mixed in the last. When dark objects are to be made, from 75 to 100 parts of tannin are substituted for the acetate of alumina. When the mixture has been well kneaded together, and made into a smooth paste, it is passed through rollers to form plates of the desired shape. These are dried and pressed into metallic moulds previously heated, or they may be reduced to a very fine powder which is introduced into heated moulds and submitted to strong pressure. The objects are afterwards dipped into the following bath: water, 100 parts, white glue, 6 parts; phosphoric acid, 10 parts. Finally, they are dried, polished, and varnished with shellac.

EXPERIMENTS have been recently made by M. Allard with regard to the range of sound from various instruments: a large and a small bell, a cornet with compressed air, a steam-whistle, a vibrator-trumpet, and a siren-trumpet. His figures show that the intensity of sound decreases in air much more quickly than according to the law of the square of the distances. This is attributed to the reflecting and dispersing power of the air (not being homogeneous). Apart from the influ nce of wind, a given sound may have, it was proved, very different ranges, varying, *e.g.*, from two nautical miles to 15 or 20; the acoustic transparence, doubtless, varies within certain limits. The work of production of sound grows rapidly for small augmentations of range. The differences of range within an octave (supposing the work done in producing the sound exactly the same) are hardly perceptible.

AS an explanation of the effect produced by a thin stratum of oil spread over the surface of the sea in quieting the waves, M. Van der Mensbrugghe, in a paper read before the Academie des Sciéncés, premises, first, that to increase the surface of a mass of water a certain amount of force is required, and this force is stored up, as potential energy, in the superficial layer of the water; secondly, when the free surface of a mass of water is decreased, a proportionate amount of this potential energy is changed into kinetic or actual energy. Thus, when one stratum of water is brought—say by the wind—over another, the potential energy of this latter is changed into kinetic energy, and a certain velocity is generated. When, however, one stratum of water is brought upon another covered with a thin layer of oil—and consequently, having less potential energy than the first—the amount of force transformed into kinetic is considerably less than that remaining as potential energy. In other words, there would be a continual disappearance of actual force, and this would explain the tendency of the waves to subside much more quickly than when no oil is present.

THE following statistics of the number of inhabitants of some of the principal cities in Europe have been recently issued. There are 92, cities in the whole of Europe each containing a population of more than 100,000, but only four which have more than a million, viz.: London, 3,832,440; Paris, 2,225,910; Berlin, 1,222,500; Vienna, 1,103,110. Of the other capitals, St. Petersburg possesses 876,570; Constantinople, 600,000; Madrid, 367,280; Buda-Pesth, 360,580; Warsaw, 339,340; Amsterdam, 317,010; Rome, 300,470; Lisbon, 246,340; Palmero, 244,990; Copenhagen, 234,850; Munich, 230,020; Bucharest, 221,800; Dresden, 220,820; Stockholm, 168,770; Brussels, 161,820; Venice, 132,830; Stutgardt, 117,300. In addition to these, Moscow contains 611,970; Naples, 493,110; Hamburg, 410,120; Lyons, 372,890; Marseilles, 357,530; Milan, 321,840; Breslau, 272,910; Turin, 251,830; Bordeaux, 220,960; Barcelona, 215,960; Odessa, 193,510; Elberfeld, 189,480; Genoa, 179,510; Lille, 177,940; Florence, 169,000; Riga, 168,840; Prague, 162,520; Antwerp, 150,650; Adrianople, 150,000: Leipsic, 149,080; Rotterdam, 148,000; Cologne, 144,770; Magdeburg, 137,130; Frankfort, 136,820; Toulouse, 136,630; Ghent, 127,650; Messina, 126,500; Hanover, 122,840; Nantes, 121,960; Liege, 115,850; The Hague, 113,460; Oporto, 105,840; and Rouen, 104,010.

IT is known that the vapor tension of liquid carbonic acid is enormous; thus while it is about 50 atmospheres at 15 deg. C., it exceeds 100 atmospheres at 50 deg. and it reaches 800 atmospheres at 200 deg. Hence, a vessel of the liquid represents a certian quantity of energy ready for use—if the cooling due to vaporization do not lower the temperature of the liquid too much. This fact has been lately turned to account by Major Witte, head of the Berlin Fire Brigade. The steam pumps are supplied with reservoirs of liquid carbonic acid. When a fire is announced the boiler fire is at once lit, but it takes some minutes to get up the requisite steam pressure, and the engine may have reached the scene before this is done In that case communication is opened between the reservoir of liquid carbonic acid and the motor cylinder of the pump, and the vapor then drives the pistons like steam. As the temperature rises water is vaporized, and for a time the pump is driven by a mixture of steam and carbonic acid; then steam is used alone. The important point is that by this arrangement, the pump is always ready to act when the fire is reached. A gain of five or six minutes, thus sometimes realized, may be of considerable importance at the outbreak of a fire. Major Witte's experiments prove that the consumption of carbonic acid, before working with steam alone, does not exceed eight kilogrammes—say 20 lb.—but two receivers should be used, because the cooling effect of vaporization of part of the acid causes the rest of the liquid to freeze. At the Krupp works it has been recently stated, according to the *Times*, liquid carbonic acid is utilized not only in the manufacture of compressed steel, but for production of ice and of seltzer water, also to give the pressure necessary for delivery of beer.

New Method of Generating Electricity.—A discovery how to generate electricity by the act of combustion has been made by Dr. Brand of La Rochelle. This discovery, which is yet only in its infancy, will probably lead to many developments. Dr. Brand has a electro-generative torch or candle, which yields a current of electricity in the act of burning. It is prepared by making a paste of coal dust and molasses, and moulding it into a stick, which serves as the inflammable wick of a candle. This rod is then covered with asbestos in a thin sheet, and dipped into fused nitrate of potash until a good thick coating of the nitrate adheres. The wick being ignited, it burns away, and a curreut of electricity is drawn from the candle by wires inserted into the nitrate and the coaly wick. Though this current is comparatively feeble and not as yet of much practical value, the discovery is important as showing the possibility of electro-generative fuels. It is pointed out that, if we had a fireplace so constructed that on burning any ordinary fuel in it so as to give heat, it would, at the same time, develop an electro current sufficient to ring electric bells or charge an accumulator, and thus give light also. Dr. Brand is understood to have this aim in view, and his researches are based on the discovery of Becquerel, the great French physicist, who found that red hot carbon plunged into nitrate of potash forms an electric battery.

Fluid Carbonic Acid for Extinguishing Fires.—An apparatus, devised by W. Raydt, for this purpose consists of an iron cylinder filled with fluid carbonic acid and a large vessel filled with water, which is placed in connection with the iron cylinder in such a way that the carbonic acid shall stream through the water when the apparatus is to be used. Carbonic acid, as is well known, possesses the property of becoming liquid under a pressure of about 40 atmospheres, and occupies then about $\frac{1}{450}$th of its bulk in gas. A receiver containing 100 liters, therefore, will hold carbonic acid which, as gas, would occupy 45,000 liters. When used a valve reduces the pressure of the gas to the requisite amount, and all that is necessary is to screw on the conducting pipes, which will then convey to any desired height the water which, in this case, is saturated with carbonic acid, and which more readily and quickly extinguishes fire than does ordinary water. The first great experiment with the Raydt Extinctive apparatus has been made by the Krupp's Fire Brigade, in Essen. The Director and Chief of this fire brigade say that the main advantage of Raydt's method consists in the fact that when dealing with a fire which has broken out, a stream of water can at once be thrown without any preparation whatever, and the stream provided is one of an extreme activity for extinguishing fire. It is said to require little personal attention, and is readily handled. It is intended for use in theaters, factories, and such kind of establishments as are especially exposed to danger from fire, as also for use on board ships. They are of opinion that in many larger theatres, factories, and fires on board ships, by a ready employment of the Raydt Extinctive apparatus, all damage may be avoided. The Berlin Fire Brigade has also recently made an experiment with the Raydt's apparatus, and the Director in that city pronounces himself very well contented with the result. When setting it in order, the adjustment of the jet which is driven by the fluid acid is made once and for all, and when there is no fire the carbonic acid remains unchanged under pressure in the wrought iron cylinder. The fabrication of the fluid carbonic acid is now carried on on a large scale at Krupp's steel factory. F. Krupp, Jr., has found a use for it in the preparation of cast steel, and for this as other purposes the carbonic acid is prepared at Essen in the liquid form. The pump is so arranged that it can yield daily about 500 kilogs. of the fluid acid. The transport of the latter by rail, in wrought iron cylinders or bottles, is freely carried on. Each bottle before use is submitted to a pressure to test it amounting to one of two hundred and fifty atmospheres, while the gas itself only exerts a pressure of about fifty atmospheres.

An improvement in the manufacture of pulp for cardboard or the like has been suggested by an English paper-maker. The object is to produce cardboard, paper, paper-maché, and the like, which shall be both luminous and damp-proof. The *Journal* of the Society of Chemical industry says it consists in adding to the pulp phosphorescent powder for giving the luminous property, and gelatine for rendering the material damp-proof. The proportions preferred are as follows:—Water, 10 parts; paper-pulp, 40 parts; phosphorescent power—by preference slacked for 24 hours—20 parts; gelatine, 1 part; saturated solution of bichromate of potash, 1 part.

According to *Der Techniker*, the hotel "Zur Stadt Paris," in Breslau, is now lit throughout by Herr Sandberger with gas obtained from human fœces. These are put in a retort, where they are not only dried but decomposed by heat, the chief products being a light yielding gas, carbonic acid, tar, oil, and ammonia. As in ordinary gas works, the tar and oil are separated, the gases washed by being passed through water, the carbonic acid fixed, and the light yielding gas purified for use. There remain in the retorts the ash-constituents with a portion of carbon, which the inventor designates coke.

Iron in contact with lead, as in the case of railing bars fixed in the rails by means of lead, might remain unharmed if kept quite dry, or if thoroughly protected by a well maintained coating of point. Paint is, however, seldom renewed until the action of air and rain has deteriorated so that corrosion of the iron takes place. Once moisture has access to the iron and lead, the otherwise almost inactive couple formed by the metals becomes active and corrosion proceeds rapidly, especially in atmospheres containing sulphur. Hence the remarkable decay of many of the old railings to be seen in London, some of which, with heavy bars, may be seen corroded to mere threads.

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VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXIII.—MAY, 1883.—VOL. XXVIII.

RECENT HYDRAULIC EXPERIMENTS.

By Major ALLAN CUNNINGHAM, R.E., Fell. of King's Coll., London.

Minutes of Proceedings of the Institution of Civil Engineers.

II.

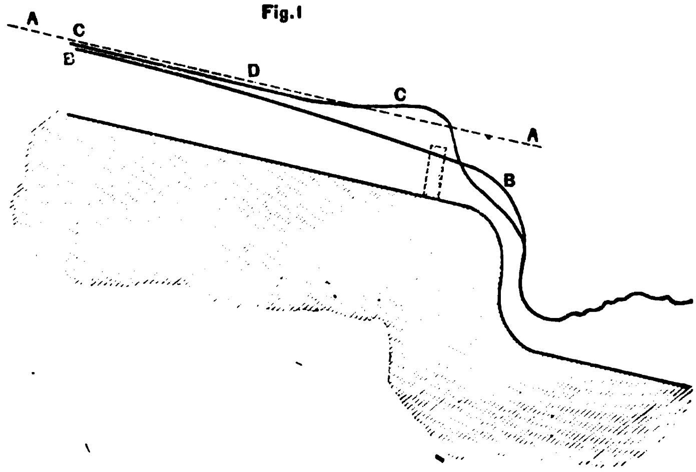
Discussion.

Major Allan Cunningham desired to add that the experiments, of which an account had been read in abstract, were very extensive, the complete report of them occupying three large volumes. It had been difficult to prepare a condensed report of the work which should give a fair account of it, and at the same time not be tedious to the meeting. The subject of the flow of water was an important one, both scientifically and financially. It had been stated that the crops which were saved in a single season of drought by irrigation from the Ganges canal, on which the experiments were made, yielded a revenue which covered the entire cost of the construction of the canal—a revenue which otherwise would have been entirely lost. The subject was a dry one; but Figs. 6 and 7 showed two average curves which might be taken as a summing-up of the whole of the experiments.

Professor W. C. Unwin fully acknowledged the credit due to the author for the labor he had expended on his experimental research, and for the high standard of accuracy and carefulness which he had aimed at throughout. There was no point of detail to which he had not paid attention, or in which any one could say that he had been careless in his observations. He hoped that in speaking on two or three points, in regard to which he differed somewhat from the author's conclusions, it would not be thought that he differed in regard to the principal points that had been worked out, or with Major Cunningham's general conclusions. He should like first to call attention to one point in the experiments, which he thought explained why it was that the author's measurements of discharge did not throw any great light on the relative value of the different formulas in use. The Ganges canal, as originally constructed, was in reaches of uniform slope, with a sudden drop at the end of the reach. It seemed to have been supposed by the constructors of the canal that the water would flow as it did in an ordinary stream of uniform depth, as at AA, Fig. 1, and that there would be a sudden drop at the fall. But it was well known that a sudden drop in a stream bed produced a form of water surface such as BB. The effect of an oversight, as it appeared to him, as to the influence of the sudden drop in the bed of the falls, was that the velocity in the canal was greater than was expected. At all events, the scouring action was greater,

and that was corrected by building at the falls temporary weirs, which backed up the water and gave the water surface a shape like CC. At some of the sites where the largest number of experiments were made, Major Cunningham had placed himself at the point D of the canal, where obviously the surface-slope was most variable. The case was still further complicated by the fact that, in regulating the discharge of the canal, temporary obstructions were placed at the falls, and that, in low-water conditions of the canal, the obstruction at the fall was higher than the bed of the canal at the site of the gaugings. From one point of view that was extremely interesting. The author had shown that the in calculating discharges, by any formula which involved the use of the surface-slope. Gaugings were made over a length of 50 feet only, and it was still further uncertain whether the slope over 50-feet length was anything like the slope over 2,000-feet length. It seemed to him that the measurement of the surface-slope over a great length, when the gauging was over a very short length, was not a satisfactory proceeding. If the surface-slope was measured by leveling operations, it was essential to measure over a considerable length. Of course, a leveling instrument was not accurate enough to measure the surface-slope in a short distance; but it did not seem impossible to adopt micrometric

Fig. I



discharge under those conditions bore no relation to the depth of water in the canal, and he had thrown light in various ways on the action of water in a reach of that kind. But Professor Unwin considered that a site of that kind was not suitable for independent observations to verify the formulas of discharge; and the confession in the paper that the measurement of the surface-slope was an exceedingly uncertain measurement, seemed to admit that the site of the gaugings was inconvenient for that particular purpose. It had been mentioned that the surface-slope had been measured in lengths of 4,000 feet and 2,000 feet, and that there was a difference of 25 per cent. Of course, that threw great doubt on what slope ought to be taken methods in measuring the slope in a very short distance. It would be possible to have a baulk of timber, say 40 feet in length, of sufficient weight and solidity to hold a tolerably stable position in the water, on which might be placed a level with micrometric screw, so as to measure the slope, say in a length of 40 feet, to $\frac{1}{1000}$ inch. In that case it would be possible to get the surface-slope precisely at the site of the gauging.

All the experiments had been made with floats, and some discredit had been thrown on the use of current-meters for gauging. He did not say that one was better than the other, because there were cases in which floats must be used and current-meters could not be used; but, as he had lately had a good deal of ex-

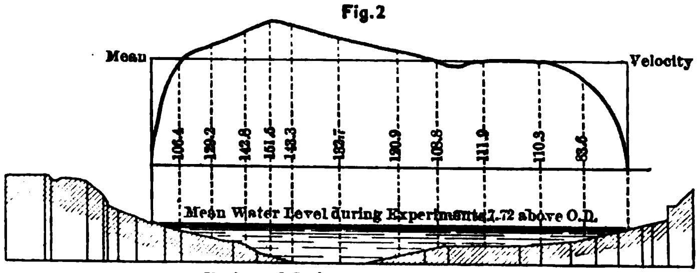
perience in the use of current-meters, he would say a few words as to the relative value of the two kinds of instruments. The author had enumerated several advantages of floats: 1st. "They interfere little with the natural motion of the water." That was true, and he would pass it by. 2d, "they measure velocity directly." It was true that they measured the average velocity over a more or less considerable length of stream, but not at the section point; he did not think that was of much importance. 3d, "they can be used in streams of any size." 4th, "they are not much affected by silt or floating weeds," which was quite true. 5th, "they measure forward-velocity," which was also true. 6th, "they can be made up and repaired by common workmen," which did not seem to be very important. And 7th, "they are very cheap." No doubt floats could be made very cheaply, but if, in the observation of velocity, they required two or three times the time which current-meters required, the instrument was not really a cheap one. In determining each velocity, the author had taken forty-eight float observations, over a length of 50 feet; in other words, he had practically observed the average velocity over a length of 2,500 feet. He should show presently that with a much shorter length of stream current-meters gave practically a constant velocity. There were certain disadvantages in the use of floats. In the first place, it was impossible to get rid entirely of the action of the wind on the exposed surface of the floats. Although surface-floats were, of all others, the least open to objection, it was just the surface-velocity which would be the last that would be observed if velocity could be recorded at all points of the section with equal facility. The moment it was attempted to get velocity below the surface by the use of floats, difficulties were encountered which were considerable. Using what appeared to be the best instrument, according to Major Cunningham, the sub-surface float, with a very light surface-float, it was impossible to get rid of the stream on the cord which connected the surface-float with the bottom-float, and that almost restricted the use of sub-surface floats, for anything like accurate measurements, to very moderate depths of water. When it was remembered that, in the Mississippi experiments, the cord connecting the surface and the bottom-float had an area one and a half time as great as the area of the sub-surface float, it would be seen that, neglecting the influence of the cord must, in that case, have introduced an enormous amount of error. On the other hand, the current-meter, supposing it to be rightly fixed, and of one of the forms which obviated the objections to engaging and disengaging gear, certainly enabled velocities to be taken with more rapidity. He took some velocities in the tidal part of the Thames in January, 1882, and on the average of five or six days' work, he obtained one good observation in about three minutes' work, a rate which he thought could not be approached in using floats. Then the velocity was obtained at the section at which it was wanted. Finally, it should be remembered that, with floats, unless the stream was comparatively narrow, there was the laborious operation of fixing the float-path by angular measure. There was one point with respect to which he thought there was a little misconception. Looking at the face of the current-meter, the blades of the screw appeared very large, and seemed as if they would interfere a good deal with the motion of the stream; but when the instrument was at work it was only the edges of the blades that met the stream, and he had a strong impression that the current-meter interfered very little with the natural action of the water, if only it was made with the right degree of delicacy. He had been using a current-meter which had a very convenient arrangement — not being fixed on any rod, but suspended by a wire. It was only open to one kind of objection, so far as he knew, that when the meter was suspended by a wire, it was uncertain whether it was exactly normal to the plane of the section, the mean velocity across which it was intended to measure; there was, therefore, a source of error in that kind of suspension, but it was as well to consider what magnitude that error was likely to assume. Suppose a meter to be fixed at 20° with the normal to the section the velocity measured would be erroneous by about 6 per cent. Now a meter suspended by a wire was not fixed at an angle with the plane of

section, but it swayed backwards and forwards through a more or less large angle. Supposing it to sway backwards and forwards, through an angle of 40°, the error of observation due to its position would not exceed 3 per cent. That, however, he believed was a much greater angle than any through which the meter swung, and the error did not seem to be a large one; so that, at all events for many purposes, a meter suspended by a wire was an extremely convenient form of current-meter. With respect to the unsteadiness of the motion of water in a stream, he had used a meter during the last flood in the Thames in this way; he took a continuous note of the time of each hundred revolutions of the meter, and he had plotted in the three curves shown on the diagram (Fig. 8) the results of the observations taken in that way at depths of 0.5, 3, and 6 meters from the surface. Taking every hundred revolutions, there was an exceedingly irregular curve. The oscillations of the velocity during periods of twelve seconds for the two upper curves, and twenty seconds for the lower curve, were very large. But by simply averaging the result for five hundred revolutions of the meter instead of one hundred, there was an exceedingly regular curve. There were eleven or twelve successive periods during which the meter made five hundred revolutions. The velocity at the end of each interval was calculated from the five hundred revolutions, and a curve was obtained which approached closely to a straight line, and he was not sure that the irregularity which existed was not due to an imperfection in observing the time. He hoped to be able to repeat the observations with an electric chronograph, which would eliminate all error of that kind. One point in the paper had not been alluded to—a point on which he entirely differed from the author. There was a well-known phenomenon of flowing streams that the position of the line of maximum velocity was considerably below the surface, often at about one-third of the depth at the middle of the stream, more or less towards the sides. The explanation of that depression of the line of maximum velocity had been considerably discussed. The explanation adopted by the author was, that the air opposed a resisting surface against which the water rubbed. The retardation due to the friction of the water against the air explained, he thought, the reduction of surface-velocity and, therefore, the depression of the line of maximum velocity. Although Professor Unwin did not deny that the air retarded the motion of the water, the explanation appeared to him to be an altogether inadequate one. The only cause which seemed to him sufficient was the mixing with the surface water of water stilled by contact with the bed, and brought up to the surface. There was more than one way in which that occurred. Eddying masses of water produced against the roughness of the bed were shot off and mingled with the stream, and were liable to accumulate at the surface because it was a boundary of the section. Probably, in addition to that, even in a straight length of stream, a curvilinear motion of the water in spirals brought up the bottom water towards the surface; and furthermore at every bend in the river there was demonstrably a rotation of the water in the plane of the transverse section. Amongst the most interesting results mentioned were the author's attempts to find some rapid way of approximating to the mean velocities in the section of the stream; and he had given two or three rules for finding the mean velocites of a stream, from observations made at two depths at the center of the stream, or by one observation with a peculiar float which had a small surface float and two equal sub-surface floats. In finding that means of rapid approximation the author had proceeded entirely in one direction; he had tried to find the mean velocity from observations at two different depths. But Professor Unwin thought the mean velocity might be more easily found by observation at two positions in the horizontal width. To test this he had worked out as a rough trial two considerable sets of the author's observations made with rod-floats, and he had found that if single observations had been taken with a rod-float at very nearly one-third of the breadth of the stream from the center, he would have got approximately the mean velocity of the stream, and much more accurately if he had made two observations with a rod-float at one-third the distance from

the center on each side of the center line. He should like to mention one point which it appeared those who measured the flow of the water had been a little too apt to neglect. So far as he knew there were no observations on flowing streams in which the temperature of the had been observed. He had found in experiments made in another way that, at the temperature of the atmosphere, about 60°, a very few degrees difference in temperature made a marked and measurable difference in the fluid friction; and he thought that in future it would be useful if experimenters would record the temperature of the water at the time their observations were made. He believed that some noticeable discrepancies in the results might perhaps be explained by observing the temperature. He had instant, to get enough observations to calculate the discharge of the stream. The flow over Teddington Weir in the flood while the observations were being made was just under 71 cubic meters per second, or 2,500 feet per second.

Mr. Baldwin Latham exhibited a diagram, Fig. 2, showing the result of a number of gaugings which had recently been made in the River Thames, immediately below Teddington Lock. The gaugings were all made about the period of low water, after the tidal water had run off, and therefore represented the flow of upland water of the Thames at that point. At the place where the gaugings were made the river was slightly curved, the concave side of the curve being on the Middlesex side of the river, and the convex side on the Surrey side.

Fig. 2



Horizontal Scale 80 Feet to 1 inch
Vertical " 40 " " " "

Surrey Middlesex

RIVER THAMES.

placed on the wall the results of two gaugings which he had made during the last year. One was a diagram of gaugings during the last Thames flood. He had drawn a curve of mean velocity which looked tolerably regular, and from it calculated the flood discharge. It was a little under 8,000 cubic feet per second—a result not very different from that which he had given to the Institution some years ago. The flood was six inches lower than that of 1875, and at the section which he had chosen this year for gauging a large quantity of water, 3,000 or 4,000 cubic feet possibly, was escaping over some miles of flooded area on the bank of the river. He exhibited some results of velocity observations at a section of the river at Putney, in order to show how far it was possible, in a tidal stream in which the surface slope and velocity varied from instant to The ordinates above the section of the river represented the relative velocities at the points in the section of the river over which they were placed. The horizontal line represented the mean velocity of the stream. It would be seen that at one point there was a depression in the curve, showing that the velocity there was less than on each side. This occurred over a shelving bank on the concave side of the river; while, on a similar shelving bank on the convex side, there was an abnormally high velocity. There could be little doubt that these peculiarities were due to the horizontal deflection of the stream; for although the diagram was based upon upwards of 60,000 feet run of the current meter, extending over numerous observation, the separate observations all more or less indicated this distinctive feature. With reference to the use of double-floats in gauging, no doubt

under some circumstances they might be useful, especially in sluggish streams; but in the generality of streams the current-meter was undoubtedly preferable. Where the maximum velocity was removed one-third of the total depth of the stream from the surface, and where the velocity was represented by the ordinate at any point in the vertical section of a parabola having its axis parallel with the surface of the stream. double-floats were not necessary, as the surface-velocity in that particular case would be equal to the mean velocity, and so a single surface-float would give the true velocity of the stream. Judging from his own observations, he should say that it was very rare indeed for the maximum velocity to be removed so far as one-third the total depth of the stream from the surface. Where such a condition did exist, it was no doubt due to the vertical deflection of the stream downwards, arising either from the channel being deeper at the place of observation than higher up the stream, or from water being tailed back over the point of gauging. This, as suggested by Professor Unwin, might have been the case at certain points in the canal which have been selected as gauging stations. The effect of the damming back was to flatten the inclination in the lower portion of the longitudinal section of the canal, and the water entering from the upper portion at an angle, naturally was projected downward, and so the maximum velocity was removed from the surface. It was important to determine the position of maximum velocity. Looking at Professor Unwin's diagrams it appeared that the mean velocity was not far from the center of the stream; but in most channels of trapezoidal cross-section, in which the larger volume of water was moving with a relatively higher velocity, the point of mean velocity in the stream would rise above half the depth. As in rivers and streams of pretty constant flow there were two points in every section where the water would be moving at the mean velocity, if these two points were ascertained a ready means would be available for easily gauging, by a current-meter, the quantity of water passing. He had made numerous gaugings in this way, but he took care at the same time to check the records by an instrument which delineated every change in the area of the section. Great precautions were needed in arriving at results based on calculations in which the use of coefficients, and the rate of inclination of the surface of the water, were the principal factors. It was well known that within certain limits the length of the channel affected the velocity of flow. In long channels of uniform grade, as compared with shorter channels there was an acceleration of flow; for example, the velocity of water in a 12-inch pipe, full or half full. having an inclination of 1 in 495, when the length was but 250 feet, would be 2 feet per second; but if the length were increased to 30,000 feet, the inclination remaining the same, the velocity would be $2\frac{1}{4}$ feet per second. He had found by actual measurement in the river Wandle that weeds attached to the bottom of the stream, and growing in the water-way of the channel, affected the flow of water to an enormous extent; for while the ordinary Eytelwein formula in the Chely form gave a coefficient of 93.4, in this case the coefficient, on an average of six carefully conducted experiments, fell to 40.23. This proved the enormous influence exercised by the growth of weeds in impeding the flow of water in a channel. With reference to the form of the curve at the surface of the water, there was no doubt that whenever a river was filling up, the surface of the water was convex, and that whenever the river was falling it was concave. If the stream remained constant there would be a pretty level surface. When a river was filled up two functions were in operation, the water was moving down the stream, and the channel was being filled. As water was moving down the stream the maximum flow was always in the deepest part, generally in the center of the stream. The consequence was that the water fell from the center towards the banks, but the reverse condition held when the stream was falling. In Mr. C. Ellet's work on "The Mississippi and Ohio Rivers," it was recorded (page 302) that whenever the latter river was rising the drift was thrown on the shores, clearly showing a current from the center outwards; but whenever the river was falling, the boatmen said they could travel for miles in the center of the stream without making a single sweep of the oar

to keep them in the current. This amounted to an actual demonstration of the curves. The author of the paper seemed to say that no results had been obtained from the observation upon silt. Mr. Latham presumed that that simply meant that a large quantity of silt was brought into the canal from day to day, and that the simple velocity of the stream itself had not much influence upon moving or creating the silt; but he wanted silt observations to be carried on from quite a different point of view. At Croydon there was a culvert 4 feet in diameter under the town. At certain periods it discharged beatifully clear spring chalk-water, at the rate of 3,000 to 4,000 cubic feet per minute. It was also connected with the street gullies of the town, and in time of rain discharged highly-colored water carrying a large amount of silt. He had been making careful observations on the culvert for many years, and he had found that whenever the water was clear the maximum velocity, with the same height and gradient, was always considerably higher than when the water was turbid; thus the mixture of silt with water had the effect of retarding its velocity. He considered that the retardation in this case was due to the extra load the water had to carry; and if the weight of water and weight of silt were taken and multiplied by the velocity, it would be found to tally pretty nearly with the weight of the water multiplied by its velocity when there was no silt. Thus by the addition of silt a diminution of velocity was effected, while in the absence of silt the velocity was greater. He contemplated making a series of observations with different percentages of material put into water, to ascertain to what extent the velocity of the water might diminish. However, it was obvious from observations already made, that silt, when present in a large quantity, did interfere with the velocity of flow. The author seemed much surprised that, with such a high temperature as 165° in the sun and 105° in the shade, there was only $\frac{1}{16}$ inch of evaporation per day from the surface water. For some years he had been carrying out experiments on evaporation. In former days it was considered that the evaporation in England greatly exceeded the rainfall, and there could be no doubt that the instrument used did show that such was the case: but common sense would show at once that if that were so there would be no streams. Mr. Charles Greaves, M. Inst., C.E., who had done so much to elucidate the question of evaporation, and other meteorological phenomena in which engineers were interested, had directed his attention to the views of Dalton on evaporation. These tended to show that evaporation was not due to temperature, but to the vapor-tension. Water at a particular temperature would give off vapor of a certain tension. The vapor in the air also had a certain tension, which was arrived at by the temperature of the dew point. So long as the tension of the vapor in the air was less than the tension of vapor due to the temperature of the water, water would pass into the air, but when the tension of the vapor in the air was greater than the tension of vapor due to the temperature of the water no evaporation would take place. Although the author of the paper recorded no experiments with the wet and the dry bulb thermometer, it was probable that at 165° and 105° the atmospheric vapor-tension would be in excess of the the tension of vapor of water at 66°. No one could expect to blow steam from a steam-boiler under 10 lbs. pressure into a boiler which was under 20 lbs. pressure: but it would be easy to pass the steam from a boiler under 20 lbs. pressure into a boiler under 10 lbs. pressure. That was exactly what was found in evaporation. The month in which the largest amount of evaporation had occurred this year was May, yet the temperature of May was much below that of July. He would point out a source of serious error in all the experiments on evaporation which he had tested. He had adopted as the standard form of evaporator the form introduced by Mr. Greaves, namely, a floating evaporator, but he originally started with one similar to that described by the author. He had a vessel floating in water. It was supported by an outside ring, which came above the water, but he found that when the sun in hot weather shone upon the evaporator, as the water always ran up the side of the vessel by capillary action, great evaporation occurred. The error in all such vessels was no doubt due to the film of water that ran up the

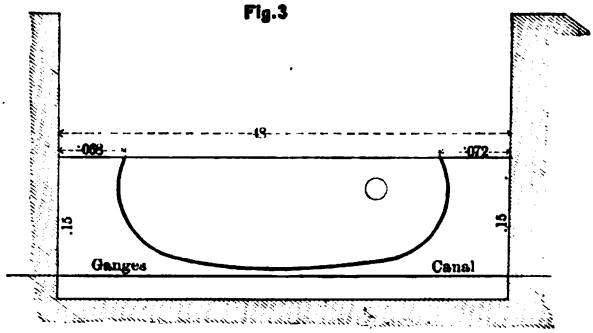
sides, and which under great heat was speedily evaporated. He therefore discarded that form of evaporator, and suspended the evaporating vessel from a floating ring like a life-buoy, and at once there was a large reduction in the amount of evaporation. The film of water now extended up the sides of the vessel both on the outside as well as the inside, and so they were kept cool. He, however, carried the experiment still further on a number of evaporators painted different colors. He observed that from a painted gauge, even if black, there was a less amount of evaporation than from a plain copper gauge. That was because capillary action was more energetic in the metal gauge than in the painted gauges. In order to test the matter, he took three evaporators made of copper, each 5 inches in diameter, and holding at least 1 foot depth of water. One of these he had slightly greased inside, and that and another copper evaporator were allowed to stand in a tank of water immersed to within 2½ inches of the top, while at the same time the third evaporator was freely exposed to the atmosphere. The evaporation from these gauges in the month of May, 1882, when compared with that of the floating gauge, 12 inches in diameter, and a gauge painted white, but immersed in water, were—

Floating gauge.	Gauge in air.	Gauge in water.	Gauge greased in wat'r.	Gauge painted white in wat'r.
Inches.	Inches.	Inches.	Inches.	Inches.
3.665	6.355	5.495	4.085	4.175

In the case of the gauge greased regularly, the coating was so slight that no oily matter ever appeared on the surface of the water. The size of the instrument had a marked influence on the amount of water evaporated, for the larger the vessel, in proportion to the area, the smaller was the marginal ring up which the water passed by capillarity, and from which it was evaporated.

Mr. F. Newman, in reference to the author's contention that Bazin's two coefficients (β, C) were not reliable, said he ought at the same time to have stated that the conditions under which the formulas were obtained, which he now brought before the Institution, were very different from those of Darcy and Bazin. The average width of the section of canal was 180 feet to 200 feet, and the depth in the center was only about 9 feet or 10 feet. The ratios of the depth to the width in the experiments made by the author were as one to twenty, while those of Darcy and Bazin were much less. In Fig. 3 was represented exactly the proportion that the depth bore to the width in the author's experiments. Attempts to reach the coefficient for velocity in a section in this must differ very much from one in the other on account of the greater excess of the wetted perimeter in the case of the canal. The section of the channel, as shown by the author, did not fairly represent the canal as it was actually constructed. That would represent a depth of 20 feet. With regard to the mean velocity the curve shown was Darcy's; if a horizontal line were drawn, which unfortunately he had not shown on the diagram, at one half the depth of the water, and a point were taken on the surface one-third of the half width from the border, and a verti-

Fig. 3

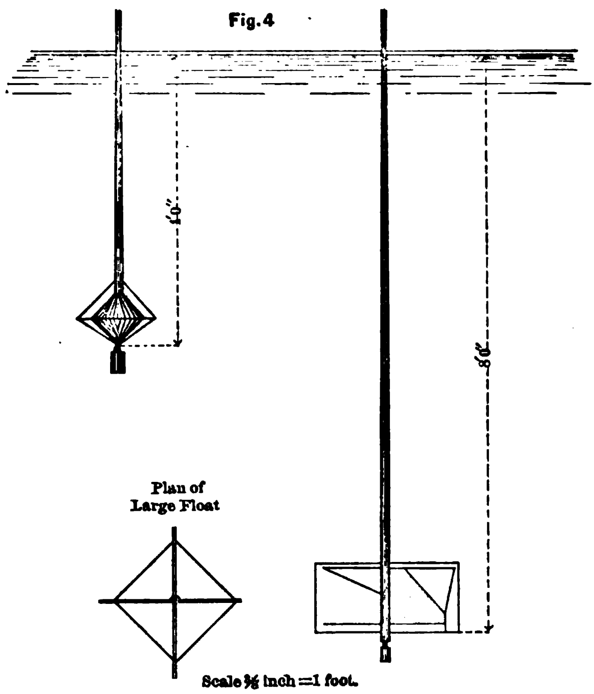


cal line drawn from that point, the intersection of those two lines would be a point of mean velocity. This nearly coincided with what was stated by Professor Unwin; in fact, the point 3.5 or 3.6 on the surface would represent the mean velocity as stated by him. The author also objected to certain formulas formed by Professor Moseley, and by Darcy and Bazin, and also to some experiments by Darcy on pipes; but those on the pipes had been remarkably correct, and they had been verified at Glasgow. Mr. J. M. Gale, M. Inst. C. E., gave the figures, the area of the pipe, and the rate of declivity, to the late Professor Rankine, who worked them out, and the slight difference between the coefficient of friction, as deduced by Gale on 4-feet pipes, and that given by Darcy amounted to less than one-thousandth per cent. That spoke well for the accuracy of Darcy and Bazin's experiments. Still, he thought that the author's remark to which he had referred should not have been made, because Bazin's coefficients were not applicable to a case of such enormous width in comparison with the area.

Mr. L. F. Vernon-Harcourt said that observers of discharge had in most cases adopted either the double float or the current-meter, and had generally considered that one or the other was exclusively the best. For instance, in Humphrey and Abbot's experiments on the Mississippi the double-float was adopted to the exclusion of the current-meter. Mr. Révy, on the contrary, held that the double-float was perfectly useless, and used current-meters alone for measuring the discharge of the Parana and La Plata rivers. Then, again, Mr. Gordon, in his experiments on the Irrawaddy, preferred the double-float, and the author had followed his example and rejected current-meters as possessing insurmountable difficulties in application, whereas Professor Unwin had carried out experiments satisfactorily with a current-meter. These facts pointed to the conclusion, which he had found in practice to be correct, that both instruments were suitable for measuring the discharge, and it was necessary to find out which was best adapted to the particular case. Of course, sometimes the double-float was the best, as, for instance, where the stream was sluggish and the channel fairly regular; but where the channel was irregular, where there were weeds—not merely floating along the stream, but growing in it—and where the bottom was uneven, the current-meter was far preferable. But there was another objection to the double-float, which the author had indeed mentioned, but passed over without the notice it deserved. namely, the effect of wind on the surface of water. According to the author, the primary effect of wind appeared to be the production of wave-motion, that it caused translation of the water only when long continued, and that, were it not so, every wind would produce a current on a lake or at sea. Of course, it was well known that long-continued winds did produce currents, as the trade winds created currents in the Atlantic Ocean. There were also currents on the Suez Canal, at one time in one direction, and at another time in another. These were produced by the prevalent winds in the Mediterranean and the Red Sea. Perhaps these would come under what the author would term winds of long duration; but if a strong wind was blowing up the Thames for even a short time it affected the tide. The author also said that the velocity at the surface of a stream was affected by the resistance of the air. If, then, a long-continued wind could produce a current, and, moreover, the resistance merely of the air affected the velocity of a stream, wind, even for a short time, must have some effect. This was brought to his notice in some float experiments he made in 1875 on the Firth of Clyde. In that case he had to measure how far the ebbing current would take down any floating material. He had a float made, which was especially designed to provide against the action of wind. The experiments were conducted at the best time of the year, in June and July, when there was no great amount of wind. The float was a tin tube above, and lower down it was brought out in shape of a cone, hollow inside. To make it float upright he had a weight attached underneath. It was about 4 feet long, and after a time he added some wings, with the object of offering a greater area to the tide where he thought that the wind would have no effect; but on a day when there was only a moderate amount of wind, blowing upstream, he found that instead of going down with the ebb tide,

it traveled in the opposite direction, though it reached about 4 feet below the surface (Fig. 4). At the same time an orange traveled up against the tide very much quicker. He concluded that the wind evidently did affect water at the depth of 4 feet, and therefore he had another float made, consisting of a tin tube, carried down about 8 feet. At the bottom there were four tin plates arranged radially to the tube. He braced these with a little tin piping so as to keep them secure (Fig. 4). He had only to add a very small weight of lead to keep however, any opposing wind sprang up, the motion of the floats was reversed, the deep float taking the foremost place, and the orange coming last in order. He had often wished that he had the means and opportunity of making experiments of this kind on the surface of a lake where there was no natural current, and where the effect of wind upon the surface of the water might be tried. This was an important subject, and one that had not received due consideration. He was certain it must materially affect the experiments with double floats.

Fig. 4



Scale ⅜ inch = 1 foot.

it floating vertically in the stream. This float traveled down with the ebb when there was a strong wind blowing upstream, and when, of course, an orange would have been practically worthless. During the course of his experiments on the Firth of Clyde, he, several times, on calm days, started his two floats (Fig. 4) and an orange, as a surface-float, together, and observed that, whilst the calm continued, the three floats traveled fairly together, the surface-float generally going rather in advance, and the deep float falling somewhat behind. Directly,

The reason advanced by Professor Unwin for the surface-velocity being less than the velocity some distance below, was that when there was an irregular bottom, the water striking against the shoals was checked, and being forced upwards imparted its loss of velocity to the surface. He could not accept this, because it seemed to him that the lowest layers of water, being checked, must impart their loss of velocity to the lower portions first. He did not see any reason why the lowest stratum of water should arrest the motion at the surface

more than it did those layers nearer the bottom.

The author evidently thought that the only formulas that could be satisfactorily got from his experiments were those in which the mean velocity could be expressed in terms of maximum velocity; but, unfortunately, a formula of this kind, though very useful on rivers and canals for determining the discharge, would be quite useless in determining what the discharge would be after a channel had been enlarged or upon a new canal. Therefore, in such a case as that some kind of formula was required other than one which merely dealt with the mean velocity. He quite admitted that an experiment in an existing stream, from which the mean velocity was got, was much better than anything computed from a mere formula; but he would be very glad if the author could, by means of the valuable experiments he had made, deduce some formula that would give the discharge in terms of the slope, because that was the only way in which a formula could be useful for rivers that had been enlarged, or for new canals.

Mr. B. T. MOORE said the author stated in his paper that the main object of all his hydraulic experiments was the determination of the discharge of large canals and rivers. The quantity of water which passed down a stream in a given time was equal in volume to the solid contained between a transverse section of the stream, a portion of the bed, a portion of the free surface, and a certain surface extending from the free surface to the bed and from bank to bank. This last surface was of a curved form, and generally convex at every point down the stream, whether it was supposed to be cut by horizontal or by vertical planes. The only practical way of measuring a quantity of that sort was by obtaining a sufficient number of parallel sections made by planes perpendicular to the transverse section, and equidistant from each other. When the areas of these sections, and their common distance were known, the prismoidal or other formulas of approximation would give the area to any required degree of exactness. This was the method he had always used when gauging rivers, and it was also the method which the author had adopted. But the areas of the parallel sections could only be found by means of velocity-measurement, and hence it was that the velocity-measurement was such an important matter. The author took a number of velocities at short distances from the surface towards the bottom, (generally about 1 foot) from which he plotted a curve, and found its area, making allowance for that portion of the curve which was below the lowest velocity and above the bed of the stream. But the author himself soon found that that was a very tedious and troublesome business; and, moreover, as it was of extreme importance that all velocities should be taken as nearly as possible at the same time, it became necessary to find some quicker means of doing the work. He was thus driven to seek some formula which would give the mean velocity at any vertical from two observations of velocity, one above and one below the half depth. Now, how did he arrive at that formula? He assumed that the form of the vertical velocity-curve was a portion of a parabola with its axis horizontal. If that assumption were made there was no necessity for a single practical experiment. By a simple mathematical transformation an equation could be obtained which would give the quantity sought. That equation was the following:

$$4(3\theta^2-3\theta+1)U = V_{\theta h} + 3(2\theta-1)^2 V_{h\frac{3\theta-2}{3(2\theta-1)}} \quad . \quad \text{(A)}$$

in which h was the depth of water, and θ a fraction which might have any value not exceeding $\frac{1}{2}$; U was the mean velocity, and the two measured velocities were represented by the letter V, one being taken at the depth θh below the surface, and the other at the depth

$$\frac{h}{3} \cdot \frac{3\theta-2}{2\theta-1}.$$

If $\theta=0$, equation (A) became

$$4\,U = V_0 + 3V_{\frac{2}{3}h} \quad . \quad . \quad . \quad \text{(B)}$$

This was the principal formula mentioned by the author, and gave the mean velocity in terms of the surface velocity, and the velocity at two-thirds of the depth.

Again, if the quantity $3(2\theta-1)^2=1$, so as to make the coefficients of the two

measured velocities the same, the resulting equation was

$$2U = V_{0.211h} + V_{0.789h} \quad . \quad . \quad . \quad (C)$$

This was the last formula at which the author said he had arrived, his object in seeking it being to obtain a formula which should be applicable to a double submerged float, and should give the mean velocity from one observation. This last formula was not convenient for rapid work in the field, where the depth at which a velocity had to be taken has so often to be calculated mentally, and it was of no particular value for any other instrument except the double submerged float.

Another very convenient formula might be obtained from the general formula (A) by giving θ the value $\frac{1}{12}$. This formula was

$$37\ U = 12\ V_{\frac{1}{12}h} + 25\ V_{\frac{7}{10}h} \quad . \quad . \quad . \quad . \quad (D)$$

and an approximate form of it, good enough in many observations, was

$$3U = V_{\frac{1}{12}h} + 2V_{\frac{7}{10}h}.$$

The quantities $\frac{1}{12}h$ and $\frac{7}{10}h$ were easily calculated mentally, and they were the only quantities required to be known during the actual measurement of the velocities.

The formulas (B), (C) and (D) could all be used with current-meters as well as with floats, but the last was the most convenient, as it avoided surface velocities, and was very easy of application. But it was not every observer, or every experimenter, who accepted the theory that the velocity-curve was a parabola with its axis horizontal. On the contrary, some of the best experiments tended to show that the curve was better represented by a parabola, with its axis vertical, and its vertex in the bed, at least as far up as the maximum velocity. If that was assumed to be the case, it would result in an entirely different formula. It was quite possible that the two formulas might give the same, or nearly the same, mean velocity. Considering that there were two rival theories as to the form of the velocity-curve, and that they gave two totally different formulas, he thought great caution should be observed in adopting any formula depending on two measured velocities, and that it was safer to trust to a formula based on three velocities, which would always give the area of the velocity-curve to a close degree of approximation whatever the form of the curve might be.

Fig. 5 would render the subject clearer. The curve shown was a parabola with its axis horizontal and focus at F. Whatever the true form of the velocity-curve might be, it was always curved in one direction, and was generally much flatter than the curve shown. Now as the radius of curvature of a parabola varied from half the latus-rectum to infinity, and as the latus-rectum might have any value, a parabola could always be found which should pass through three points of the velocity-curve, and should coincide with it very closely throughout. The area of this parabola was easily calculated from three known velocities, and it could only differ very slightly from the area of the true velocity-curve. In Fig. 5 the three lines marked V_s, V_m, and V_h represented the three measured velocities, V_s being taken at the small distance δ below the surface, V_h at the same distance above the bed, and V_m at half the depth.

The area between the velocity-station and the curve was then given by the formula—

$$\text{Area} = \frac{h-2\delta}{6}\left\{V_s + V_h + 4V_m\right\} + V_s\,\delta + V_h\,\delta \quad . \quad . \quad . \quad (1)$$

if the two exceedingly small triangles at a and c were neglected. But in practice it was seldom necessary to take into account the small distance δ. It was generally sufficiently accurate to consider V_s the velocity at the surface, and V_h the velocity at the bottom. The formula for the area thus becomes—

$$\text{Area} = \frac{h}{6}\left\{V_s + V_h + 4V_m\right\} \quad . \quad . \quad . \quad (2)$$

The difference between (1) and (2) was

$$\frac{2}{3}\delta(V_s + V_h - 2V_m).$$

Now as δ was a small quantity, and as the velocity at mid-depth never differed

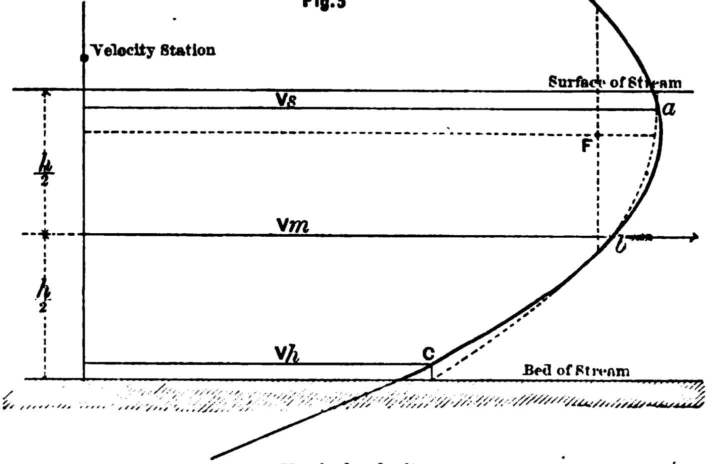
much from the arithmetic mean of the velocities at the surface and bottom, the error made by using formula (2) in place of formula (1) was always very small.*

The same conclusion would follow from an inspection of Fig. 5, in which the dotted line represented the curve resulting from the supposition that V_s and V_h were the surface and bottom velocities. It would be seen at once that the area bounded by the dotted curve was too small above the half depth and too large below it, and that on the whole it could only differ from the area in question very slightly. In general the velocity-curve was much flatter than the curve drawn, which was purposely much curved for the sake of giving distinctness to all parts of the figure.

So far as he had spoken only of the use which was to be made of these three velocities and the parallel sections, but now he would consider how these velocities were to be obtained, for that was a most important thing. The author put his faith entirely in floats, not having a good word to say for any current-meter.

It was not his intention to discuss all the objections that were raised to current-meters, but he would make this remark, that current-meters were indispensable and floats were not. Some years ago he had measured the discharge of the Nile. He had to do that at low Nile, which took place about the 21st of June. All the velocities had to be taken in three days, because the river remained pretty well constant during that time but no longer. Now, even at low Nile the best site which he could select was 1,600 feet or 1,700 in width, with a mean depth of over 10 feet. There were four sections extending to $\frac{1}{4}$ mile, nearly equi-distant from each other. The method he adopted was to measure the discharge through each of those sections, and compare them together as checks upon each other. There were sixteen principal velocity-stations in each section. That was the smallest number with which he could operate on such a large scale. That meant two hundred velocities, and all these two hundred velocities had to be taken within three days. Egypt, in June, was not a country in which one could work late in the day. All the work had to be done before ten o'clock in the morning, after which time the heat of the sun put a stop to field work. He would ask how the work could have been done by floats? As to taking velocities at depths where a number of observations had to be taken to get one velocity, it would have been

Fig. 5



Vertical velocity-curve.

* In a velocity-curve taken on the Thames the following numbers were found:

V_s =160.4 feet per minute.
V_m=149.6 " "
V_h =135.9 " "

and δ was 6 inches.

$\therefore$ Difference above$=\frac{2}{3}\times\frac{1}{2}(160.4 + 135{,}9 - 2\times 149.6)=$ 1.27 square feet, the whole area being 244 square feet.

simply out of the question. That was an instance where current-meters were absolutely essential if work had to be done in a given time. The author had stated that floats were cheap, and current-meters expensive. But were floats cheap? If they were used, a number of skilled observers were necessary, and such observers were by no means cheap. The expense incurred in this way would soon pay for a considerable number of current-meters. One person could use three current-meters at the same time without the slightest difficulty. He could put one near the bottom and one near the surface, and a third in the middle, and get all the observations at the same time. A great saving of time was thereby secured, and the velocities were taken together, which was also an important point. He was speaking now of using the current-meter by means of suspension. Any contrivance for fixing the current-meters in position was a cause of great difficulty and delay. The author objected to current-meters in suspension, and spoke of the uncertainty of orientation. But Professor Unwin had pointed out that there was not much in that objection about orientation, and that if a current-meter was set at 20° from the direction of the current the error in its registration would not exceed 6 per cent. of the velocity, and if allowed to swing 20° on each side of the true direction the error would not be more than 3 per cent. Mr. Moore had used suspended current-meters very freely, but had never seen one swing 20° from the normal on each side; 10° would be an ample allowance to make. But with an allowance of 10° there was little to be said on account of this defect in orientation, because the error would not amount to more than ¾ per cent. The author also objected to current-meters being suspended, because there might be an error in position as regarded depth; but if the stream was a slow one, the suspending line was very nearly vertical, and there was no question of error in the position of the meter. If the stream was rapid, of course the line, even when the current-meter was loaded, would be drawn back a little from the vertical, but in that case there was a compensation. The variation of velocity from point to point vertically was very small compared with the velocity, when the velocity was considerable, and therefore it was not of so much importance whether the meter was a little above or below the level where it was intended to be. In his book the author showed a current-meter fixed to an apparatus attached to two pontoons; the current-meter was deprived of its tail. and was held in a position of restraint parallel to what the author supposed to be the axis of the stream; but if the current-meter was allowed to hang freely, it would find out the axis of the stream for itself, much better than could be done from the surface. The author raised several other objections to current-meters, which he could not accept, but he would not discuss them, because he had already taken up too much time. In his opinion the best way to obtain the mean velocity, and from it the area of a vertical velocity-curve was to use a current-meter which admitted of being lowered from the surface to the bottom with tolerably uniform velocity, and also of being drawn up again with uniform velocity. It was not necessary that the velocity going down should be the same as the velocity coming up. The whole reading, divided by the time, gave the mean velocity; nothing could be simpler. The mean velocity was got by one reading, and the elaborate contrivance of the electrical bell and apparatus was got rid of. He did not like those arrangements, because perhaps just at the very critical moment something might go wrong with the insulation, or the battery might not work, and so the current-meter would be rendered useless, or would give incorrect results.

Mr. E. A. Cowper wished to make one or two remarks in reference to the question raised in the paper on the instability of the floating gauges. Balls 1⅝ inch in diameter, were spoken of, weighing but little more than the water displaced. They were kept from sinking by a thread and small float at the top. It seemed to him that it was not necessary to have such a small float. He would prefer to have two boards 1 foot square put across each other for the lower float, or at all events a large surface on which the water could act. But the point to which he wished to draw attention was this, that if the water on the surface went faster than the water below, it drew the string aside horizontally, and lifted the lower gauging board higher than it ought to be, as

pointed out in the paper. If, on the contrary, there were three threads hanging from three points at a distance apart on the top float, the lower float would always be at the proper depth from the level of the water, unless the current was so strong as to slack the threads altogether. It seemed to him to be a simple form of float, ensuring the exact depth of the lower float below the surface of the water. He wished to ask what price was charged for the water for irrigation? He understood that the experiments were all for the purpose of finding out what quantity of water to charge for, but the price was not stated, nor how the water was measured and sold to the natives.

Sir Frederick Bramwell said three points had been touched upon by the previous speakers, about which he wished to say a few words. One was as to the effect of wind on the surface of the water—whether it was merely skin deep, or was effective in moving the water along. Recently he was at Niagara, and when crossing below the Falls in the ferryboat, he asked the boatmen whether there was much variation in the height of the river. Their answer was that it depended not so much upon the season as on the direction of the wind. He crossed on several occasions, and one day the boatman pointed out to him that the water in the river below the Falls had risen 12 inches, entirely in consequence of a change of wind acting on the water above. That was good evidence that the direction of the wind either retarded or augmented the flow of the water. He therefore thought it must be regarded as effective far below the surface. Mr. Baldwin Latham objected to the possibility of there being a greater evaporation than was equal to the rainfall. It appeared to him, however, that all the rivers on the face of the earth must be derived from the excess of evaporation from the sea over the rainfall on the sea. The last speaker had suggested that if a current-meter had not got its tail, and was not allowed to follow the direction of the stream for itself, it would give an inaccurate result; but was that so? Was it not desirable that the current-meter should be fixed in a line in the axis of the stream, if it was required to know, not what was the maximum flow of water in some one direction at that particular point, but what was the flow of water in the direction of the river? It was quite clear that there were eddies all through the river. If there were not, the flowing river would deposit the suspended matters, just as they were deposited in a quiescent lake. What was wanted, therefore, was not that the current-meter should seek out for itself the point of strongest current, and should sway backwards and forwards as the current varied, but that the meter, always pointing in one direction, should give the velocity in that direction from time to time, because that when multiplied into the cross section would give the true velocity, and not the velocity that would be attained if the meter were at liberty to wander into the line of greatest current at the time being.

Mr. J. B. Redman directed attention to the well-known treatise by the late Major Rennell on ocean currents traced by him to the winds. Amongst others a local one at the back of the Goodwin was quoted, which increased in intensity after a continuance of S. W. gales. The recent abnormal addition to the tidal column of the Thames, an increase of 5 feet, or 25 per cent. as regarded altitude, due almost entirely to gales of wind, must necessarily have been accompanied by a corresponding increase of velocity.

Mr. Baldwin Latham said that the amount of evaporation from the sea was equal to the amount of rain falling on the sea and the volume of water carried by streams from the land into the sea. It was obvious that, taking equal areas, the depth of evaporation from the sea was less than the depth of the rainfall on the land, as the quantity of water carried by rivers and streams into the sea from the land was less than the rain which fell on the land. The excess of depth of rainfall on the land arose from the smaller area of land compared to that of sea, evaporation also taking place from the land as well as from the sea which vapor, on condensation, fell again on the land in the form of rain or dew.

Major Allen Cunningham observed, in reply to criticisms as to the suitability of the Ganges Canal for experiments on the flow of water, in consequence of its being laid out in reaches closed by obstructed falls, first, that irrigation canals in hot climates were of such enormous importance, both financially and in the interests

of humanity, where the very existence of immense populations sometimes depended on them in seasons of drought, that the conditions of the flow of water in them, even if peculiar, were of more public importance than those of flow of water in channels wholly natural. But the condition in question, of obstruction at the tail of each reach, was in no way peculiar; it was common to most fresh-water canals and also to most rivers of small size in highly civilized countries.

A more important objection had been raised by Professor Unwin as to the position of the two sites, at which most of the experiments were made, as being within the influence of the obstruction at the tail of the reach. The reach was 9½ miles long, and the sites in question were 5¼ and 4¾ miles from the obstruction; the fall of the bed was 11.5, 6.1, and 4.9 feet in those lengths, whilst the raised crest of the falls at the tail of the reach was 5.1 feet high, or 0.2 feet above the bed of the lower site; but there were also means of raising the crest-level temporarily about 4 feet more, or over 4 feet above the bed of the lower site. This might, no doubt, be an objection to the position of both sites; but apart from that they were exceptionally favorable for experiment, from their situation in a straight 3 mile length of channel with regular masonry banks, and advantage which could not have been secured elsewhere. The power of varying the obstruction at the lower site was the source of, perhaps, the most important feature of the experiments, namely, the great range of conditions and of consequent results; for example, surface-slope 480 to 24 per million; velocity, 7.7 feet to 0.6 per second, &c., which were obtained at a single site, and in some cases without change of depth at the site. In no other large experiments had this great range been approached, nor could it be approached probably without artificial regulation; whilst without a wide range it was not likely that general laws of fluid motion would not be discovered, nor a single formula be properly tested.

It was suggested that surface-slope should, if possible, be deduced from the surface-fall in the same length, say 50 feet, as the run through which the floats were timed. The "slope-length" should certainly be the shortest practicable, but it would be impossible to measure the fall of the free surface in a 50 feet length in any ordinary stream. The oscillations of the free surface, often ½ inch, and the awkward position of the body and eye, necessarily above that surface, precluded determining the minute quantity in question, say ⅛ inch in 50 feet, with accuracy. The introduction, as proposed, of a baulk of timber 50 feet long into the running water for carrying micrometers would ruffle the surface. The only hopeful way of doing it appeared to be to lead the water from two fine nozzles 50 feet apart, similar, and similarly placed into two still-water boxes the difference of water-level in which could be accurately read with a differential hook-gauge or differential Pitot's tube; but no doubt the result would be affected by any want of similarity in the nozzles or in their position.

The objection to floats, that though they were cheap, their use was expensive from the frequent repetition, and therefore great length of time required to determine properly an "average velocity," was undoubtedly an important one. If it was true that an "average velocity" could be determined, as apparently suggested by Professor Unwin, in three minutes with a current-meter, then the current-meter should, if certain improvements could be effected in it, supersede the use of floats in suitable channels. The drawbacks to the current-meter were discussed at length in Chapter XXIII. of the Roorkee experiments. The objection that floats, even when deeply submerged, were seriously affected by wind, was contrary to most recorded experience. The instance given by Mr. Vernon Harcourt of a large sub-float with 4 feet immersion being driven by a high wind against tide was extraordinary; the ascribing this effect to the wind was contrary to all the Roorkee experience. Out of many hundred cases of upstream wind, not a single instance was noticed of even a surface-float (much less of a sunken float) being driven upstream by the wind; but the instance in question was in a tidal channel; in such a channel there were often counter subcurrents at certain states of the tide, so that this instance was not certain evidence of wind effect.

The double-float proposed by Mr.

Cowper had two great defects; the large triangular surface-float was much too large, and the three-cord connector exposed much more resistance to the current than a single cord of the same strengh would; these were the two worst faults in a double-float.

The statement that in the Mississippi experiments the double-float was so badly designed that its connector exposed one and a half time the area of the sub-float to the current depended probably on the description given in the original Mississippi report, in which the connector was said to have been $\frac{8}{10}$ inch thick; this has since been reported to be a misprint for $\frac{1}{10}$ inch, which reduced the ratio to 8:10.

Attention had been drawn by Mr. Baldwin Latham to some properties of transverse velocity curves in his own experiments, viz. (1) the maximum velocity being over the deepest channel; (2) the existence of two maxima on each side of a central shallow; (3) a depression in the curve near a shoulder in the bed; all these points were obvious in the Roorkee experiments. (Vol. I., pp. 257-260, and Pl. xxxv., xxxvi.) The statement of the maximum and mean velocity past a vertical being at definite depths, viz. at $\frac{1}{3}$ and at $\frac{2}{3}$ of the full depth did not agree with experiment; the maximum velocity might lie anywhere between the surface and mid-depth, and the mean velocity line varied in position with it, but the curve was so flat that the velocity at $\frac{2}{3}$ depth was, except near the banks, an approximation to the mean. The great variability of the coefficient (C) in the Chezy formula ($V = C \times 100 \sqrt{RS}$) was the very point towards which most of the Darcy Bazin and Roorkee experiments were directed.

The accuracy of the Darcy Bazin experiments on which so much stress had been laid had never been questioned. The suggestion of Mr. Newman that the failure of their coefficients when applied to the Roorkee results was due to the disparity of proportions of the Darcy Bazin canals and the Ganges canal was very likely correct, and amounted to an admission of the want of generality of those coefficients, as urged in the paper.

One main result of the Roorkee experiments was that approximation to mean velocity was more likely to be attained by direct velocity-measurement than by surface-slope measurement; but it could not be fairly said that scarcely any light was thrown on the use of formulas involving the latter. Much special experiment was done with this very aim, and with the definite result that Kutter's formula was the only one (not requiring velocity-measurement) of pretty general applicability, and would under favorable conditions give results differing by not more than $7\frac{1}{2}$ per cent. from actual velocity-measurements. This was surely a definite and important result.

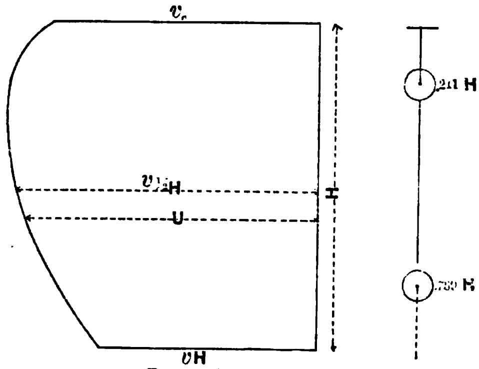
The process used by Mr. Moore for discharge-computation was substantially that used in the Roorkee experiments. The velocity-formulas given by him were mostly those of, or easily deducible from that work (pp. 211–213, vol. I). The spacing at 0.211 and 0.789 of the full depth, Fig. 6, approximately at $\frac{2}{10}$ H and $\frac{8}{10}$ H, which were simple fractions, had great advantages. It gave the best general approximation possible, with only two ordinates for any curve whatever. The two velocities could be measured at one operation, which saved time, with a special double-float. When the two velocities were separately measured, the result was at once found as the arithmetic mean, which saves computation. Next, it was hardly fair to describe the taking the vertical curve to be a parabola with horizontal axis as a mere assumption. The curve was most likely not a parabola, but the amount of evidence that it did approximate very nearly to a parabola with horizontal axis, sufficiently for computing approximations, was now very great (chap. xi., vol. I). The parabola was by no means so accommodating a curve as represented. Thus, assuming a horizontal axis (Fig. 6), it could only be fitted to three points, whilst forty-five of the Roorkee curves had more than three, and ten of them have ten measured ordinates. This amounted to a severe test. On the other hand, the placing the axis vertical was certainly wrong, being incompatible with a maximum velocity-line below the surface, a condition the evidence of which was overwhelming.

The only evidence adduced by Mr. Latham as to the convexity or concavity of a river when rising or falling was very indirect; the observed facts might well be due to other causes; for instance,

the set of driftwood towards the banks might be due to wind or to surface-currents.

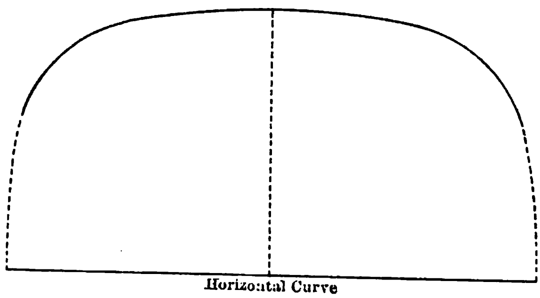
As to evaporation, dryness of the air was probably the most important exciting cause. In northern India the dryness of the air during the hot winds was excessive, the difference between dry and wet bulb thermometers being often 40°; so that evaporation must then be active. The smallness of the evaporation from the canal at Roorkee seemed to be due to the coldness of the water. The use of oil inside an evapometer would be hardly safe, as a little oil would probably escape and form a surface skin over the water, and so reduce the evaporation.

Fig.6

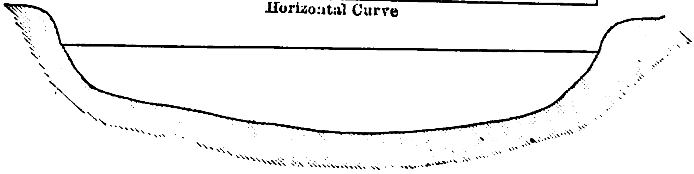


Vertical Curve

Fig.7



Horizontal Curve



CROSS SECTION OF CHANNEL.

At a recent meeting of the Academie des Sciences, M. Bertrand reported that he had been present with M. Du Moncel at experiments which appeared to absolutely confirm the correctness of the law formulated by M. Marcel Deprez, viz.: (1) The intensity of an electric current remaining the same, whatever be the speed of the motor, the static effort does not change; and (2) in a machine worked by a current, the speed may be doubled, quadrupled, or decupled, without the intensity of the current varying. M. Du Moncel added that during the experiments the resistance of the circuit had been varied without changing the intensity of the current.

UNDERGROUND TEMPERATURE.

From "Nature."

II.

E. We now Proceed to a Comparison of Results.

The localities at which definite results have been obtained may thus be classified:

1. Metallic mines. 2. Coal mines. 3. Wells and wet borings. 4. Tunnels.

1. The mines at Przibram in Bohemia, with a depth or 1900 feet, are in very quartzose rock, and give a very slow rate of increase, viz. 1° F. in 135 feet. As all the shafts are in lofty hills, an allowance of $\frac{1}{15}$ may be made for convexity, leaving 1° F. in 126 feet. Quartz is found by Prof. Herschel to have a conductivity of about .0086.

The mines at Schemnitz in Hungary, with a depth of 1368 feet, give an average rate of 1° F. in 74 feet, the rock being a green hornblende-andesite (in German, *Grunstein-Trachyt*), which is a compact fine-grained, crystalline, more or less vitreous rock. Prof. Lebour estimates its conductivity as being probably nearly the same as that of Calton Hill trap-rock, which Prof. Herschel found to be about .0029.

2. The principal results from coal mines are as follows:

The mines of the Société Cocqueril at Seraing (Belgium), with a depth of 1657 feet, give an average rate of 1° F. in 50 feet. The rock is coal shale. Prof. Herschel found for shale the low conductivity .0019.

The mines of Anzin, in the north of France, with a depth of 658 feet, gave in the deepest shaft an increase of 1° in 47 feet.

Rosebridge Colliery, near Wigan, with a depth of 2445 feet, gave a mean rate of 1° in 54 feet.

The four following are in the East Manchester coalfield.

Astley Pit, Dukinfield, with a depth of 2700 feet, gave a mean rate of 1° in 72 ft.

Ashton Moss Colliery, with a depth of 2790 feet, gave 1° in 77 feet.

Bredbury Colliery, with a depth of 1020 feet, gave 1° in 78.5 feet.

Nook Pit, with a depth of 1050 feet, gave 1° in 79 feet.

South Hetton Colliery, Durham, with a depth of 1929 feet, including a bore hole at bottom, gives very consistent observations at various depths, and an average rate of 1° in 57.5 feet.

Boldon Colliery, between Newcastle and Sunderland, with a depth of 1514 feet, and excellent conditions of observation, gives an average rate of 1° in 49 feet.

Kingswood Colliery, near Bristol, with a depth of 1769 feet, and remarkable consistency between observations at various points, gives 1° in 68 feet.

Prof. Phillips' observations in Monkwearmouth Colliery, published in *Phil. Mag.* for December 1834, showed a temperature of 71.2 in a hole bored in the floor of a recently exposed part at the depth of 1584 feet. The surface of the ground is 87 feet above high water, and the mean temperature of the air is assumed by Prof. Phillips to be 47.6. If, as usual, we add 1° to get the soil temperature, instead of assuming, as Prof. Phillips does, that the temperature 100 feet deep is identical with the air temperature at the surface, we obtain a rate of increase of 1° in 70 feet.

3. The following are the most trustworthy results from wells and borings:—

The Sperenberg bore, near Berlin, in rock salt, with a depth of 3492 English feet, to the deepest reliable observation, gave an average of 1° in 51.5 feet. This result is entitled to special weight, not only on account of the great depth, but also on account of the powerful means employed to exclude convection.

Rock salt, according to Prof. Herschel, has the very high conductivity .0113.

Three artesian wells in the chalk of the Paris Basin gave the following results:—

	Feet.	Rate, Feet.
St. Andre, depth of observation.	830	1 in 56.4
Grenelle	1812	1 in 56.9
Military School	568	1 in 56.2

An artesian well at St. Petersburgh, in the Lower Silurian strata, with a depth of 656 feet, gave about 1° in 44 feet.

A well sunk at Yakoutsk, in Siberia, to the depth of 540 feet, disclosed the fact that the ground was permanently frozen to this depth, and probably to the depth of 700 feet. The rate of increase of temperature was 1° in 52 feet.

Of the English wells in which observations have been taken the most important is that at Kentish Town, in which Mr. G. J. Symons, F.R.S., has taken observations to the depth of 1100 feet. The temperatures at different depths form a smooth series, and have been confirmed by observations repeated at long intervals. The only question that can arise as to the accuracy of the results is the possibility of their being affected by convection.

The well is 8 feet in diameter, with brickwork to the depth of 540 feet, and this part of it is traversed by an iron tube 8 inches in diameter, which is continued to the depth of more than 1300 feet from the surface. The tube is choked with mud to the depth of about 1080 feet, so that the deepest observations were taken under 20 feet of mud. The temperature at 1100 feet was 69°.9, and by combining this with the surface temperature of 49°.9 observed at the Botanic Gardens, Regent's Park, we obtain a rate of 1° in 55 feet. These data would give at 250 feet feet a calculated temperature of 54.5, whereas the temperature actually observed at this depth was 56.1, or 1°.6 higher; the temperature of 300 feet and at 350 feet being also 56.1. This seems to indicate convection, but it can be accounted for by convection in the 8-foot well which surrounds the tube, and does not imply convection currents within the tube. Convection currents are much more easily formed in water columns of large diameter than in small ones, and the 20 feet of mud at the bottom give some security against convection at the deepest point of observation. It is important to remark that the increase from 1050 to 1100 feet is rather less than the average instead of being decidedly greater, as it would be if there were convection above, but not in the mud. The rate of 1° in 55 feet may therefore be adopted as correct.

The strata consist of tertiary strata, chalk (586 feet thick), upper greensand, and gault.

The Kentish Town temperature at the depth of 400 feet (58°) is confirmed by observations in Mr. Sich's well at Chiswick, which is 395 feet deep, and has a temperature varying from 58° to 57°.5.

The Bootle well, belonging to the Liverpool Waterworks, is 1302 feet deep, and the observations were taken in it during the sinking. The diameter of the bore is 24 inches, and convection might have been suspected but for the circumstance that there was a gradual upward flow of water from the bottom, which escaped from the upper part of the well by percolation to an underground reservoir near at hand. This would check the tendency to downflow of colder water from the top; and as the observations of temperature were always made at the bottom, they would thus be protected against convective disturbance.

The temperature at 226 feet was 52°, at 750 feet 56°, at 1302 feet 59°, giving by comparison of the first and last of these a mean rate of 1° in 154 feet. The circumstance that the boring ceased for six weeks at the depth of 1004 feet, and the temperature fell during this interval from 58°.1 to 57°.0, would seem to indicate an elevation of 1° due to the heat generated by the boring tool. An assumed surface temperature of 49° (only 0°.9 lower than that of the Botanic Gardens in London) would give by comparison with 57° at 1004 feet, a rate of 1° in 125½ feet, and by comparison with 59°, at 1302 feet, a rate of 1° in 130 feet, which last may be adopted as the best determination. The rock consists of the pebble beds of the Bunter or lower Trias, and the boring was executed at the rate of nearly 100 feet per month.

The boring at Swinderby, near Scarle (Lincoln), in search of coal, was carried to a depth of 2000 feet, with a diameter at the lower part of only 3½ inches—a circumstance favorable to accuracy, both as impeding convection and as promoting the rapid escape of the heat of boring. The temperature at the bottom was 79°, the water having been undisturbed for a month, and this by comparison with an assumed surface temperature of 50° gives a rate of 1° in 69 feet.

The rocks are Lower Lias, New Red Marl, (569 feet thick), New Red Sand-

stone (790 feet thick), Red Marl, and earthy Limestone.

The following results have been obtained from shallow borings. The first three were made under Sir William Thomson's direction, with a thermometer which could be read by estimation to hundredths of a degree:

Blythswood bore, near Glasgow, with a depth of 347 feet, gave a very regular increase of 1° in 50 feet.

Kirkland Neuk bore, in the immediate vicinity of the above, gave consistent observations at different seasons of the year from 180 feet to the bottom (354 feet), the rate being 1° in 53 feet. This bore passed through coal which had been "very much burned or charred."

South Balgray bore, near Glasgow, and north of the Clyde, with an available depth of 525 feet, gave by comparing the temperature at the bottom with that at 60 feet a rate of 1° in 41 feet.

Shale extends continuously from 390 to 450 feet from the surface, and the increase in these 60 feet of shale was 2°.02, which is at the rate of 1° in 30 feet. This rapid increase agrees with the fact that shale has very low conductivity, averaging .0019 in Prof. Herschel's experiments.

The only small bore remaining to be mentioned is that at Manegaon, in India, which had 310 feet available, and gave by comparing the temperature at this depth with that at 60 feet a rate of 1° in 68 feet. The rocks consist of fine softish sandstones and hard silty clays, the dip being 10°.

4. *Tunnels.*—The Mont Cenis Tunnel, which is about seven miles long, is at a depth of exactly a mile (5280 feet) beneath the creast of Mont Fréjus overhead. This was the warmest part of the tunnel, and had a temperature of 85°.1 F. The mean air temperature at the crest overhead was calculated by the engineer of tunnel, M. Giordano, by interpolating between the known temperature of the hill of San Theodule and that of the city of Turin, the former being 430 meters higher, and the latter 2650 meters lower than the point in question. It is thus calculated — 2°.6 C. or 27°.3 F. If, according to our usual rule, we assume the ground to be 1° warmer than the air, we have 28°.3 to compare with 85°.1. This gives a rate of 1° in 93 feet; but, inasmuch as the convexity of the surface increases the distance between the isotherms, a correction will be necessary before we can fairly compare this result with rates under level ground. As a rough estimate we may take $\frac{6}{8}$ of 93, and adopt 1° in 79 feet, as the corrected result.

"The rocks on which the observations have been made are absolutely the same, geologically and otherwise, from the entrance to the tunnel on the Italian side for a distance of nearly 10,000 yards. They are not faulted to any extent, though highly inclined, contorted, and subjected to slight slips and slides. They consist, to a very large extent indeed, of silicates, chiefly of alumina, and the small quantity of lime they contain is a crystalline carbonate."

The St. Gothard Tunnel, which has a length of about nine miles, has been subjected to much more minute observation, a skilled geologist, Dr. Stapff having, under Government direction, devoted his whole time to investigating its geology and physics. He not only observed the temperature of the rock in the tunnel at very numerous points, but also determined, by observations of springs, the mean temperatures of the surface of the mountain at various points, and compared these with an empirical formula for air temperature deduced from the known mean temperatures of the air at Göschenen, Andermatt, Airolo, and the Hospice of St. Bernard. He infers from his comparisons a considerable excess of soil above air temperature, increasing from 2° C. at the ends of the tunnel to 6° C. at the crest of the mountain over the center of the tunnel. The highest temperature of the rocks in the tunnel was at this central part, and was above 30°.6 C. or 87° F. The soil temperature at the crest above it was about—0°.6 C., or 31° F., giving a difference of 56° F. The height of the crest above sea level was about 2850 m., and that of the tunnel at this part 1150 m., giving a difference of 1700 m. or 5578 feet. The rate of increase here is, therefore, about 1° F. in 100 feet; and if we apply the same correction for convexity as in the case of the Mont Cenis Tunnel, this will be reduced to about 1° F. in 87 feet, as the equivalent rate under a level surface. From combining his observation in all parts of the tunnel through the medium of empirical formulæ, Dr. Stapff deduces an average

rate of 1° F. for every 88 feet measured from the surface directly overhead. Where the surface is a steep ridge, the increase was less rapid than this average; where the surface was a valley or plain, the increase was more rapid. As this average merely applies to the actual temperatures the application of a correction for the general convexity of the surface would give a more rapid rate. If we bring the isotherms nearer by one part in 15, which seems a fair assumption, we shall obtain a rate of 1° F. in 82 feet.

Collecting together all the results which appear reliable, and arranging them mainly in the order of their rates of increase, but also with some reference to locality, we have the following list:

		Depth feet.	Feet for 1° F.
Bootle waterworks (Liverpool)....		1392	130
Przibram mines (Bohemia).......		1900	126
St. Gothard tunnel..............		5578	82
Mont Cenis tunnel................		5280	79
Talargoch lead mine (Flint).....		1041	80
Nook Pit Colliery	East Manchester coalfield	1050	79
Bredbury "	East Manchester coalfield	1020	78½
Ashton Moss "	East Manchester coalfield	2790	77
Denton "	East Manchester coalfield	1817	77
Astley Pit, Dukinfield...........	East Manchester coalfield	2700	72
Schemnitz mines (Hungary)......		1368	74
Scarle boring (Lincoln)..........		2000	69
Manegaon boring (India)..........		310	68
Pontypridd colliery (S. Wales)....		855	76
Kingswood colliery (Bristol)......		1769	68
Radstock " (Bath)........		620	62
Paris artesian well, Grenelle......		1312	57
" " St. Andre....		830	56
" " Military Sch'l.		568	56
London " Kentish Town		1100	55
Rosebridge colliery (Wigan)......		2445	54
Yakoutsk, frozen ground (Siberia)		540	52
Sperenberg, boring in salt (Berlin)		3492	51½
Seraing collieries (Belgium).......		1657	50
Monkwearm'th collieries (Durham)		1584	70
South Hetton " " ...		1929	57¼
Boldon " " ...		1514	49
Whitehaven " Cumberl'd.		1250	45
Kirkland Neuk bore (Glasgow)...		354	53
Blythswood " "		347	50
South Balgray " "		525	41
Anzin collieries (North of France).		658	47
St. Petersburg, well (Russia).....		656	44
Carrickfergus, shaft of salt mine (Ireland)......................		770	43
Carrickfergus, shaft of salt mine (Ireland)......................		570	40
Slitt mine, Weardale (Northumberland)......................		660	34

The depth stated is in each case that of the deepest observation that has been utilized.

F. In Deducing a Mean from these very Various Results, it is better to operate not upon the number of feet per degree, but upon its reciprocal—the increase of temperature per foot. Assigning to the results in the foregoing list weights proportional to the depths, the mean increase of temperature per foot is found to be .01563, or about $\frac{1}{64}$ of a degree per foot—that is. 1° F. in 64 feet.

It would be more just to assign greater weight to those single results which represent a large district or an extensive group of mines, especially where the data are known to be very accurate. Doubling the weights above assigned to Przibram, St. Gothard, Mont Cenis, Schemnitz, Kentish Town, Rosebridge, and Seraing, and quadrupling that assigned to Sperenberg, no material difference is made in the result. The mean still comes out 1° F. in 64 feet, or more exactly .01565 of a degree per foot.

This is a slower rate than has been generally assumed, but it has been fairly deduced from the evidence contained in the Committee's Reports; and there is no reason to throw doubt on the results in the upper portion of the above list more than on those in its lower portion. Any error that can reasonably be attributed to the data used in the calculations for the St. Gothard Tunnel and for the numerous deep mines of the East Manchester coalfield, will have only a trifling effect on the rates of increase assigned to these localities.

To obtain an approximation to the rate at which heat escapes annually from the earth, we will first reduce the above rate of increase .01566 to Centigrade degrees per centimeter of depth. For this purpose we must multiply by .0182, giving .000285.

To calculate the rate of escape of heat, this must be multiplied by the conductivity.

The most certain determinations yet made of the conductivity of a portion of the earth's substance are those deduced by Sir William Thomson by an indirect method, involving observations of underground thermometers at three stations at Edinburgh, combined with laboratory measurement at the specific heats and densities of the rocks in which the thermometers were planted. The specific heats were determined by Regnault, and

the densities by Forbes. Specific heats and densities can be determined with great accuracy in the laboratory, but the direct determination of conductivity in the laboratory is exceedingly difficult, it being almost impossible to avoid sources of error which make the conductivity appear less than it really is.

Prof. Herschel, in conjunction with a Committee of the British Association, has made a very extensive and valuable series of direct measurements of the conductivities of a great variety of rocks, and has given additional certainty to his results by selecting as two of the subjects of his experiments the Calton Hill Trap and Craigleith sandstone, to which Sir William Thomson's determinations apply.

From combining Prof. Herschel's determinations with those of Sir William Thomson, .0058 is adopted as the mean conductivity of the outer crust of the earth, which, being multiplied by the mean rate of increase, .000285, gives

$$16330 \times 10_{-10}$$

as the flow of heat in a second across a square centimeter. Multiplying by the number of seconds in a year, which is approximately 31½ millions, we have

$$1633 \times 315 \times 10_{.4} = 41.4.$$

This, then, is our estimate of the average number of gramme degrees of heat that escape annually through each square centimeter of a horizontal section of the earth's substance.

AMERICAN PRACTICE IN WARMING BUILDINGS BY STEAM.

By the Late ROBERT BRIGGS, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

I.

The application of steam to the warming of buildings in the United States originated with the late Mr. Joseph Nason, of Boston and New York, who died six or seven years ago. He was not only the first to make the attempt, but was also the originator, improver, and adapter of much that is essential, and now implicitly followed, in the general arrangement and details of the apparatus employed. He enjoyed the advantage of having been for a short time a pupil of Jacob Perkins in London, about 1840; and his earliest endeavor in America was to adapt the Perkins system of hot water inside small tubes for meeting the severity of the climate of America. The large extent of warming surface and the great strength presented by steam apparatus constructed of small and comparatively inexpensive wrought iron tubes, and the facility thereby afforded for transmitting heat in any direction from a central source, are merits which led to so rapid a development of this system of warming, that by 1860, or in less than twenty years, there were already many hundred establishments throughout America for the manufacture of the apparatus. With the maturing of the system are associated the names of Mr. J. J. Walworth, of Boston, brother-in-law and partner of Mr. Nason; Mr. Gregg, of New York; Mr. J. O. Morse, of New York, by whom, amongst other improvements, was introduced the method of closed circulation for working with steam below atmospheric pressure; Professor Mapes, of New York, who supplied a reliable steam trap; Mr. Miles Greenwood, of Cincinnati, to whom the author believes the arrangement is due of a coil or nest of tubes connected by return-bends; and Mr. Thomas T. Tasker, of Philadelphia, who introduced the first closed apparatus. For himself, the author may claim to have established certain characteristic shapes and dimensions now universally adopted for the "globe" stop-valves and for the fittings or couplings of the tubes.

Wrought Iron Welded Tubes.—In the construction of the apparatus for warming by steam, the prevailing practice in America is to employ wrought iron welded tubes, not only for the

mains, but also to a large extent for the radiating surfaces that diffuse the heat. The separate lengths of tubes are connected by wrought iron couplings, when in the same straight line; and when not so, by cast iron elbows, tees, branch-pieces, and return-bends. The so called coils or radiators usually employed for diffusing the heat are compact nests of tubes, sometimes arranged vertically by having their bottom ends screwed into a cast-iron box, and at other times placed horizontally and connected together by branch-tees and return-bends.

In the use of these tubes, the essential feature of practical importance is the employment of tapering screw-threads, externally upon the tube-ends and internally within the sockets of the couplings or fittings, as the means of securing durable steam-tight joints which can be readily made or unmade. The system of taper screw-threads for the tube connections is believed by the author to have been originated by Mr. Nason, with whom he became associated in 1846; at that time the threads were cut in a lathe.

Where any two metallic surfaces are to form a steam-tight joint, they must be brought together into complete contact, either with each other or with some elastic or yielding packing interposed between them; and the force compressing them together must be sufficient both to obliterate any inequalities in the surfaces themselves, and also to withstand any internal pressure tending to part them. In the case of the machine-finished surfaces of the external and internal taper screw-threads forming the tube joints, the required contact is no doubt mainly effected by the yielding of the metal of the tube under compression and of the socket under tension; while the thread itself serves to maintain the joint against the internal disruptive steam-pressure.

A screw-thread, either external or internal, cut or tapped by means of the usual workshop appliances, is far from possessing absolute accuracy in radius, either for the top of the thread or for the bottom of the groove; and presents also the defect known as "drunkenness," or want of uniformity in its inclination or pitch. In making a steam-tight joint, these imperfections have to be overcome by the application of force. The thread has also defects in angular form, arising from imperfections in the dies or taps, and from the metal having been torn instead of being cut clean in the formation of the thread. These defects are remedied by the use of a paste, made of white lead ground in linseed oil and mixed with about an equal quantity of dry red-lead. This material acts both as lubricant and as packing in making the joint; it fills all interstices in screwing the joint up, and sets hard on the first application of steam heat, or within a short time if left cold.

The leverage of the pipe-tongs or wrench, employed to screw the tube up, is largely multiplied by the action of the screw itself in converting the force into the longitudinal direction; and the tube end being moreover shaped as a conical plug of slight taper, the longitudinal force results in a greatly intensified pressure of contact between the threads of the tube and its socket. For example:—the ordinary pitch is $11\frac{1}{2}$ threads per inch for a tube of 1 inch nominal diameter inside or 1.31 inch actual diameter outside, and the taper of both the tube end and the inside of the socket is 1 in 32 to their axis. After the tube has been entered into the socket as far as it can readily be screwed up by hand, one quarter turn more with the pipe-tongs is generally sufficient to make the joint steam-tight. In this quarter turn of the tube the difference of diameter is in the ratio of $1 : 11\frac{1}{2} \times 16 \times 4$, and is therefore equal to 1-736th of an inch. Meanwhile the hand, acting on the tongs at a radius of usually about 16 inches, will move through an arc of 25 inches, giving thus an effective leverage of say 18,000 times. The bursting pressure thereby produced within the socket, or the compression upon the tube end, is concentrated on about $\frac{1}{2}$ inch length of the socket or tube, and the mean diameter is there about $1\frac{1}{4}$ inch; whence the pressure per square inch tending to burst the socket is roughly about 11,000 times the manual force exerted on the tongs, if the frictional resistance to turning be neglected.

In practice it is really by the frictional resistance to turning that the limit of bursting strain actually thrown upon the cast-iron socket in screwing up is determined. For supposing the limit be taken

to be a radial pressure of say 5,000 lbs. per square inch, it is seen from the foregoing that, after overcoming the frictional resistance to turning, an insignificant ¼lb. would be all the extra force required on the pipe-tongs for throwing this great bursting strain upon the socket. Allow now 15 per cent. as the coefficient of friction—a value not unreasonable when it is borne in mind that points of metal are brought into contact, and that the great pressure between them destroys the continuity of the imperfect lubricant. Then neglecting the screw-thread, and regarding the tube end and the casting as a plain conical plug and corresponding socket, the resistance to turning, offered by the ½-inch length of tube-end of 1¼ inch mean diameter, is 5,000 × 0.15 × 3.1416 × 1¼ × ½ = 1,400 lbs. acting at a radius of ⅝ inch, which is equivalent to about 55 lbs. exerted on the handle of the tongs of 16 inches radius. A skilled workman judiciously keeps well within this limit in the force which he exerts; and the cast-iron fittings being made according to the proportions hereafter given, the thickness of metal left outside the thread after tapping them is amply sufficient to stand the tensile strain so exerted, which rises probably to 10.000 or 12,000 lbs., say 5 tons per square inch of section of metal. In practice, moreover, it is found that a joint can safely be screwed up with the tongs through even one whole turn, instead of only a quarter turn; the wrought-iron tube-end yields permanently under the compression, while the limit of elasticity of the cast-iron socket is not exceeded, and hence, neither metal has its strength impaired. Complete facility is thus afforded for leading off bends or branches in any desired angular position.

The taper employed for the conical tube-ends is uniform with all makers of tubes or fittings—namely an inclination of 1 in 32 to the axis. Custom has established also a particular length of screwed end for each different diameter of tube. Tubes of the several diameters are kept in stock by manufacturers and merchants, and form the basis of a regular trade in the apparatus for warming by steam. A knowledge of all these particulars is, therefore, essential for designing apparatus for the purpose. The ruling dimension in wrought-iron tube work is the external diameter of certain nominal sizes, which are designated roughly according to their internal diameter. These nominal sizes were mainly established in the English tube trade between 1820 and 1840, and certain pitches of screw-thread were then adopted for them, the coarseness of the pitch varying roughly with the diameter, but in an arbitrary way utterly devoid of regularity. The length of the screwed portion on the tube end varies with the external diameter of the tube according to an arbitrary rule of thumb; whence results, for each size of tube, a certain minimum thickness of metal at the outer extremity of the tapering screwed tube-end. It is the determination of this minimum thickness of metal for the tapering screwed end of a wrought-iron tube which constitutes the question of mechanical interest.

The thread employed has an angle of 60°; it is slightly rounded off both at the top and at the bottom, so that the height or depth of the thread, instead of being exactly equal to the pitch, is only four-fifths of the pitch, or equal to $0.8\times\frac{1}{n}$ if n be the number of threads per inch. For the length of tube end throughout which the screw-thread continues perfect, the empirical formula used is $(0.8\ D + 4.8)\times\frac{1}{n}$, where D is the actual external diameter of the tube throughout its parallel length, and is expressed in inches. Further back, beyond the perfect threads, come two having the same taper at the bottom, but imperfect at the top. The remaining imperfect portion of the screw-thread, furthest back from the extremity of the tube, is not essential in any way to this system of joint; and its imperfection is simply incidental to the process of cutting the thread at a single operation. From the foregoing it follows that, at the very extremity of the tube, the diameter at the bottom of the thread$=\mathrm{D}-\left(\frac{2\times(0.8\mathrm{D}+4.8)}{32n}+\frac{3\times0.8}{n}\right)=\mathrm{D}-(0.05\mathrm{D}+1.9)\times\frac{1}{n}$. The thickness of iron below the bottom of the thread, at the tube extremity, is empirically taken to be $= 0.0175\ D + 0.025$. Hence the actual internal diameter d of any tube is found to be, in inches,

$$d = D - (0.05D + 1.9) \times \frac{1}{n} - 2 \times (0.0175\,D + 0.025), \text{ or } d = 0.965\,D - 0.05\frac{D}{n} - \frac{1.9}{n} - 0.05.$$

For the various sizes of tubes, ranging from ⅛ inch to 10 inches nominal internal diameter, with their corresponding numbers of screw-threads per inch, the actual internal diameter d is expressed by the following Table I. in terms of the actual external diameter D.

TABLE I.—DIAMETERS OF WROUGHT-IRON WELDED TUBES FOR WARMING BY STEAM.

Nominal internal diameter of tube.	No. of screw threads per inch.	Actual internal diameter d in terms of actual external diameter D.
Inches.	No.	Inches.
⅛	27	$d=0.9631\,D-0.1204$
¼ and ⅜	18	$d=0.9622\,D-0.1556$
½ and ¾	14	$d=0.9614\,D-0.1857$
1, 1¼, 1½ & 2	11½	$d=0.9607\,D-0.2152$
2½ to 10	8	$d=0.9587\,D-0.2875$

The figures derived from this statement which are of importance for practical use are presented in detail in the accompanying Table II. in a convenient order for reference.

The number of screw-threads per inch for the several sizes of tubes is here accepted from customary usage. It is the workman's approximation to the pitch practically desirable, and much reluctance must consequently be felt in calling it in question. Still it would have been better to investigate the general case upon the basis of a pitch ranging in closer accordance with the range of tube diameter. Thus the nominal ½-inch tubes might have had 16 threads per inch; ¾ inch, 14 threads; 1 and 1¼ inch, 12 threads; 1½ and 2 inches, 11 threads; 2½ to 3½ inches, 10 threads; 4 to 6 inches, 8 threads; 7 to 9 inches, 7 threads; and 10 inches, not more than 6 threads per inch. The existing numbers of threads, however, as given in Tables I. and II., are now too well established to be disturbed: at all events they must be taken in any statement of present practice.

The smaller sizes of tubes, up to and including 1¼ inch nominal inside diameter, are butt-welded, and are proved by hydraulic pressure to 300 lbs. per square inch. The larger sizes, commencing with 1½ inch, are lap welded, and are proved to 500 lbs. per square inch by hydraulic pressure. The question of butt-welding or lap-welding is simply one of economy in manufacture: it is not easy to make lap-welded tubes of less than 1¼ inch inside diameter, while it is cheaper to make lap welded than butt-welded tubes of that size and upwards. The proving pressures here given are far below the ultimate strength of sound welded tubes; and when required the test is increased to double or treble these pressures.

Couplings.—The internal threads in the sockets or fittings, by which the tubes are connected together, require the same accuracy of workmanship as the external threads on the tube ends. In practice it is found that the straight couplings or sockets, connecting tubes in the same straight line, may be made of wrought iron, and may be tapped parallel instead of taper; they are sized to screw freely upon the small extremity of the tapering tube-end up to as far as half the length of the perfect thread on the tube; and they then become stretched by the force exerted in screwing them upon the remaining half length of larger diameter, and form their own taper to fit the tube. To allow for clearance between the ends of the two tubes united by the coupling socket, the length of the coupling is made as nearly 2½ times the length of the perfect threaded tube-end as will suit the width of iron from which the coupling is manufactured. The minimum thicknesses of metal for the several sizes of couplings previous to tapping are given in Table III., with their corresponding diameters and lengths.

This parallel tapping is not altogether satisfactory for the straight couplings for tubes above 2½ or 3 ins. nominal diameter, and the joints so made are not reliable if screwed up with the ordinary tongs; for elbows or tees the method is very unsatisfactory. Tapering threads are usual in the wrought-iron sockets for the "oil tubing" in petroleum wells, and for the "line pipe" through which the petroleum is conveyed; these tubes range from 5 to 10 inches in diameter, and are subjected to pressures of 1,200 to 1,600 lbs. per square inch; the tubes themselves are frequently burst by the pressures ex-

TABLE II.—STANDARD DIMENSIONS OF WROUGHT-IRON WELDED TUBES FOR WARMING BY STEAM.

Diameter of tube.			Thickness of metal.	Circ'mfere'ce.		Length of tube per sq. foot of		Transverse area.		Length of tube to contain one cubic foot.	Weight per foot of length.	Screwed ends.	
Nominal inside.	Actual inside.	Actual outside.		Inside diameter.	Outside diameter.	Inside surface.	Outside surface.	Inside diameter.	Outside diameter.			No. of threads per inch.	Length of perfect screw.
Ins.	Ins.	Ins.	Ins.	Ins.	Ins.	Feet.	Feet.	Sq.ins.	Sq.ins	Feet.	Lbs.	No.	Inch,
⅛	0.270	0.405	0.068	0.848	1.272	14.150	9.440	0.0572	0.129	2500.00	0.243	27	0.19
¼	0.364	0.540	0.088	1.144	1.696	10.500	7.075	0.1041	0.229	1385.00	0.422	18	0 29
⅜	0.494	0.675	0.091	1.552	2.121	7.670	5.657	0.1916	0.358	751.50	0.561	18	0 30
½	0.623	0.840	0 109	1.957	2.652	6.130	4.502	0.3048	0.554	472.40	0.845	14	0.39
¾	0.824	1.050	0.113	2.589	3.299	4.635	3.637	0.5333	0.866	270.00	1.126	14	0.40
1	1.048	1.315	0.134	3.292	4.134	3.679	2.903	0.8627	1.357	166.90	1.670	11½	0.51
1¼	1.380	1.660	0.140	4.335	5.215	2.768	2.301	1.496	2.164	96.25	2.258	11½	0.54
1½	1.610	1 900	0.145	5.061	5.969	2.871	2.010	2.038	2.835	70.65	2.694	11½	0.55
2	2 067	2.375	0.154	6.494	7.461	1.848	1.611	3.355	4.430	42.36	3.667	11½	0.58
2½	2.468	2 875	0.204	7.754	9.032	1.547	1.328	4.783	6.491	30.11	5.773	8	0.89
3	3.067	3.500	0.217	9.636	10.996	1.245	1.091	7.388	9.621	19.49	7.547	8	0.95
3½	3.548	4.000	0.226	11 146	12.566	1.077	0.955	9.887	12.566	14.56	9.055	8	1.00
4	4.026	4.500	0.237	12.648	14.137	0.949	0.849	12.730	15.904	11.31	10.728	8	1.05
4½	4.508	5.000	0.246	14.158	15.708	0.848	0.765	15.939	19.635	9.08	12.492	8	1.10
5	5.045	5.563	0.259	15.849	17 475	0.757	0.629	19.990	24.299	7.20	14.564	8	1.16
6	6.065	6.625	0.280	19.054	20.813	0.630	0.577	28.889	34.471	4.98	18.767	8	1.26
7	7.023	7.625	0.301	22.063	23.954	0.544	0.505	38.737	45.663	3.72	23.410	8	1.36
8	7.982	8.625	0.322	25.076	27.096	0.478	0.444	50.039	58.426	2.88	28.348	8	1.46
9	9.000	9.688	0.344	28.277	30.433	0.425	0.394	63.633	73.715	2.26	34.077	8	1.57
10	10.019	10.750	0.366	31 475	33.772	0.381	0.355	78.838	90.762	1.80	40.641	8	1.68

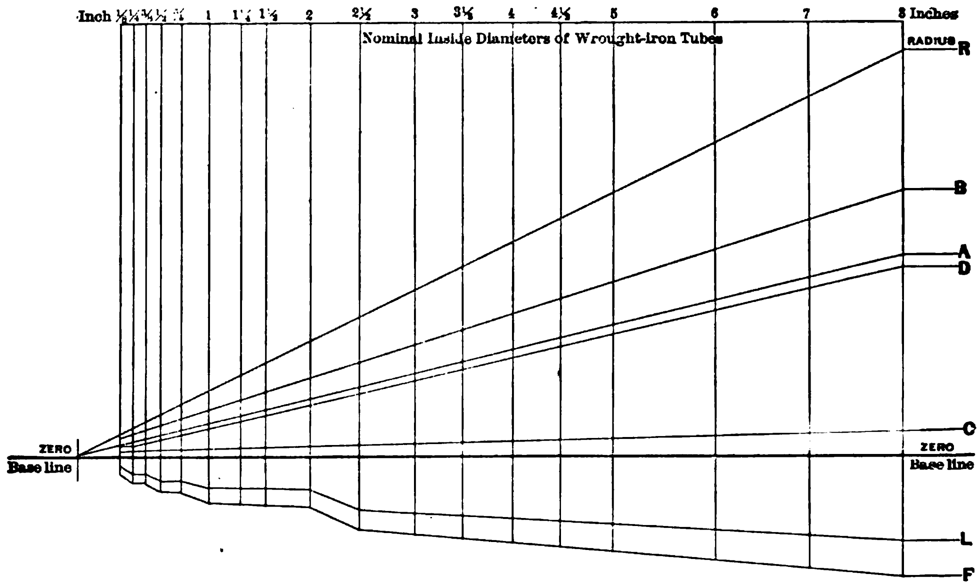
Taper of conical tube-ends, 1 in 32 to axis of tube.

In tubes for heating water or steam, the surface in contact with the products of combustion is to be taken as the effective heating surface, whether it be the inside or the outside of the tubes. But in tubes for heating liquids by steam, for superheating steam, or for transferring heat from one liquid or gas to another, the mean between the inside and outside surfaces is to be taken as the effective surface. In warming by steam, the outside surface exposed to the air is to be taken.

ceeding these, but the joints are perfectly reliable and unyielding. In the original construction of taper joints by Mr. Joseph Nason, the sockets of all sizes were made taper; the subsequent departure from this practice was a matter of cheapness in manufacture, and has never been a mechanical success.

The great power required for tapping a tapering thread in wrought iron, and the extra cost of making wrought-iron elbows and tees &c., have led to the employment of cast iron as the material for the fittings or couplings. Careful study having been bestowed upon the reduction of the material to the smallest quantity requisite, and upon the production of the cored castings with the least amount of preparatory work, these cast-iron fittings have now become universal for all joints of wrought-iron tubes, excepting only the straight couplings already mentioned. The scale drawn one quarter full size in Fig. 1 gives the exact dimensions for all parts of every possible elbow, tee, cross, or branch, for tubes of ⅛ inch up to 8 inches nominal inside diameter. With these dimensions the least quantity of material, consistent with uniformity of strength, is here employed, and is arranged in the most compact form. A longitudinal section is shown in Fig. 2 of a 1¼ inch elbow, that is, the elbow suitable for a nominal 1¼ inch tube. The dimensions for this section are obtained from the scale by measuring, along the vertical line headed 1¼ inch, the height of ordinate intercepted between the horizontal base or zero line and the slant line marked by the letter corresponding with that on the section. In the two orifices, which have to be tapped subsequently for screwing upon the tube-ends, the diameter (twice the dimension marked D in the section Fig. 2) is cored just so much smaller than the smallest diameter

Fig.1



Quarter-size scale for dimensions of cast-iron fittings.

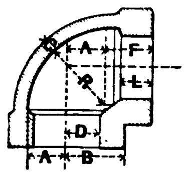
TABLE III.—DIMENSIONS OF STRAIGHT SOCKETS FOR COUPLING WROUGHT-IRON TUBES.

Diameter of tube.		Diameter of socket before tapping.		Thickness of socket before tapping.		Length of socket.
Nominal inside.	Actual outside.	Outside.	Inside.	Actual.	Birmingham wire gauge.	
Inches.	Inches.	Inches.	Inches.	Inch.	No.	Inches.
1/8	0.405	0.551	0.333	0.109	12	*7/8
1/4	0.540	0 699	0.431	0.134	10	1
3/8	0.675	0.834	0.566	0.134	10	1
1/2	0.840	1.011	0.699	0.156	8½	1 5/16
3/4	1.050	1.239	0.909	0.165	8	1⅜
1	1.315	1.527	1.143	0.192	6½	1½
1¼	1.660	1.893	1.487	0.203	6	1⅝
1½	1.900	2.166	1.726	0 220	5	*2
2	2.375	2 677	2.201	0.238	4	*2¼
2½	2.875	3.190	2.623	0.284	2	2½
3	3.500	3.870	3.245	0.313	—	3
3½	4.000	4.404	3.724	0.340	0	3⅛
4	4.500	4.992	4.242	0.375	00	3¼
4½	5.000	5.491	4.741	0.375	00	3⅜
5	5.563	6.186	5 311	0 438	—	3½
6	6.625	7.236	6.361	0.438	—	3¾
7	7.625	8.358	7.358	0.500	—	4
8	8.625	9.489	8.364	0.563	—	4¼
9	9 688	10.539	9.414	0.563	—	4½
10	10.750	11.720	10.470	0.625	—	5

* The three lengths marked by an asterisk are increased disproportionately for appearance.

of the root of the thread on the tube-end as will allow for removing the scale of hard iron from the casting by means of a drill; after which a taper tap, corresponding with the external thread on the tube-end, produces the internal thread in the socket, cutting with the proper taper a perfect thread inside the orifice through-

Fig. 2



Longitudinal section of cast-iron elbow for 1¼ inch tubes (before tapping) quarter full size.

out the whole of its length, marked L in the section. There are, therefore, excepting in special cases, no imperfect threads in the cast iron fittings or sockets.

It may be well to explain, in regard to the construction of the scale shown in Fig. 1, that the ordinates, though here headed for convenience with the nominal inside diameters of the tubes, are spaced from the zero point at horizontal distances (half full size) equal to the actual outside diameters as given in the foregoing Table II. Of the slant lines, only two pass through the origin or zero point: namely, the line for the internal radius R of the back of the elbow, and the line giving the dimension A which is exactly half the radius R. Of the remaining lines, those giving the three dimensions B, D, C, intersect the base line a little to the left of the origin or zero point: which means that each of the three dimensions given by these lines contains some small increment in excess of the dimension that would be strictly proportional. For instance, with regard to the thickness C at the back of the elbow, it is evident that, if the metal in any casting, whether small or large, thin or thick, were truly homogeneous throughout both its skin and its interior portion, this thickness should then be strictly proportional to the diameter or radius of the bore; but for small castings a certain allowance of extra thickness is needed, in order to ensure soundness, as well as to compensate for the increased brittleness of thin cast iron. There will however be some size of fitting for which the normal strength of metal may safely be assumed, and for which therefore no extra thickness will be needed. On this assumption the thickness for the largest size is set out at the end of the scale furthest from the origin; while nearest to the origin is set out the thickness proper for the smallest size, in which the increment is at its maximum; and the slanting straight line drawn between these two extremes gives for intermediate sizes an increment which varies inversely as the diameter or radius. Needlessly complicated formulæ, involving higher or lower powers of the diameter, are thus replaced by the simple straight line. In the two bottom lines L and F, pertaining to the screwed portions of the fitting, which are a function of the pitch, the irregularities of slope are due to the fact already commented upon, that the pitch of the screw-thread for different diameters of tube proceeds by a medley of arbitrary jumps, instead of by a steady progression concordant with that of the tube diameter.

Similar scales, but much more elaborate, furnish the dimensions for the valves and other details in the construction of steam-heating apparatus. The leading example already given will suffice however as an illustration of the method.

It might seem that too much prominence has here been given to minor details, in dwelling at such length upon the screw threads and the exact proportions of tubes and fittings; but it can be unhesitatingly asserted that facility of construction, economy of material, and careful elaboration of detail, have been largely concerned in the establishment in America of an industry almost unknown in Great Britain, and of which the history has hitherto remained almost wholly unrecorded. Indeed the only book in the English language on heating by steam, until the publication in America of an elementary work a few months ago, was that of Robertson Buchanan, 1811–1814. Beyond the catalogues and circulars of manufacturers and dealers, little or no practical information has appeared in print; while a whole class of skilled workmen, and nearly forty years of experience have developed the practicability of the

system, and extensive usage has accustomed the entire American community to its attendant discomforts and annoyances.

Boilers.—Most frequently, and in particular for large apparatus, the steam for warming in America is supplied by one or more horizontal tubular or Seguin boilers set in brickwork. For a small extent of warming apparatus the vertical tubular boiler with internal fire box is possibly the one most usual. For medium and large apparatus the original Seguin boiler is adopted, which consists of a horizontal cylindrical shell, containing tubes in the lower half, with a steam dome on the top; the tubes are arranged in vertical and horizontal rows, not alternating or zigzag; and the best practice is to place a manhole in the front end over the firedoor, and beneath the tubes. The boiler is fired underneath, and the products of combustion return through the tubes from the back to the front, and then pass back again over the top of the boiler, which is covered in with brickwork: the temperature of this top flue rarely exceeds 400° Fahrenheit. The fuel used is anthracite coal, yielding 8 to 12 per cent. of ash; and when well supplied with air it evaporates from 8½ to 9 lbs. of water per lb. of coal. The boiler fittings, gauges, valves, pipes, &c., are such as are usual for boilers in England; only, as they are rarely made by engine-builders, but by the "steam-fitting" maker, it can be claimed for them that they are generally more sightly, as well as better and less expensive, than in England. Dampers and damper-regulators are in general use, the latter being constructed with elastic diaphragms arranged to be loaded for any desired temperature, and often with some automatic arrangement for changing the load to meet changes of temperature outside or inside the building warmed. Where the whole of the circulation through the warming apparatus is entirely closed from communication with the atmosphere, the boiler pressure is sometimes allowed to fall below atmospheric pressure to as low as 140° temperature of the steam.

One other boiler at least has successfully stood the severe test of competition with the Seguin boiler, namely that known as the Babcock and Wilcox water-tube circulating boiler, having a horizontal steam-drum or separator overhead. This boiler was tried by Mr. Nason in nearly its present form, but with smaller tubes, the joints of which failed as made by him.

Either of these boilers is practically safe from disastrous explosion. Out of the many thousands of Seguin boilers which have now been in use from one year to forty years, only two or three well authenticated instances have occurred of violent explosion, and none, so far as the author is aware, have involved loss of life. Of all the stationary boilers that are employed in the United States for any purpose whatsoever, perhaps one half are of the Seguin type. The deterioration of all heating-apparatus boilers is more serious from rusting in summer when idle than from wear in use. In fair service the duration of the shell of the horizontal tubular boiler is from thirty to forty years, at least where water is employed that does not produce scale or deposit. The wrought-iron flue-tubes cannot be considered to last more than seven or eight years, even with water of good quality.

The most serious defect in the Seguin boiler is the liability of the bottom plate just over the fire-bridge, to "come down" or bulge outwards, or to become blistered when strong firing is carried to the extent of burning 10 lbs. of coal or upwards per square foot of grate per hour. This accident is more likely to happen when the tubes are arranged zigzag in alternate rows, or are placed close together the circulation of the water between them being impeded in either case; and the provision of a large water-space in the bottom of the boiler, such as results from the practice already mentioned of placing a manhole in the front end beneath the tubes, tends to obviate the risk. Recently a hollow framing or slab, made of boiler-plate, filled with water and connected with the boiler by circulating pipes, has been substituted for the front plate of the furnace containing the fire-door, and thus forms a permanent wall, serving also as a saddle to support the front end of the boiler. The details of brickwork setting, flues, and chimney, present no unusual features as carried out in America.

There are, of course, innumerable varieties of boilers and settings. The

possible contortions of boiler-plates tubes, and castings, are far from being exhausted; and the economy claimed for each fresh device is generally in proportion to its complication. Just now there is a crop of devices in America for the more complete combustion of gases, after the plans generally of the late Mr. Charles Wye Williams, Assoc. Inst. C. E.; some of these, and others for burning such fuel as saw-dust, coal-dust, spent tan, &c., are as successful as they were thirty years ago.

For the fire-grates also of the boilers there is a revival of sundry shaking appliances; but for the American anthracite these schemes will speedily die out, since it resents the least disturbance after having been once ignited, and admits only the gentlest turning at bottom for allowing the ash to run out like sand from an hour-glass. An improved grate, however, made with wrought-iron fire-bars of square or triangular section, with their ends resting in bearings so that each bar separately can be rotated, seems really to have won favor for heavy firing, and must be mentioned with approval.

For the smaller sizes of warming apparatus there are several devices of magazines for holding a store of fuel, from which a continuous supply to the fire-grate is dragged down or allowed to run down upon the grate. Some of these magazines hold 4 to 8 tons of coal, which will last from one month to three months. It may be mentioned that American anthracite coal, having the strength of most English building stone, is got from the seam in massive blocks; these on reaching the pit bank are broken up into lumps, which for domestic use are screened to various sizes, ranging from as small as peas to as large as eggs, while the "furnace coal" for manufacturing purposes is sorted in still larger sizes.

THE ENGLISH MILE: ITS RELATION TO THE SIZE OF THE EARTH, AND TO ANCIENT METRICS.

By JACOB M. CLARK, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THIS itinerary, on account of its lack of geographical correlation, and singular dimension, has evoked much interesting discussion, and been the means of bringing to the surface, under new aspects, a variety of important facts.

The reader is referred to a very instructive article in Vol. 25, p. 69, of this magazine, giving a full abstract of the views of M. Faye, as read before the Paris Academy, "On the Origin of the English Mile."

In that paper, the writer favors the view that the dimension is traceable to the survey of Eratosthenes, compared with that of Ptolemy; and, incidentally, that the surveys were conducted in terms of the Babylonian degree, and by implication, for the purpose of determining its length, or rather the subtense of one minute of arc; that the error in dimension arose partly from misapprehending the relative values of the stadia of different epochs, through disregarding the assumption that the computation of Eratosthenes was based on surveys made with the Egyptian foot* (0.27 m. = $10\frac{5}{8}$ inches), while the survey of Ptolemy was based on the Philetærian foot (nearly 0.36 m. = $14\frac{1}{6}$ inches).

Much additional light is thrown on the subject through a valuable contribution to these pages, from the pen of Prof. Mansfield Merriman ("The Shape and Size of the Earth," VAN NOSTRAND'S MAGAZINE, Vol. 22, pp. 53–62, 115–128, and 233–241). Reference is more particularly made to the different versions of the earth's circumference by ancient mathematicians on page 58. In the absence of direct evidence to the contrary, the results, the definition of amplitude observed by Eratosthenes, and the chronol-

* The Egyptian foot is uncertain. The dimension above given agrees very fairly with the ady of Malabar, the palmo of Malta, Messina, Naples, Sardinia, and Nice, the pièd of Rouen, the stonecutter's schuh of Zug, and the miner's spanne of Prussia; dimensions varying from 10.265 to 10.570 inches.

The Philetæric foot is quoted by Alexander as equivalent to 13.893 inches.

ogy,* as given by Prof. Merriman, must be taken as clear and conclusive.

Hitherto, the moderns have regarded these statements as the results of successive experiments by the ancient geometers, to ascertain what has been supposed to be unknown to them—the length of the terrestrial degree, or, at any rate, the true circumference of the earth; and, on the face of it, the discrepancy is certainly glaring enough to justify the impression, and, at the same time, to suggest the theory both of serious errors in their work, and confusion among them as to the dimensions of the stadia used in the different surveys.

The true character and object of these operations can only be understood by reading them in the light of contemporaneous history and in view of the spirit of the times, aided by such knowledge of the actual dimensions made use of as can be obtained or fairly inferred.

It should be borne in mind that, as a rule, conquest has always involved more or less serious interference with the metrics of the people. And, from the nature of the case, this was a prominent feature of state policy among the ancients. So long as a subjugated, but powerful and intelligent people retained the use of their traditional measures, cherished by the philosophers, and indissolubly connected with the mysteries and service of the temple, their complete subjection would be a matter of doubt. But the perils involved in the sudden and arbitrary overthrow of entire systems, were generally, in fact, sought to be avoided by modifications—compromises, under more or less specious and flattering pretexts.

Now as to the measures—the stadia probably used—and the mode of reckoning:

The Greek stadium was $\frac{1}{8}$ of their mile of 1,000 paces, or double parade-steps; value = $604\frac{3}{8}$ feet. The Romans had practically the same mile. Elsewhere, the stadium was 100 fathoms, and the fathom, generally, 3 cubits. But the Hebrew fathom was 4 cubits.

The Egyptian, Phenician, and Persian cubit was $\frac{1}{100}$ of the schœnus, which was equivalent to $145\frac{92}{100}$ English feet. The Hebrew cubit was $\frac{1}{80}$ of the schœnus.

The Babylonian cubit was apparently the subtense of 1° on a radius of 100 duodecimal feet, or 1,200 inches. Its value would depend on that of the inch. A form of it appears in Egypt, with some uncertainty as to the date of its introduction, under the name of the royal cubit. The Turin and Nilometer cubits, so called, are versions of it. The dimension was not far from 1.75 feet. A slight modification, to be understood further on, would place it at 1.75104 English feet.

There would result:

The Egyptian stadium = $437\frac{76}{100}$ English feet.
The Hebrew stadium = $729\frac{6}{10}$ English feet.
The (supposed) stadium on fathom of 3 Hebrew cubits = $547\frac{2}{10}$ English feet.
The (supposed) stadium on fathom of 3 royal cubits = $525\frac{312}{1000}$ English feet.

Among the peoples concerned, the reckoning, for all general purposes, was purely decimal, except that the Babylonians had the duodecimal mixed up in their system, with alternations of 6 and 10.

To facilitate the view, the different results are arranged in the table in chronological relation with prominent epochs, and in connection with the above lengths of stadia:

Epoch.	Event, etc.	Earth's circumference in stadia.	Length of stad. English feet.
B. C. 538	Babylon taken by Cyrus.		
B. C. 525	Independence of Egypt destroyed by Cambyses.		
B. C. 340	ARISTOTLE........	300,000	437.76
B. C. 332	Macedonian Conquest; end of the Pharaohs.		
B. C. 250	ARCHIMEDES......	300,000	437.76
B. C. 230	ERATOSTHENES....	250,000	525.312
B. C. 146	Greece made a Roman province.		
B. C. 90	POSIDONIAS........	240,000	547.20
B. C. 30	Cleopatra's death; end of the Ptolemies; Egypt becomes a Roman province.		
A. D. 170	PTOLEMY (in the reign of Marcus Aurelius)........	180,000	729.60

With these values of the stadium, the circumference in each case is 131,328,000 English feet.

* In a recent discussion (Trans. Am. Soc. C. E., vol. XI., p. 415), the writer, adopting this chronology, inadvertently placed Aristotle 200, instead of 100, years earlier than Archimedes.

By Clarke's elements of 1878, as quoted by Prof. Merriman, the mean circumference is 131,331,455 English feet.

The skill and accuracy of ancient astronomers is strikingly illustrated by the survey of Almamoun, in Mesopotamia, in the 9th century, referred to for illustration by both Prof. Merriman and M. Faye. Taking the Arabian mile (palpably a version of Ezekiel's 500 reeds) at Haswell's quotation, 2,146 yards, with the Professor's statement of the result, $56\frac{2}{3}$ miles to the degree, the circumference is $2{,}146 \times 3 \times 56\frac{2}{3} \times 360 = 131{,}335{,}200$ feet, a trifle above the ancient and modern, in a total disagreement as to the whole circumference of less than a mile and a half.

Both Egypt and Mesopotamia are fairly situated for apprehending the mean circumference by meridian observations.

Leaving aside, for the moment, the above suggested adjustment of the royal cubit, the question arises pretty distinctly whether the most promising theory to square with all the facts may not, after all, be something like this:

(1.) At the very earliest assignable epoch, the mean circumference of the earth, and consequently its radius, were known with astonishing precision. Under a very perfect system of geometry, the metrics of the ancient leading nations were founded on this knowledge. The opinions of Aristotle and Archimedes were derived from this source, through Egypt.

(2.) After the Macedonian conquest, it became apparent that, by the breaking up and commingling of nationalities, the multiplicity of units was inconvenient and perplexing. The Mosaic and Babylonian cubits were in collision. The "cubit and a hand-breadth" of Ezekiel, by this time widely diffused and popularized, differed from the 2-feet rule by about an inch. The Egyptian and Persian cubit was becoming confounded with the Indian* cubit of 18 inches. And the Greek measures were a new element of discord. Eratosthenes was charged by Ptolemy Philadelphus with the work of reform. To satisfy the prevailing preferences for the decimal method, and at the same time strike a reducible mean among the cubits, an itinerary was invented which should be an even decimal of 4 terrestrial great circles. It is more than probable that the survey of Eratosthenes was simply to test the correctness of the ancient standards, and fix the adjustment of the royal cubit. The circumference was already known, according to the Egyptians, and if their account proved correct, the *relation* was apparent beforehand. The royal cubit would have to be $\frac{6}{5}$ of the Egyptian $= \frac{24}{25}$ of the Mosaic $=$ 1.75104 English feet.

(3.) This unwieldy division of the circle, unfit for geography or astronomy, along with the strong preferences of the Egyptians, Persians, Hebrews and kindred races for the ancient measures, and their wide-spread traditional sympathy as against Babylonian methods, finally broke this system down. Accordingly, after the Roman supremacy was established, and in the reign of one of the later Ptolemies, Posidonias restored the Mosaic cubit, but in a 3-cubit fathom, so that his itinerary was decimally related to the hour angle. And so far from his survey being the worst measurement of a degree ever made, it serves to verify in a very lucid way the work of his predecessors. The aptness of his system, as an itinerary, is attested by the survival of the Turkish mile, through all vicissitudes, and of its correlative fathom, by dozens of analogues, in the islands and along the Mediterranean and in Central Europe. And it may seem significant to many that in the Apocalypse, written 186 years later, the division of the circle by 24 is paramount.

(4.) Finally, in the reign of Marcus Aurelius, a further attempt at unification seems to have been made, by re-instituting the Mosaic itinerary—the leuga of the ancient Gauls and the mile of Sardinia. This was the survey of Ptolemy. Possibly it involved some concession to the Egyptians, in that the schœnus became simply 20 fathoms, instead of $33\frac{1}{3}$ as by their ancient method or $26\frac{2}{3}$ as by that of Posidonias. It lacked, however, the key-note of Ezekiel's reform—radius to be the base of direct

* It is a peculiarity of the purely duodecimal method that, reckoning from the inch, it has no longer dimension than the 12-feet pole or "joktan." And, wherever it has taken root, this dimension, as well as its derivatives by bisection, the vulgar fathom, the yard, and the Indian cubit, as also the foot, when used as units, are, as a rule, but with occasional intermediaries of 3 and 6, reckoned upwards decimally. Its singular distribution—about the Mediterranean, among the islands, as Great Britain and Japan, and upon the salients of Africa and Asia—are strikingly suggestive of the maritime enterprise of busy Tyre.

and square measure, itinerary to be ruled by the division of the circle.

There seems to be little, either in the accounts we have, the necessities of the case, or the character of the rulers under whom these operations were conducted, to indicate that they were instituted for any other purpose than that above suggested—verification of the ancient work, and adjustment of the standard for the particular purpose in view at the time. Neither is it apparent that in any of them, the Babylonian degree was either used or its dimension sought (except possibly as to Ptolemy), or that the Egyptian foot, whatever it may have been, or the Philetærian foot or the Greek stadium were used or referred to at all, or that the geodetic work had any other fundamental base than the ancient public surveys of Egypt, familiar to the learned throughout the whole period of operations, and unquestionably made with the schœnus. And this view is confirmed by Eratosthenes' definition of amplitude, as quoted by Prof. Merriman—$\frac{1}{60}$ of 4 right angles — a decidedly decimal expression as well as by the general ancient preference for the decimal method.

The English mile, by its dimension, suggests with strong probability that it was at first either equal to 1751 yards, representing the survey of Eratosthenes, or else 1824, like the Turkish mile, the itinerary of Posidonias; and that it took its present form at the time the English forced the 36-inch yard into their land measure, by means of the invention of Gunter's chain. The former is the more probable of the two.

ELECTRIC POTENTIAL, ENERGY AND WORK.

By Lieut. BRADLEY A. FISKE, U. S. N.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

In no study more than in that of electrical science is a clear comprehension needed of the terms used in the nomenclature, and of the laws under which the forces act. The purely experimental stage of its development has passed, and we have now reached a point where theory and practice must work hand in hand. A careful effort to apply its principles to its practical applications will discover the methods of securing effects by the most rapid and inexpensive means, and it is a well-known fact that the surprising results obtained in practice by many inventors are not due to happy inspiration or to experiment alone, but also to profound and laborious calculation.

To many, the terms heading this paper convey a very indefinite meaning, and their relation to the question of electric supply, lighting, etc., seem very remote. It can be shown, however, that they possess a most real and absolute signification, and that they bear directly upon nearly all the problems of electrical engineering.

The books tell us that electric potential means power to do electrical work, and that the difference of potential between two points is the number of units of work necessary to be done on a plus unit of electricity in order to move it from one to the other.

Work, as is well known, is the product of force by distance. In order to ascertain the relations between electrical and mechanical work, it has been necessary to devise units in terms of which both can be expressed.

In the system invented, known as the c. g. s. system, the unit of force is called the *dyne*, and means that force which, acting for one second on a mass of one gramme gives it a velocity of one centimeter per second. The unit of length is the centimeter; and the unit quantity of electricity assumed is that which, placed at a distance of one centimeter from a similar and equal quantity, repels it with a force of one dyne.

The dyne being the unit of force, and the centimeter the unit of length, the unit of work must be the dyne-centimeter. To this unit is given the name *erg*.

From the fact that charges of electricity of similar name repel one another, it is evident that to move a plus unit of electricity towards a plus charge necessitates the expenditure of work. Suppos-

ing the plus unit to be at an infinite distance from the plus charge, and therefore free from its influence, and then to be moved towards it, the number of ergs expended in getting it up to a certain point expresses the potential of that point. To move the unit to a point nearer the charge would clearly require a greater expenditure of work; in other words, the potential of the second point would be higher.

Now, it is obvious that to move any plus unit from the first to the second point would require an amount of work equal to the difference of the two amounts; so that, if this difference were one erg, the difference of potential between the two points would be one.

If, on the other hand, instead of raising a quantity of electricity from a lower to a higher potential, we allow it to fall from a higher to a lower, it will, in accordance with the law of conservation of energy, perform exactly the same work as was expended upon it in raising it, through the same difference of potential.

We see at once that this is the case with which we have to deal in computing the electrical work done in any circuit, for there we find units of electricity falling through units of potential. The units (called electro-magnetic units) are different from those thus far considered (called electro-static), but the same laws evidently apply.

An electro-magnetic unit of quantity is that carried in one second by a current of such a strength that, if a length of it equal to one centimeter be bent into an arc having a radius of one centimeter, it will act with a force of one dyne on a unit magnet pole placed at its center. Now, this unit bears to the electro-static unit the ratio

$$LT^{-1};$$

but as the electro-magnetic difference of potential bears to the electro-static the ratio

$$\frac{1}{LT^{-1}}$$

one electro-magnetic unit performs just one erg of work in falling through one electro-magnetic unit of potential.

Therefore, if we know the strength of current passing through any circuit or part of a circuit, and the potential through which it falls in so passing, the work performed in one second is clearly

$$CE,$$

or, as sometimes written,

$$C^2 R \text{ or } \frac{E^2}{R}.$$

But, as these units are manifestly too small, larger and more practical ones are used. Of these, the volt equals 10^8 electro-magnetic units of potential, and the ohm equals 10^9 electro-magnetic units of resistance. Necessarily, therefore, the practical unit of quantity is 10^{-1} times the electro-magnetic unit.

Therefore, if in measuring with our practical instruments, we find a strength of current C′ and a fall of potential E′, it is obvious that the work done in one second is in ergs,

$$C'E' \times 10^7.$$

Knowing, as we do, that one horse-power means an energy of 746×10^7 ergs in one second, the horse-power performed is, of course,

$$\frac{C'E' \times 10^7}{746 \times 10^7} = \frac{C'E'}{746}$$

or, as often written,

$$\frac{E^2 \times 44.24}{R \times 33.000}$$

As it is potential which does the work of driving the current through the resistances of the circuit, it follows that less potential is expended in any circuit in overcoming the small resistances than the great ones. From this it follows again that, in any simple circuit, the fall of potential in different parts is proportional to the resistances of those parts; and also that, as no generator, whether battery, accumulator, or machine is a perfect conductor of electricity, we must not neglect to take its resistance into consideration. This is frequently neglected, and great mistakes sometimes ensue in consequence. The Electrical Power Storage Co., in England, gave out recently that the number of their accumulators necessary to feed a given number of incandescence lamps could be calculated by simply dividing the potential needed for one lamp by the electro-motive force of one cell. Though plausible upon its face, this rule is clearly incorrect, for the rea-

son that all of the electro-motive force generated is not furnished to the external circuit, but only the fraction thereof, represented by the quotient of the external resistance divided by the generator's resistance plus the external resistance. Evidently, when a great many lamps in multiple arc are fed by a number of accumulators in series, the distribution of potential is far from being the same that it is when only a few are supplied. Suppose we wish to know how many accumulators, having each an electromotive force e, are necessary to supply n incandescence lamps, having each a resistance r' when hot; the potential necessary for each lamp being e'.

The whole current necessary, then, is

$$C=n\frac{e'}{r'}.$$

Suppose the current which each cell is capable of furnishing most economically to be c. Then we must have such a number of series of cells that the whole current shall be c. Letting y = this number—

$$y=\frac{C}{c}.$$

Let x=No. cells in each series,
" r=resistance of each cell,
" R= " " conductors.

Then

$$C=\frac{xe}{\frac{xr}{y}+\frac{r'}{n}+R}=\frac{xeny}{nxr+r'y+Rny.}$$

$$Cnxr+Cr'y+CRny=xeny$$

$$x(eny-Cnr)=Cr'y+CRny.$$

$$\text{But } C=\frac{ne'}{r'}.\therefore n=\frac{r'C}{e'}$$

$$\therefore x(ey-Cr)=e'y+\frac{R}{r'}ne'y.$$

$$x=\frac{e'y+\frac{R}{r'}ne'y}{ey-Cr.}$$

In case R is very small as compared with r', this formula may be written—

$$x=\frac{e'y}{ey-Cr.}$$

It will be noticed that we have considered this as a simple circuit by taking the joint resistance of the lamps and conductors as a single external resistance.

In calculating the fall of potential in any branch of a divided circuit, we have to deal with somewhat different conditions. When two or more branches divide and reunite at common points, the difference of potential at the ends of each branch, instead of being that due to the resistance of that branch, is only that due to the joint resistance of all the branches. When lamps or motors are supplied in multiple arc, however, we cannot say that the difference of potential supplied to each lamp is that due to the joint resistance of all the lamps, even if all are burning, for the obvious reason that each lamp does not get its wires direct from the generator, and that all are not at the same distance therefrom. For this reason, the potentials furnished all the lamps are not equal, but vary with their distances from the generator, or rather with the resistances of the mains between the generator and the points from which the lamps draw their supply. Clearly, a lamp near the generator will get the benefit of nearly the whole potential furnished to the circuit, for the reason that it takes one wire from very near the positive pole of the generator, and the other from very near its negative pole; while the case of the lamp farthest away is manifestly different. This lamp takes one wire from a point where the potential is less than at the positive pole, and takes the other wire from a point where it is greater than at the negative pole. In order that the difference of potential furnished the lamps may vary as little as possible, both from this cause and by reason of adding lamps to or subtracting them from the circuit, it is clear that the resistances of the mains and the machines should be as small as possible.

Remembering the formula

$$CE,$$

and remembering, too, that in any simple circuit, the fall of potential is proportioned among the resistances, the formulas follow for the *electrical* efficiency of machines—*i. e.*, the ratio of the energy in the external circuit to that generated.

The most simple case is that of the series dynamo. As the current, C, is the same in the external circuit, the

armature and the magnet coils, the efficiency F is clearly

$$\frac{r}{a+m+r}$$

where r is the resistance of the external circuit, a that of the armature, and m that of the magnet coils.

In the case of the shunt dynamo, the current in the armature is equal to the sum of the currents in the external circuit and the magnet coils, supposing shunt taken direct from brushes.

Let A=current in armature, then current in main circuit=

$$A\frac{m}{m+r}$$

and the whole resistance of the circuit

$$R=\frac{mr}{m+r}+a$$

$$\therefore F=\frac{\left(A\frac{m}{m+r}\right)^2 r}{A^2\left(\frac{mr}{m+r}+a\right)}=\frac{m^2 r}{a\ +r)^2+m^2\ r+m\ r^2}$$

The case of a dynamo whose field magnets are wound partly in shunt and partly in series is very similar.

It is obvious that the formala for the proportion of energy consumed in the external circuit plus that consumed in the series coils (supposing the shunt to be taken direct from the brushes) may be gotten from the above formula by simply substituting r' for r, in which case r' equals the added resistances of external circuit and series coils.

$$\therefore F'\frac{m^2 r'}{a(m+r')^2+m^2 r'+mr'^2}$$

Letting r, as before, equal the resistance of the external circuit alone, we have

$$F : F'=r : r'$$

$$F=\frac{Fr}{r'}$$

$$\therefore F=\frac{m^2 r}{a(m+r')^2+m^2 r'+mr'^2}$$

In the case of transmitting power from one point to another, the amount of energy consumed in the wires becomes a most important consideration. This energy appears in the form of heat, which is not only lost for all practical purposes, but which has the further effect of raising the resistance of the wires, which, in turn, causes the absorption of more heat.

Evidently, as we are to give power to the wire, we wish to know in what form it is most economical to give it, in order to reduce this loss in transitu to a minimum. Power being composed of the two factors C and E, is it better to make C and E large? A casual inspection of the formula $\frac{E^2}{R}$ might lead to the hasty conclusion that it were better to generate as low an electro-motive force as possible. But this conclusion would manifestly be incorrect, for the reason that this would necessitate a large C in order to produce to requisite power; and we all know that heat in any circuit varies as C^2. The formula, it need not be said, however, is correct, for here E means not the whole electro-motive force, but only that fraction thereof represented by the ratio of the external to the whole resistance. Thus we see that we must get R as small as possible, because, although E diminishes in the same ratio, yet E enters by its square. We also see that the whole resistance as compared with R must be great. In other words, we must use a machine with considerable resistance generating a high electro-motive force, and conductors of small resistance. Thus we clearly get a small C.

It is to be observed, however, that, in cases of power distribution, other considerations may have weight.

It may be desired, for instance, to transmit power during the day by the same company which supplies incandescence lamps by night. In this case it would probably be found more economical to use to same wires and to supply motors in multiple arc, than to have a separate system supplying them in series. The greater safety of a low tension current is, moreover, a most powerful consideration.

In applying the formulas

$$C\ E\frac{E^2}{R}\text{and } C^2R,$$

care must be taken to remember that as C indicates the quantity of electricity passing in one second, these expressions indicate not work, but the amount of

work performed in one second. In order to get the work, we must either multiply the expression representing the energy by the time during which that energy was furnished, or else we must find the number of coulombs passing in the time. Thus we see the rationale of the Edison meter. The indicator shows the number of coulombs which have passed; and as the potential is kept nearly constant, this indication furnishes a measure of the work which has passed through the subscriber's wires. In other words, the subscriber does not pay for the amount of electricity furnished, but for the work performed.

In conclusion, it would seem pertinent to suggest that it is not well to have the same unit, the erg, for both energy and power. Almost every issue of the English "Electrical Review" contains communications from persons, who, from that cause alone, seem to hopelessly confound electrical energy and power.

FAST TRAINS IN EUROPE AND AMERICA.

By Mr. A. L. ROTCH.

Read before the E. M. E. Society of the Massachusetts Institute of Technology.

I SHALL first consider American trains, taking into account the acceleration which has occurred within the last few years. For England, my data are brought down to 1880, and on the Continent it is as old as 1878; but, generally speaking, the figures are sufficiently accurate to-day to admit of comparisons.

AMERICA.—The fastest trains in the United States are in the East, and are those between Jersey City and Philadelphia, Boston and New York, and one from New York to Albany. In selecting representative trains I have given preference to those which maintain the highest speed for the longest time. Probably the fastest train in this country, and one which is little below the speed of the fastest English trains for the same distance, is the Philadelphia express, on the Pennsylvania Railroad, which leaves Jersey City at 4.08 p.m., and makes the run of 88.4 miles in 1 hour 52 minutes, including three stops, or at the rate of 47.8 miles per hour. Jersey City to Germantown Junction, 84.2 miles, is run in 1 hour 41 minutes, including one stop, or 50.5 miles per hour. I timed last winter the fastest train east, whose schedule time is 1 hour 59 minutes. It was then, however, 6 minutes late, and as it was bulletined "on time," perhaps it seldom does better. The train consisted of six cars, one of them a Pullman. A stop was made at Germantown Junction, and it was some time before a high speed was attained. The fastest miles were run in 63 seconds each, or 57 miles per hour; 21 miles were made in 25¾ minutes, or 49 miles per hour. The 84.2 miles from Germantown Junction to Jersey City were run in 1 hour 52¼ minutes=45 miles per hour, including slackening through several towns. It may here be said that the Pennsylvania road-bed equals that of any foreign road I have seen, while the freedom of the train from oscillation and vibration was remarkable. This train used to run in five minutes less time, and the fast train west was two minutes quicker when started a few years ago, so perhaps it was found impossible to maintain these higher speeds. The Bound Brook route of the Philadelphia and Reading Railroad has a train from Philadelphia to Jersey City, 89.4 miles, in 1 hour 57 minutes, including five stops, or 45.8 miles per hour. Wayne Junction to Jersey City, 85.1 miles, is run in 1 hour 47 minutes, including three stops, which is 47.7 miles per hour. The 4.30 p.m. train from New York to Boston *via* Springfield, is the fastest between the two cities, taking 6 hours 3 minutes for the 234 miles. I timed the train last September when it was on time. The engines were of the ordinary type, with about 5 feet 6 inch drivers, and the train consisted of six cars, two of them being parlor cars. On the New Haven Division, the fastest runs were 1.90 miles in two minutes=57 miles per hour; 1.73 miles in one minute 55 seconds=54.5, and three miles in 3 minutes 15 seconds=53 miles per hour. Owing to the Connecticut law requiring a stop at every drawbridge, the time for

the 73¼ miles between New York and New Haven, including six stops, was 1 hour 54¾ minutes, or only 38.5 miles per hour. The 62 miles between New Haven and Hartford were made in 1 hour 30¼ minutes, including two stops, or 41.5 miles per hour; average speed while running, 43.8 miles per hour. On the Boston and Albany, 96 miles (Springfield to junction of Brookline Branch, Boston) were run in 2 hours 20½ minutes, including three stops, or 41 miles per hour; running speed 43.2 miles per hour, including stops. The time for the trip of 234 miles was 5 hours 30 minutes, and the speed 42.5 miles per hour. The Shore Line express makes its run to Stonington, 93 miles, in 2 hours 1 minute including a stop at Providence, or over 46 miles per hour. As long ago as 1878 the *Railroad Gazette* said: "For over two years a daily train has been run over the 43½ miles between Boston and Providence in one hour, including a dead stop at the crossing, a 6-mile grade over Sharon hill of 35 feet to the mile, a slowing up at Mansfield and through Pawtucket, over a mile, and a slacking over three bridges. The engines have 5½-foot drivers and 17-inch cylinders. The train is a baggage and smoker, and there are sometimes four 24-ton passenger and a 32-ton Pullman." In February, 1881, I timed this train as follows: Left crossing 1.03 p.m. Beyond Foxboro' the speed was a mile in 63 seconds, or 57 miles per hour; 2.48 miles were run in 2 minutes 40 seconds=55.5 per hour. Before reaching Pawtucket, 14.9 miles had been run in 17½ minutes=54.5 miles per hour. Providence was reached at 1.59 p.m.=45 miles per hour. From the crossing the speed was 47 miles per hour. Last June this train was quickened beyond Providence as above. The fastest train on the New York Central Railroad is the Chicago and Lake Shore special, which runs to Albany, 140 miles, without a stop, at the rate of 49.5 miles per hour. The first 58¾ miles are made in 1 hour 28 minutes. These figures should, I think, be sufficient to show that regular trains in this country seldom run a mile in 60 seconds. The fastest run which I have made here was on the New York and New England Railroad the other day, when, on a down grade beyond Bolton Notch, 4 miles were covered in 4 minutes 10 seconds, or 58 miles per hour. In England I have been faster, and it is said, on authority, that it is not uncommon for English express trains to make 60 or 70 miles per hour when running on their schedule time. It has been said that it is impossible for an engine with a 5½-foot driver to make a mile in a minute. Mr. Le Van gives the greatest piston speed as about 1,200 feet per minute, which would give about 59 miles per hour with such a wheel. On the Pennsylvania the fast train engines had two pair of drivers 68 inches in diameter and had to make 300 revolutions to make a mile a minute. Coupled drivers are said to be dangerous at high speeds from the liability of the parallel-rods to break, and such accidents have recently happened. Still, in late years, large coupled wheels have been much used on English and continental express engines. When in Europe during 1877–78, I looked up the fastest trains in the several countries, and as I traveled by some of them I further noted their maximum speeds. A few general remarks about European railways may be interesting. The continental railways are partly controlled by the governments. They are well built, with curves of large radius. Instead of cars there are carriages, divided into compartments holding six or eight persons. They do not, as a rule, have bogie trucks, but their small wheel-base enables them to pass round the curves. There are three or four classes of passengers. Generally, first and second class are carried by the express trains, but in France only first-class are taken, and a higher class are charged. The locomotives often have only a single pair of drivers, 7 feet or so in diameter. In England 6½-feet driving-wheels are quite as common as 5½ feet with us, and some engines on the Great Western and Great Northern railways have wheels 8 or 9 feet in diameter. There is much dispute as to the merits of single *versus* coupled engines, but I noticed at the Paris Exposition that all the French express engines shown had their driving-wheels coupled. Many of the modern engines have cabs and bogie trucks in front. Distances are given on the Continent in kilometers (0.62 mile each), which have been converted into miles for the sake of comparision here.

SWITZERLAND AND RUSSIA.—There are no trains exceeding 27 miles per hour in those countries, which are, therefore, not considered. In Belgium, trains travel as fast as 42 miles per hour, but these are generally through trains between France and Germany, and may, therefore, be classed with the trains of those countries.

ITALY.—The only fast train is the mail which makes the long run from Bologna to Brindisi, 472 miles, in 14 hours 55 minutes, which, including three stops, is 31.5 miles per hour. The train is largely due to English enterprise. It carries the English mails, and takes only through passengers.

FRANCE.—Many of the French expresses are fast. I believe the fastest long distance run is on the Orleans line, from Paris to Bordeaux, where 359 miles are run in 9 hours 10 minutes, including 17 stops, an average of over 39 miles per hour. Allowance being made for these, the average running speed is 42.5 miles per hour. I do not think this performance is surpassed in America. From Paris to Marseilles is 536 miles, which used to be covered in 15 hours 25 minutes, at the rate of 34.8 miles per hour, including 13 stops made. The time has since been reduced to 15 hours only. The expresses on the Northern Railway run from Calais to Paris, 184½ miles, at 36.7 miles per hour, including seven stops, or at over 39 miles per hour, while running. A run of 27 miles is made at the rate of 45.5 miles per hour. I traveled by one of these trains and found the oscillation of the short carriages tremendous. On the Eastern Railway, the schedule time for a run of 47½ miles is 1 hour 5 minutes, and on the Western Railway, I timed the express from Paris to Dieppe, running at its usual rate of speed, 16¼ miles in 20 minutes 20 seconds, or 47.7 miles per hour.

GERMANY.—Taking Germany and Austria together, the fastest trains are found in North Germany. Undoubtedly the fastest train is from Berlin to Hanover, on the Magdeburg-Halberstadt Railway 152½ miles in 3 hours 48 minutes, including three stops=51.7 miles per hour. Deducting these, the speed is 44 2 miles per hour. This beats the fast train from New York to Albany, which is the fastest train in America for that distance. A run of 93½ miles is made by the German train in 2 hours 8 minutes=44 miles per hour. I timed the part from Berlin to Stendal. Some miles were run at 52 miles per hour; 62½ miles were done in 1 hour 30 minutes, including a stop. Berlin to Cologne by this route is 363 miles, accomplished in 9 hours 41 minutes=37.5 miles per hour. On the Berlin-Potsdam-Magdeburg Railway the fast express I have timed 24¾ miles in 30 minutes=48.7 miles per hour and 50 miles in 1 hour 9 minutes=43.5 miles per hour. This forms part of the through line between Berlin and Paris, and is, perhaps, the best example of a very long run abroad. The distance is 668 miles, done in about 22½ hours, or at the rate of about 30 miles per hour, notwithstanding the fact that three countries are traversed with attendant custom-house formalties at the frontiers. Thus it will be seen that the speeds of the fastest continental trains about equal our own, except in the case noted. For very long runs nothing there equals the speed of the New York-Chicago trains, which average some 35 miles per hour for over 900 miles.

ENGLAND.—In England, however, we are beaten both in number and speed of fast trains. Mr. Le Van says the average speeds are 20 per cent. greater and the loads 25 per cent. less. Where there are so many rival trains running at nearly the same speeds, it is difficult to name the fastest. The "Flying Scotchman" (*via* the Great Northern) runs to Edinburgh, 397 miles, in 9½ hours, including stoppages, at nearly 42 miles per hour. The Great Western has a 7-feet gauge, and formerly its express went from London to Bristol, 118¼ miles, in two hours =59 miles per hour. This train, known as the "Flying Dutchman," is still, perhaps, the fastest train in the world, though its speed has been much reduced. The 118¼ miles are now made in 2 hours 36 minutes=45.3 miles per hour, including two stops amounting to 11 minutes. Excluding these, the speed is 49 miles per hour. London to Exeter, 194 miles in 4¼ hours, exclusive of 20 minutes' halt on the way, is 49.5 miles per hour. London to Swindon, without stopping, 77¼ miles in 1 hour 27 minutes, is 53.3 miles per hour. The Leeds summer expresses on the Great Northern make the

186½ miles in 3¾ hours=49.4 miles per hour, including two stops amounting to 8 minutes. London to Grantham, 105½ miles, in 2 hours 2 minutes=51.7, and Grantham to Wakefield, 70¼ miles, in 1 hour 17 minutes=54.7 miles per hour. This last is claimed to be the fastest run in the world. My timing of the Scotch express, the fastest train from London on the North-Western Railway, may be interesting. Before reaching Rugby 76½ miles had been traversed in 1 hour 35½ minutes=48 miles per hour. Crewe, 158 miles, was reached in 3 hours 39 minutes (beating the best Continental time for that distance), and Liverpool, 202 miles, in 5 hours, including six stops= 45 miles per hour. A few miles were run in 61 seconds, which is at the rate of 59 miles per hour.

If the speed of American trains continues to improve during the next few years as much as it has recently, we may soon hope to outstrip England, where the maximum has been apparently reached. In fact Mr. Le Van has prophesied that within five years the distance from Philadelphia to Jersey City will be accomplished in one hour, but this seems rather too much to expect, especially on a road running through such a populous district.

HYDRAULIC TABLES BASED ON KUTTER'S FORMULA.

By P. J. FLYNN, C. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

SECOND PAPER.

EGG SHAPED SEWERS.—INTERNAL DIMENSIONS.

Depth of vertical diameter is 1.5 times the greatest transverse diameter; that is, the diameter of top of arch.

Let D=greatest transverse diameter, that is the diameter of top or arch$=\frac{2H}{3}$, then

H=depth of sewer or vertical diameter=1.5D.

B=radius of bottom or invert$=\frac{H}{6}$.

R=radius of sides=H.

By reference to column $c\sqrt{r}$ in Tables 3 and 4 it will be seen that the mean velocity of an egg-shaped sewer flowing two-thirds full is always *greater* than that of the mean velocity of same sewer flowing full. When the slopes are equal, columns $c\sqrt{r}$ and $ac\sqrt{r}$ give a ready means for comparing velocities and discharges.

APPLICATION AND USE OF THE TABLES.

To find the velocity and discharge in an egg-shaped sewer.

Example 10.—An egg-shaped sewer 7 feet by 10 feet 6 inches has a slope of 6 feet per mile. What is its velocity and discharge flowing full, flowing two-thirds full depth and one-third full depth?

A slope of 6 feet per mile is equal to 1 in 880, opposite to which in Table 2 the value of $\sqrt{s}$ is found to be=.03271.

In Tables 3, 4 and 5 opposite a transverse diameter of 7 feet find the values of $c\sqrt{r}$ and $ac\sqrt{r}$ and substitute them and also the value of $\sqrt{s}$ above found in formula (1) $v=c\sqrt{r}\times\sqrt{s}$.

" (5) $Q=ac\sqrt{r}\times\sqrt{s}$ and we get the following:

Full depth. $\begin{cases} v=160.2\times.03371=5.4 \text{ feet per second.} \\ Q=9015.7\times.03371=303.9 \text{ cubic feet per second.} \end{cases}$

Two-thirds depth. $\begin{cases} v=169.6\times.03371=5.72 \text{ feet per second.} \\ Q=6283.5\times.03371=211.8 \text{ cubic feet per second.} \end{cases}$

One-third depth. $\begin{cases} v=127.9\times.03371=4.31 \text{ feet per second.} \\ Q=1779.4\times.03371=59.98 \text{ cubic feet per second.} \end{cases}$

To find the dimensions of an egg-shaped sewer to replace a circular sewer.

Example 11.—A circular sewer 5 feet diameter and 4800 feet long has a fall of 16 feet. It is to be removed and replaced by an egg-shaped sewer with a fall of 8 feet whose discharge flowing full shall equal that of the circular sewer flowing full? Give dimensions of egg-shaped sewer.

A fall of 16 in $4800=1$ in 300 and in Table 2 the $\sqrt{s}$ corresponding to this is .057735. In Table 1 opposite 5 feet diameter the value $ac\sqrt{r}$ is 2272.7, substitute this value and also the value of $\sqrt{s}$ in formula (5) $Q=ac\sqrt{r}\times\sqrt{s}$ and we have $Q=2272.7\times.057735=131.21$ cubic feet per second, the discharge of the circular sewer. The egg-shaped sewer is to have a fall of 8 in $4800=1$ in 600, and in Table 2 the equivalent $\sqrt{s}$ is 040825, substitute this value and also the discharge found above in

formula (7) $ac\sqrt{r}=\dfrac{Q}{\sqrt{s}}=\dfrac{131.21}{.040825}=$ 3213.9.

In Table 3, the nearest value of $ac\sqrt{r}$ to this is 3353 opposite a transverse diameter 4 feet 10 inches, therefore the egg-shaped sewer is to be 4 feet 10 inches by 7 feet 3 inches.

To find the diameter of a circular sewer whose discharge flowing full shall equal that of an egg-shaped sewer flowing one-third full depth.

Example 12.—Find the diameter of a circular sewer whose discharge flowing full shall equal that of the egg-shaped sewer in last example flowing one-third full the slope being the same in each.

In Table 5 and opposite transverse diameter 4 feet 10 inches the value of $ac\sqrt{r}$ $=657.53$.

In Table 1 the value of $ac\sqrt{r}$ nearest to this is found to be 661.77 opposite a diameter of 3 feet 2 inches, which is the diameter of the circular sewer required.

To find the diameter of a circular sewer whose velocity flowing full shall equal that of an egg-shaped sewer flowing one-third full depth.

Example 13.—What is the diameter of a circular sewer whose mean velocity flowing full shall equal that of an egg-shaped sewer 4 feet by 6 feet flowing one-third full, the grade in each being the the same?

In Table 5 and opposite the transverse diameter 4 feet the value of $c\sqrt{r}=86.61$.

In Table 1 the value of $c\sqrt{r}$ nearest to this is 87.15, opposite diameter 3 feet 4 inches, which is the diameter of the circular sewer required.

To find the dimensions and slope of an egg-shaped sewer flowing full, the mean velocity and discharge being given.

Example 14.—An egg-shaped sewer flowing full is to have a mean velocity not greater than 5 feet per second, and is to discharge 108 cubic feet per second. What is size and slope?

By formula (6) $a=\dfrac{Q}{v}$ substitute values of Q and v given and $a=\dfrac{108}{5}=21.6$ square feet.

In column 2 of Table 3 the nearest area to this is 21.556 opposite the transverse diameter 4 feet 4 inches, therefore the sewer required is 4 feet 4 inches by 6 feet 6 inches. At the same time the value of $ac\sqrt{r}$ opposite 4 feet 4 inches diameter is found equal to 2501.4, substitute this and also value of

formula (8) $\sqrt{s}=\dfrac{Q}{ac\sqrt{r}}=\dfrac{108}{2501.4}=$ .043176, and in Table 2 the nearest value of $\sqrt{s}$ to this is .043193 opposite the slope of 1 in 536, which is slope of sewer.

The diameter and slope of a circular sewer being given, to find dimensions and slope of an egg-shaped sewer whose discharge flowing two-thirds depth shall equal that of the circular sewer flowing full and whose velocity at same depth shall not exceed a certain rate.

Example 15.—A circular sewer 6 feet in diameter and with a slope of 1 in 600 is to be removed and to be replaced by an egg-shaped sewer whose discharge flowing at two-thirds of its full depth shall be equal to that of the circular sewer flowing full and whose mean velocity at the same two-thirds depth shall not exceed 5 feet per second? Give dimensions and slope of egg-shaped sewer.

In Table 1 and opposite 6 feet diameter the value of $ac\sqrt{r}$ is 3702 3, and in Table 2 opposite 1 in 600 the value of $\sqrt{s}$ is .040825, substitute these values in formula (5) $Q=ac\sqrt{r}\times\sqrt{s}$ and we get

$Q=3702.3\times.040825=151.15$ cubic feet per second, the discharge of the circular sewer. Now substitute this discharge and the velocity above given 5 feet per second in formula (6) $a=\dfrac{Q}{v}$ and

we get $a=\frac{151.15}{5}=30.23$ square feet, the area at two-thirds depth of the egg-shaped sewer. In column 2 of Table 4 the nearest area to this is 30.317 opposite a transverse diameter of 6 feet 4 inches, therefore the dimensions of egg-shaped sewer are 6 feet 4 inches by 9 feet 6 inches.

At the same time take out the value of $ac\sqrt{r}$ opposite 6 feet 4 inches, which is 4811.9. Substitute this and also the value of Q found in formula (8)

$$\sqrt{s}=\frac{Q}{ac\sqrt{r}}=.031412$$

and this not being found in Table 1, square each side and

$$s=.0009867,$$

and $\frac{1}{.0009867}=1013$ nearly, therefore the slope of egg-shaped sewer is 1 in 1013 and its size 6 feet 4 inches by 9 feet 6 inches.

To find the dimensions and grade of an egg-shaped sewer to have a certain discharge flowing full, and whose mean velocity shall not exceed a certain rate when flowing two-thirds full depth.

Example 16.—An egg-shaped sewer is to discharge 110 cubic feet per second flowing full and its mean velocity flowing two-thirds full depth is not to exceed 5 feet per second? Find its dimensions and slope.

As a first approximation assume the velocity flowing full at 5 feet per second, then $\frac{110}{5}=22$ square feet the area of egg-shaped sewer flowing full, and in Table 3 opposite this area the transverse diameter 4 feet 4 inches is found. Now with this diameter

the value of $c\sqrt{r}$ full depth $=116.0$
the value of $c\sqrt{r}$ two-thirds depth $=123.1$

therefore we may assume that the velocity of sewer flowing full is for 4 feet 4 inches, transverse diameter about 6 per cent. less than when flowing two-thirds full, that is, assuming the velocity at two-thirds depth 5 feet per second, the velocity at full depth will be about 4.7 feet per second. Substituting this velocity and also discharge in formula (6) $a=\frac{Q}{v}=\frac{110}{4.7}=23.4$ the area of egg-shaped sewer flowing full. In Table 3 the transverse diameter opposite this is 4 feet 6 inches, which is the diameter required of the egg-shaped sewer. At the same time that diameter is found look out the value of $ac\sqrt{r}$ which is 2770, substitute this in

$$\text{formula (8)}\;.\;\sqrt{s}=\frac{Q}{ac\sqrt{s}}$$
$$=\frac{110}{2770}=.039711.$$

In Table 2 the $\sqrt{s}$ nearest to this is .039715 opposite a slope of 1 in 634, therefore the dimensions of egg-shaped sewer are 4 feet 6 inches by 6 feet 9 inches and its slope 1 in 634.

Now in Table 4 the value of $c\sqrt{r}$ opposite transverse diameter of 4 feet 6 inches is 126.3, substitute this and also value of $\sqrt{s}$ above found in

formula (1) $v=c\sqrt{r}\times\sqrt{s}$ and we have
$v=126.3\times.039711=5$ feet per second, the mean velocity of sewer flowing two-thirds full.

TABLE 2—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance$=\frac{h}{l}$.

Slope 1 in	s=side of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
2640	.000378787	.019463	2280	.000438597	.020943
2600	.000384615	.019612	2240	.000446429	.021129
2560	.000390625	.019764	2200	.000454545	.021320
2520	.000396825	.019920	2160	.000462963	.021517
2480	.000403226	.020080	2120	.000471698	.021719
2440	.000409836	.020244	2080	.000480769	.021927
2400	.000416666	.020412	2040	.000490196	.022140
2360	.000423729	.020585	2000	.000500000	.022361
2320	.000431034	.020761	1980	.000505050	.022473

TABLE 2 (*Continued*).—GIVING VALUES OF s AND $\sqrt{s}$.

h: -sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
1960	.000510204	.022588	1080	.000925926	.030429
1940	.000515464	.022704	1070	.000934579	.030571
1920	.000520833	.022822	1060	.000943396	.030715
1900	.000526316	.022942	1050	.000952381	.030861
1880	.000531915	.023063	1040	.000961538	.031009
1860	.000537634	.023187	1030	.000970873	.031159
1840	.000543478	.023313	1020	.000980392	.031311
1820	.000549450	.023440	1010	.000990099	.031466
1800	.000555555	.023570	1000	.001000000	.031623
1780	.000561798	.023702	999	.001001001	.031639
1760	.000568182	.023836	998	.001002004	.031654
1740	.000574712	.023973	997	.001003009	.031670
1720	.000581395	.024112	996	.001004016	.031686
1700	.000588235	.024254	995	.001005025	.031702
1680	.000595238	.024398	994	.001006036	.031718
1660	.000602409	.024744	993	.001007049	.031734
1640	.000609756	.024693	992	.001008065	.031750
1620	.000617284	.024845	991	001009082	.031766
1600	.000625000	.025000	990	.001010101	.031782
1580	.000632911	.025158	989	.001011122	.031798
1560	.000641025	.025318	988	.001012146	.031814
1540	.000649351	.025482	987	.001013171	.031830
1520	.000657895	.025649	986	.001014199	.031847
1500	.000666666	.025820	985	.001015228	.031863
1480	.000675675	.025994	984	.001016260	.031879
1460	.000684932	.026171	983	.001017294	.031895
1440	.000694444	.026352	982	.001018330	.031911
1420	.000704225	.026537	981	.001019368	.031928
1400	.000714286	.026726	980	.001020408	.031944
1380	.000724638	.026919	979	.001021450	.031960
1360	.000735294	.027116	978	.001022495	.031977
1340	.000746268	.027318	977	.001023541	.031993
1320	.000757576	.027524	976	.001024590	.032009
1300	.000769231	.027735	975	.001025641	.032026
1280	.000781250	.027951	974	.001026694	.032042
1260	.000793651	.028172	973	.001027749	.032059
1240	.000806452	.028398	972	.001028807	.032075
1220	.000819672	.028630	971	.001029866	.032091
1200	.000833333	.028868	970	.001030928	.032108
1190	.000840336	.028988	969	.001031992	.032125
1180	.000847458	.029111	968	.001033058	.032141
1170	.000854701	.029235	967	.001034126	.032158
1160	.000862069	.029361	966	.001035197	.032174
1150	.000869566	.029488	965	.001036269	.032191
1140	.000877193	.029617	964	.001037344	.032208
1130	.000884956	.029748	963	.001038422	.032224
1120	.000892857	.029881	962	.001039501	.032241
1110	.000900900	.030015	961	.001040583	.032258
1100	.000909090	.030151	960	.001041667	.032275
1090	.000917431	.030289	959	.001042753	.032292

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=full of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
958	.001043841	.032309	908	.001101322	.033186
957	.001044932	.032325	907	.001102536	.033204
956	.001046025	.032342	906	.001103753	.033223
955	.001047120	.032359	905	.001104972	.033241
954	.001048218	.032376	904	.001106195	.033259
953	.001049318	.032393	903	.001107420	.033278
952	.001050420	.032410	902	.001108647	.033296
951	.001051525	.032427	901	.001109878	.033315
950	.001052632	.032444	900	.001111111	.033333
949	.001053741	.032461	899	.001112347	.033352
948	.001054852	.032479	898	.001113586	.033370
947	.001055966	.032496	897	.001114827	.033389
946	.001057082	.032513	896	.001116071	.033408
945	.001058201	.032530	895	.001117318	.033426
944	.001059322	.032547	894	.001118568	.033445
943	.001060445	.032565	893	.001119821	.033464
942	.001061571	.032582	892	.001121076	.033483
941	.001062699	.032599	891	.001122334	.033501
940	.001063830	.032616	890	.001123596	.033520
939	.001064963	.032634	889	.001124859	.033539
938	.001066098	.032651	888	.001126126	.033558
937	.001067236	.032669	887	.001127396	.033577
936	.001068376	.032686	886	.001128668	.033595
935	.001069519	.032703	885	.001129944	.033614
934	.001070664	.032721	884	.001131222	.033633
933	.001071811	.032738	883	.001132503	.033653
932	.001072961	.032756	882	.001133787	.033672
931	.001074114	.032774	881	.001135074	.033691
930	.001075269	.032791	880	.001136364	.033710
929	.001076426	.032809	879	.001137656	.033729
928	.001077586	.032826	878	.001138952	.033748
927	.001078749	.032844	877	.001140251	.033768
926	.001079914	.032862	876	.001141553	.033787
925	.001081081	.032879	875	.001142857	.033806
924	.001082251	.032897	874	.001144165	.033825
923	.001083423	.032915	873	.001145475	.033845
922	.001084599	.032933	872	.001146789	.033864
921	.001085776	.032951	871	.001148106	.033883
920	.001086957	.032969	870	.001149425	.033903
919	.001088139	.032987	869	.001150748	.033923
918	.001089325	.033005	868	.001152074	.033942
917	.001090513	.033023	867	.001153403	.033962
916	.001091703	.033041	866	.001154734	.033981
915	.001092896	.033059	865	.001156069	.034001
914	.001094092	.033077	864	.001157407	.034021
913	.001095290	.033095	863	.001158749	.034040
912	.001096491	.033113	862	.001160093	.034060
911	.001097695	.033131	861	.001161440	.034080
910	.001098901	.033149	860	.001162791	.034099
909	.001100110	.033168	859	.001164144	.034119

TABLE 2 *(Continued)*—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
858	.001165501	.034139	808	.001237624	.035179
857	.001166861	.034159	807	.001239157	.035201
856	.001168224	.034179	806	.001240695	.035223
855	.001169591	.034199	805	.001242236	.035245
854	.001170960	.034219	804	.001243781	.035267
853	.001172333	.034239	803	.001245330	.035289
852	.001173709	.034259	802	.001246883	.035311
851	.001175088	.034279	801	.001248439	.035333
850	.001176471	.034300	800	.001250000	.035355
849	.001177856	.034320	799	.001251564	.035377
848	.001179245	.034340	798	.001253133	.035399
847	.001180638	.034360	797	.001254705	.035422
846	.001182033	.034381	796	.001256281	.035444
845	.001183432	.034401	795	.001257862	.035466
844	.001184834	.034421	794	.001259446	.035489
843	.001186240	.034442	793	.001261034	.035511
842	.001187648	.034462	792	.001262626	.035533
841	.001189061	.034483	791	.001264223	.035556
840	.001190476	.034503	790	.001265823	.035578
839	.001191895	.034524	789	.001267427	.035601
838	.001193317	.034544	788	.001269036	.035623
837	.001194743	.034565	787	.001270648	.035646
836	.001196172	.034586	786	.001272265	.035669
835	.001197605	.034606	785	.001273885	.035691
834	.001199041	.034627	784	.001275510	.035714
833	.001200480	.034648	783	.001277139	.035737
832	.001201923	.034669	782	.001278772	.035760
831	.001203369	.034689	781	.001280410	.035783
830	.001204819	.034710	780	.001282051	.035806
829	.001206273	.034731	779	.001283697	.035829
828	.001207729	.034752	778	.001285347	.035852
827	.001290190	.034773	777	.001287001	.035875
826	.001210654	.034794	776	.001288660	.035898
825	.001212121	.034816	775	.001290323	.035921
824	.001213592	.034837	774	.001291990	.035944
823	.001215067	.034858	773	.001293661	.035967
822	.001216545	.034879	772	.001295337	.035991
821	.001218027	.034900	771	.001297017	.036014
820	.001219512	.034922	770	.001298701	.036038
819	.001221001	.034943	769	.001300390	.036061
818	.001222494	.034964	768	.001302083	.036084
817	.001223990	.034985	767	.001303781	.036108
816	.001225490	.035007	766	.001305483	.036131
815	.001226994	.035028	765	.001307190	.036155
814	.001228501	.035050	764	.001308901	.036179
813	.001230012	.035071	763	.001310616	.036202
812	.001231527	.035093	762	.001312336	.036226
811	.001233046	.035115	761	.001314060	.036250
810	.001234568	.035136	760	.001315789	.036274
809	.001236094	.035158	759	.001317523	.036298

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any distance (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
758	.001319261	.036322	708	.001412429	.037582
757	.001321004	.036346	707	.001414427	.037609
756	.001322751	.036370	706	.001416431	.037636
755	.001324503	.036394	705	.001418440	.037662
754	.001326260	.036418	704	.001420455	.037689
753	.001328021	.036442	703	.001422475	.037716
752	.001329787	.036466	702	.001424501	.037743
751	.001331558	.036490	701	.001426534	.037769
750	.001333333	.036515	700	.001428571	.037796
749	.001335113	.036539	699	.001430615	.037824
748	.001336898	.036563	698	.001432665	.037851
747	.001338688	.036588	697	.001434720	.037878
746	.001340483	.036613	696	.001436782	.037905
745	.001342282	.036637	695	.001438849	.037932
744	.001344086	.036662	694	.001440922	.037959
743	.001345895	.036686	693	.001443001	.037987
742	.001347709	.036711	692	.001445087	.038014
741	.001349528	.036736	691	.001447178	.038042
740	.001351351	.036761	690	.001449275	.038069
739	.001353180	.036786	689	.001451379	.038097
738	.001355014	.036810	688	.001453488	.038125
737	.001356852	.036835	687	.001455604	.038152
736	.001358696	.036860	686	.001457726	.038180
735	.001360544	.036885	685	.001459854	.038208
734	.001362398	.036911	684	.001461988	.038236
733	.001364256	.036936	683	.001464129	.038264
732	.001366120	.036961	682	.001466276	.038292
731	.001367989	.036986	681	.001468429	.038320
730	.001369863	.037012	680	.001470588	.038348
729	.001371742	.037037	679	.001472754	.038376
728	.001373626	.037063	678	.001474926	.038405
727	.001375516	.037088	677	.001477105	.038433
726	.001377410	.037113	676	.001479290	.038461
725	.001379310	.037139	675	.001481481	.038490
724	.001381215	.037164	674	.001483680	.038518
723	.001383126	.037190	673	.001485884	.038547
722	.001385042	.037216	672	.001488095	.038576
721	.001386963	.037242	671	.001490313	.038604
720	.001388889	.037268	670	.001492537	.038633
719	.001390821	.037294	669	.001494768	.038662
718	.001392758	.037320	668	.001497006	.038691
717	.001394700	.037346	667	.001499250	.038720
716	.001396648	.037372	666	.001501502	.038749
715	.001398601	.037398	665	.001503759	.038778
714	.001400560	.037424	664	.001506024	.038808
713	.001402525	.037450	663	.001508296	.038837
712	.001404494	.037477	662	.001510574	.038866
711	.001406470	.037503	661	.001512859	.038895
710	.001408451	.037529	660	.001515152	.038925
709	.001410437	.037556	659	.001517451	.038954

TABLE 2 (*Continued*)—GIVING VALUES OF s AND $\sqrt{s}$.

s=sine of slope=fall of water surface (h) in any diameter (l), divided by that distance=$\frac{h}{l}$.

Slope 1 in	s=sine of slope.	$\sqrt{s}$.	Slope 1 in	s=sine of slope.	$\sqrt{s}$.
658	.001519757	.038984	608	.001644737	.040555
657	.001522070	.039013	607	.001647446	.040589
656	.001524390	.039043	606	.001650165	.040622
655	.001526718	.039073	605	.001652893	.040656
654	.001529052	.039103	604	.001655629	.040689
653	.001531394	.038133	603	.001658375	.040723
652	.001533742	.039163	602	.001661130	.040757
651	001536098	.039193	601	.001663894	.040791
650	.001538462	.039223	600	.001666667	.040825
649	.001540832	.039253	599	.001669449	.040859
648	.001543210	.039284	598	.001672241	.040893
647	.001545595	.039314	597	.001675042	.040927
646	.001547988	.039344	596	.001677852	.040961
645	.001550388	.039375	595	.001680672	.040996
644	.001552795	039405	594	.001683502	.041031
643	.001555210	.039436	593	.001686341	.041065
642	.001557632	.039467	592	.001689189	.041100
641	.001560062	.039498	591	.001692047	.041135
640	.001562500	.039528	590	.001694915	.041169
639	.001564945	.039559	589	.001697793	.041204
638	.001567398	.039590	588	.001700680	.041239
637	.001569859	.039621	587	.001703578	.041274
636	.001572327	.039653	586	.001706485	.041309
635	.001574803	.039684	585	.001709420	.041345
634	.001577287	.039715	584	.001712329	.041380
633	.001579779	.039746	583	.001715266	.041416
632	.001582278	.039778	582	.001718213	.041451
631	.001584786	.039809	581	.001721170	.041487
630	.001587302	.039841	580	.001724138	.041523
629	.001589825	.039873	579	.001727116	.041559
628	.001592357	.039904	578	.001730104	.041594
627	.001594896	.039936	577	.001733102	.041630
626	.001597444	.039968	576	.001736111	.041667
625	.001600000	.040000	575	.001739130	.041703
624	.001602564	.040032	574	.001742160	.041739
623	.001605136	.040064	573	.001745201	.041776
622	.001607717	.040096	572	.001748252	.041812
621	.001610306	.040128	571	.001751313	.041848
620	.001612903	.040161	570	.001754386	.041885
619	.001615509	.040193	569	.001757469	.041922
618	.001618123	.040226	568	.001760563	.041959
617	.001620746	.040258	567	.001763668	.041996
616	.001623377	.040291	566	.001766784	.042033
615	.001626016	.040324	565	.001769912	.042070
614	.001628664	.040357	564	.001773050	.042108
613	.001631321	.040389	563	.001776199	.042145
612	.001633987	.040422	562	.001779359	.042183
611	.001636661	.040456	561	.001782531	.042220
610	.001639344	.040489	560	.001785714	.042258
609	.001642036	.040522	559	.001788909	.042295

TABLE 3.—GIVING VALUES OF a AND r AND ALSO THE FACTORS $c\sqrt{r}$ AND $ac\sqrt{r}$ FOR CORRESPONDING TRANSVERSE DIAMETERS OF EGG-SHAPED SEWERS FLOWING *full depth* GIVEN IN FIRST COLUMN.

These factors are to be used only where the value of n, that is the coefficient of roughness of lining of channel = .015, as in second-class or rough-faced brickwork, well dressed stone work, foul and slightly tuberculated iron, cement and terra-cotta pipes with imperfect joints and in bad order.

Area of egg-shaped sewer flowing full depth = $D^2 \times 1.148525$.
Perimeter of egg-shaped sewer flowing full depth = $D \times 3.9649$.
Hydraulic mean depth of egg-shaped sewer flowing full depth = $D \times 0.2897$.

$$v = c\sqrt{r} \times \sqrt{s}. \quad Q = av = ac\sqrt{r} \times \sqrt{s}.$$

D = transverse diam. ft. in.	a = area in square ft.	r = hydraulic mean depth in feet.	For velocity. $c\sqrt{r}$	For discharge $ac\sqrt{r}$	D = transverse diam. ft. in.	a = area in square ft.	r = hydraulic mean depth in feet.	For velocity. $c\sqrt{r}$	For discharge $ac\sqrt{r}$
1 0	1.148	.2897	39.62	45.528	5 2	30.660	1.497	130.7	4007.9
1 2	1.563	.3380	44.66	69.804	5 4	32.669	1.545	133.6	4364.9
1 4	2.041	.3864	49.57	101.17	5 6	34.743	1.593	136.4	4738.0
1 6	2.584	.4345	54.08	139.74	5 8	36.880	1.642	139.2	5131.7
1 8	3.190	.4828	58.64	187.06	5 10	39.081	1.690	142.0	5548.0
1 10	3.860	.5311	62.83	242.52	6 0	41.347	1.738	144.6	5980.3
2 0	4.594	.5794	66.93	307.48	6 2	43.676	1.787	147.3	6435.1
2 2	5.391	.6277	71.01	382.81	6 4	46.068	1.835	149.8	6902.6
2 4	6.253	.6760	74.93	468.54	6 6	48.525	1 883	152.5	7399.3
2 6	7.178	.7242	78.76	565.34	6 8	51.046	1.931	155.2	7920.6
2 8	8.167	.7725	82.44	673.29	6 10	53.629	1.980	157.7	8547.1
2 10	9.220	.8208	86.21	794.86	7 0	56.278	2.028	160.2	9015.7
3 0	10.337	.8691	89.70	927.23	7 4	61.764	2.124	165.0	10192
3 2	11.517	.9174	93.25	1074.0	7 8	67.508	2.221	170.1	11482
3 4	12.761	.9657	96.73	1234.4	8 0	73.506	2.318	174.8	12852
3 6	14.069	1.014	100.1	1407.6	8 4	79.758	2.414	179.6	14327
3 8	15.442	1.062	103.4	1596.7	8 8	86.268	2.511	184.3	15898
3 10	16.877	1.111	106.6	1799.1	9 0	93.030	2.607	188.8	17563
4 0	18.376	1.159	109.9	2019.5	9 4	100.049	2.704	193.1	19323
4 2	19.940	1.207	113.0	2254.0	9 8	107.324	2.800	197.5	21198
4 4	21.566	1.255	116.0	2501.4	10 0	114.853	2.897	201.9	23191
4 6	23.258	1.304	119.1	2770.0	10 6	126.625	3.042	208.3	26376
4 8	25.013	1.352	122.1	3053.8	11 0	138.972	3.187	214.6	29822
4 10	26.830	1.400	125.0	3353.0	12 0	165.388	3.476	226.8	37502
5 0	28.713	1.449	128.0	3675.6					

Table 4.—Giving Values of a and r and also the Factors $c\sqrt{r}$ and $ac\sqrt{r}$ for Corresponding Diameters of Egg Shaped Sewers Flowing *two-thirds full depth* given in First Column.

These factors are to be used only where the value of n, that is the coefficient of roughnes of lining of channel = .015 as in second class or rough-faced brickwork, well-dressed stone work, foul and slightly tuberculated iron, cement and terra-cotta pipes with imperfect joints and in bad order.

Area of section of egg-shaped sewer flowing two-thirds full depth = $D^2 \times 0.755825$.
Perimeter of section of egg-shaped sewer flowing two-thirds full depth = $D \times 2.3941$.
Hydraulic mean depth of section of egg-shaped sewer flowing two-thirds full depth = $D \times 0.3157$.

$$v = c\sqrt{r} \times \sqrt{s}. \quad Q = av = ac\sqrt{r} \times \sqrt{s}.$$

D = transverse diam. ft. in.	a = area in square ft.	r = hydraulic mean depth in feet.	For velocity. $c\sqrt{r}$	For discharge. $ac\sqrt{r}$	D = transverse diam. ft. in.	a = area in square ft.	r = hydraulic mean depth in feet.	For velocity. $c\sqrt{r}$	For discharge $ac\sqrt{r}$
1 0	0.756	0.316	42.40	32.048	5 2	20.177	1.631	138.6	2795.9
1 2	1.029	0.368	47.80	49.181	5 4	21.498	1.684	141.7	3045.5
1 4	1.344	0.421	52.82	70.993	5 6	22.863	1.736	144.6	3305.3
1 6	1.701	0.474	57.68	98.115	5 8	24.270	1.789	147.5	3578.9
1 8	2.099	0.526	62.46	131.10	5 10	25.718	1.842	150.3	3864.8
1 10	2.540	0.579	66.94	170.02	6 0	27.210	1.894	153.1	4165.3
2 0	3.023	0.631	71.42	216.54	6 2	28.743	1.947	155.9	4481.6
2 2	3.548	0.684	75.59	268.19	6 4	30.317	1.999	158.7	4811.9
2 4	4.115	0.737	79.69	327.93	6 6	31.933	2.052	161.5	5158.5
2 6	4.724	0.789	83.90	396.32	6 8	33.592	2.095	164.2	5516.6
2 8	5.375	0.842	87.82	472.01	6 10	35.292	2.157	166.9	5891.0
2 10	6.067	0.894	91.60	555.74	7 0	37.035	2.210	169.6	6283.5
3 0	6.802	0.947	95.33	648.40	7 4	40.646	2.315	174.8	7106.8
3 2	7.579	1.000	99.10	751.08	7 8	44.426	2.420	179.9	7993.0
3 4	8.398	1.052	102.7	862.41	8 0	48.373	2.526	184.9	8944.0
3 6	9.259	1.105	106.2	983.24	8 4	52.487	2.631	189.8	9964.1
3 8	10.161	1.158	109.7	1115.1	8 8	56.771	2.736	194.6	11050
3 10	11.106	1.210	113.2	1256.1	9 0	61.222	2.841	199 5	12213
4 0	12.093	1.263	116.5	1409.4	9 4	65 840	2.947	204.2	13444
4 2	13.123	1.315	119.8	1572.1	9 8	70.628	3.052	208.7	14743
4 4	14.192	1.368	123.1	1746.9	10 0	75.583	3.157	213.3	16125
4 6	15.305	1.421	126.3	1932.7	10 6	83.330	3.315	220.1	18342
4 8	16.460	1.473	129.4	2130.5	11 0	91.455	3.473	226.8	20738
4 10	17.656	1.526	132.5	2338.6	12 0	108.839	3.788	239.4	26060
5 0	18.895	1.579	135.5	2560.3					

TABLE 5.—GIVING VALUES OF a AND r AND ALSO THE FACTORS $c\sqrt{r}$ AND $ac\sqrt{r}$ FOR CORRESPONDING DIAMETERS OF EGG-SHAPED SEWERS FLOWING *one-third full depth* GIVEN IN FIRST COLUMN.

These factors are to be used only where the value of n, that is the coefficient of roughness of lining of channel = 015 as in second-class or rough-faced brickwork, well dressed stone work, foul and slightly tuberculated iron, cement and terra-cotta pipes with imperfect joints and in bad order.

Area of section of egg-shaped sewers flowing one-third full depth = $D^2 \times 0.284$.
Perimeter of section of egg-shaped sewer flowing one-third full depth = $D \times 1\ 3747$.
Hydraulic mean depth of section of egg-shaped sewers flowing one-third full depth = $D \times 0.2066$.

$$v = c\sqrt{r} \times \sqrt{s}.\quad Q = av = ac\sqrt{r} \times \sqrt{s}.$$

D = transverse diam. ft. in.	a = area in square ft.	r = hydraulic mean depth in feet.	For velocity. $c\sqrt{r}$	For discharge $ac\sqrt{r}$	D = transverse diam. ft. in.	a = area in square ft.	r = hydraulic mean depth in feet.	For velocity. $c\sqrt{r}$	For discharge $ac\sqrt{r}$
1 0	0.284	0.207	30.41	8.637	5 2	7.581	1.068	103.7	785.86
1 2	0.387	0.241	34.38	13.303	5 4	8.078	1.102	106.1	856.67
1 4	0.505	0.276	38.16	19.269	5 6	8.591	1.136	108.3	930.54
1 6	0.639	0.310	42.23	26.98(	5 8	9.120	1.171	110.6	1008.7
1 8	0.789	0.344	45.39	35.815	5 10	9.664	1.205	112.9	1091.0
1 10	0.955	0.379	48.74	46.546	6 0	10.224	1.240	115.0	1175.8
2 0	1.136	0 413	52.09	59.173	6 2	10.800	1.274	117.3	1266.4
2 2	1.333	0 448	55.29	73.696	6 4	11.391	1.309	119.4	1359.8
2 4	1.546	0.482	58.58	90.568	6 6	12.999	1.343	121.5	1458.1
2 6	1.776	0.517	61.58	109.37	6 8	12.622	1.377	123.7	1561.0
2 8	2.020	0.551	64.49	130.26	6 10	13.261	1.412	125.8	1668.8
2 10	2.280	0.585	67.46	153.80	7 0	13.916	1.446	127.9	1779.4
3 0	2.556	0.620	70.48	180.14	7 4	15.273	1.515	131.9	2014.1
3 2	2.848	0.654	73.24	208.98	7 8	16.693	1.584	135.8	2266.7
3 4	2.156	0.689	75.98	239.79	8 0	18.176	1.653	139.9	2542.7
3 6	3.479	0.723	78.63	273.54	8 4	19.722	1.722	143.7	2833.8
3 8	3.818	0.758	81.31	310.44	8 8	21.332	1.791	147.5	3146.2
3 10	4.173	0.792	84.03	350.67	9 0	23.004	1.859	151.3	3480.7
4 0	4.544	0.826	86.61	393.55	9 4	24.739	1.928	155.0	3834.7
4 2	4.931	0.861	88.98	438.75	9 8	26.538	1.997	158.6	4208.4
4 4	5.333	0.895	91.60	488.50	10 0	28.400	2.066	162.1	4604.7
4 6	5.751	0.930	94.08	541.04	10 6	31.311	2.169	167.5	5245.3
4 8	6.185	0.964	96.57	597.29	11 0	34.364	2.273	172.6	5932.1
4 10	6.635	0.999	99.10	657.53	12 0	40.892	2.479	183.1	7489.0
5 0	7.100	1.033	101.3	719.27					

DYNAMO-ELECTRIC MACHINERY.

By PROFESSOR SILVANUS P. THOMPSON, B. A., D. Sc., M. S. T. E.

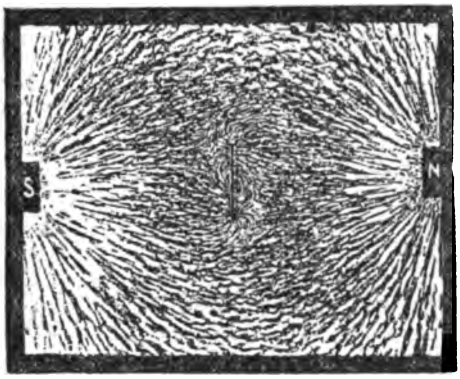
From the "Journal of the Society of Arts."

III.

The Dynamo as a Motor.

In my first lecture, I laid down the definition that dynamo-electric machinery meant "machinery for converting the energy of mechanical motion into the energy of electric currents, or *vice versa*." In the two lectures which I have already had the honor of delivering, I have treated the dynamo solely in its functions as a generator of electric currents. In this third lecture I come to the converse function of the dynamo, namely, that of converting the energy of electric currents into the energy of mechanical motion.

Fig. 46.

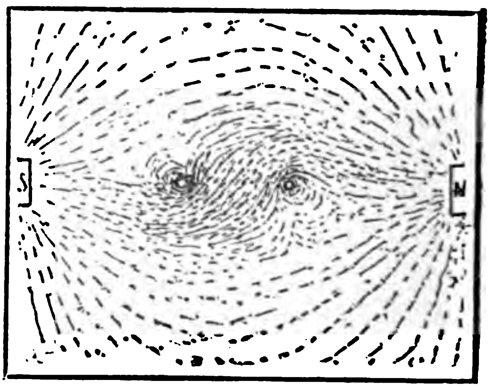


ACTION OF MAGNETIC FIELD ON A MAGNETIC NEEDLE.

An electric-motor, or, as it was formerly called, an electro magnet engine, is one which does mechanical work at the expense of electric energy; and this is true, no matter whether the magnets which form the fixed part of the machine be permanent magnets of steel or electro-magnets. Any one, in fact, of the four kinds of dynamo, can be used conversely as a motor, though, as we shall see, some more appropriately than others. But whether their field magnets be of permanently magnetized steel or of temporarily magnetized iron, all these motors are electro magnetic in principle; that is to say, there is some part either fixed or moving which is an electro-magnet, and which as such attracts and is attracted magnetically.

Every one knows that a magnet will attract the opposite pole of another magnet, and will pull it round. We know, also, that every magnet placed in a magnetic field tends to turn round and set itself along the lines of force. Let me, as a first illustration of this class of actions, exhibit to you the nature of the forces at work in the magnetic field. In the figure

Fig. 47.



ACTION OF MAGNETIC FIELD ON A WIRE CARRYING A CURRENT.

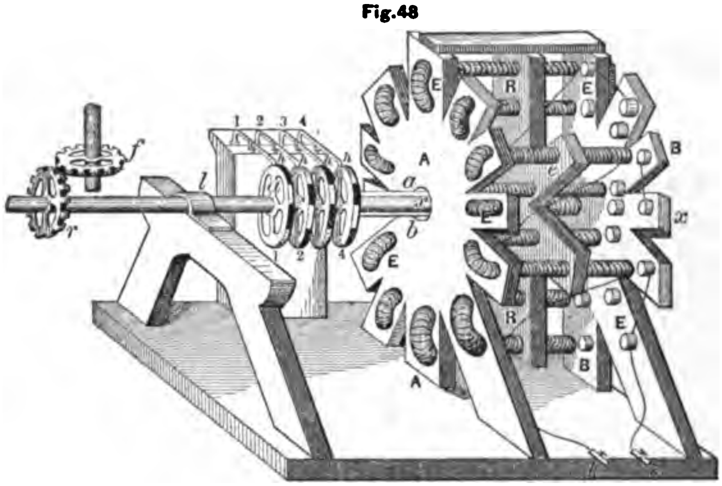
which I now throw upon the screen (Fig. 46), we have, in the first place, a simple magnetic field produced between the poles of two strong magnets, one on the right, the other on the left. Between the two, confined forcibly at right-angles to the lines of force, I hold a small magnetic needle. Iron filings sprinkled in the field reveal the actions at work in a most instructive way. Faraday, who first taught us the significance of these mysterious lines of force, has told us that we may reason about them as if they tended to contract or grow shorter. Now a simple

inspection of Fig. 46 will show that the shortening of the lines of force must have the effect of rotating the magnetic needle upon its center, through an angle of 90°, for the lines stream away on the right hand above, and on the left hand below, in a most suggestive fashion. It is not, therefore, difficult to understand that very soon after the invention of the electro-magnet, which gave us for the first time a magnet whose power was under control, a number of ingenious persons perceived that it would be possible to construct an electro-magnetic engine in which an electro-magnet, placed in a magnetic field, should be pulled round; and further, that the rotation should be kept up continuously, by reversing the current at an appropriate moment. As a matter of fact, a mere coil of wire, carrying a current, is acted upon when placed in the magnetic field, and is pulled round as a magnet is. Fig. 47 shows how, in this case, the lines of force reveal the action. The magnetic field is, as before produced, between the ends of two large magnets. The two round spots are two holes drilled in the sheet of glass, where the wire which carried the current came up through the glass and descended again. You will notice how the lines of iron filings, which would, if there were no current, run simply across from left to right, are bent out of their course. If these lines could shorten themselves, they must, of necessity, twist the loop of wire round, and cause it to set at right angles to its present position.

On this very principle was constructed the earliest electric motor of Ritchie, so well known in many forms as a stock piece of electric apparatus, but little better in reality than a toy.

A great step in advance was made by Jacobi, who, in 1838, constructed the multipolar machine, of which we give a representation in Fig. 48. This motor, which Jacobi designed for his electric boat, had two strong wooden frames, A and B, in each of which a dozen electro-magnets were fixed, their poles being set alternately. Between them, upon a wooden disk, were placed another set of electro-magnets, which, by the alternate attraction and repulsion of the fixed poles, were kept in rotation, the current which traversed the rotating magnets being regularly reversed at the moment of passing the poles of the fixed magnets by means of a commutator, consisting, according to Jacobi's directions, of four brass-toothed wheels, having pieces of ivory or wood let in between the teeth for insulation. Jacobi's motor is, in fact, a very advanced type of dynamo, and differs very little in point of design from one of Wilde's most successful forms.*

Fig.48



JACOBI'S ELECTRIC MOTOR (1838.)

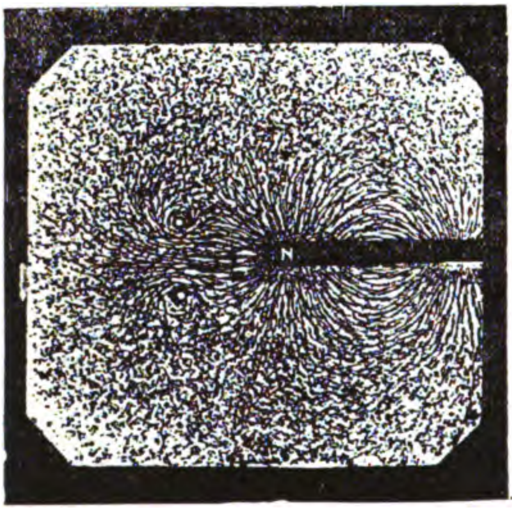
* Wilde's is, however, designed as a generator. Jacobi's, on the contrary, was designed as a motor; though, of course, it would generate currents if driven round by mechanical power.

A still earlier rotating apparatus, and, like Ritchie's motor, a mere toy, was Barlow's wheel, Fig. 43, described in 1823. This instrument, interesting as being the forerunner of Faraday's disk dynamo, is the representative of an important class of machines, namely, those which have a sliding contact merely, and need no commutator.

A fourth class of motors may be named, wherein the moving part, instead of rotating upon an axis, is caused to oscillate backwards and forwards. Professor Henry, to who we owe so much in the early history of electro-magnetism, constructed, in 1831, a motor with an oscillating beam, alternately drawn backwards and forwards by the intermittent action of an electro magnet. Dal Negro's motor of 1833 was of this class; in it a steel rod was caused to oscillate between the poles of an electro-magnet, and caused a crank, to which it was geared, to rotate in consequence. A distinct improvement in this type of machine was introduced by Page, who employed hollow coils or bobbins as electro-magnets, which, by their alternate action, sucked down iron cores into the coils, and caused them to oscillate to and fro. Motors of this kind form an admirable illustration of one of the laws of electro-magnetics, first formulated by Gauss, but developed later by Maxwell, to the effect that a circuit acts on a magnetic pole in such a way as to make the number of magnetic lines of force that pass through the circuit a maximum. Once more I have recourse to iron filings to illustrate this abstract proposition of electric geometry.

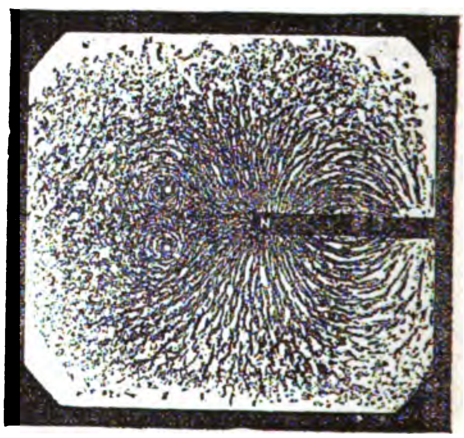
In the figure before you (Fig. 49), the north pole of a bar magnet is placed opposite a circuit or loop of wire traversed by a current, and which comes up through the glass at the lower hole, and descends at the upper hole. The tendency to draw as many as possible of the magnet's lines of force into the embrace of the circuit is unmistakable. If now we reverse the current, what do we find? Fig. 50 supplies the answer; for now we find that the magnet's lines of force, instead of being drawn in, are pushed out. In fact, in one case, the pole is attracted, in the other repelled.

Fig. 49.



POLE OF MAGNET ATTRACTED INTO A CIRCUIT TRAVERSED BY A CURRENT.

Fig. 50.



POLE OF MAGNET REPELLED OUT OF THE CIRCUIT WHEN THE CURRENT IS REVERSED.

Page's suggestion was further developed by Bourbouze, who constructed the curious motor which looks uncommonly like an old type of steam-engine. We have here a beam, a crank, fly-wheel, connecting-rod, and even an eccentric valve-gear and a slide valve. But for cylinders we have four hollow electro-magnets; for pistons we have iron cores, that are

alternately sucked in and repelled out; and for slide valve we have a commutator, which, by dragging a pair of platinum-tipped springs over a flat surface made of three pieces of brass separated by two insulating strips of ivory, reverses at every stroke the direction of the currents in the coils of the electro-magnets. It is really a very ingenious machine, but, in point of efficiency, far behind many other electric motors. Unfortunately, it does not do to design dynamo-electric machinery on the same lines as steam-engines.

Yet, a fifth class of electric-motors owes its existence to Froment, who, fixing a series of parallel iron bars upon the periphery of a drum, caused them to be attracted, one after the other, by an electro-magnet or electro-magnets, and thus procured a continuous rotation.

Lastly, of the various types of motors we may enumerate a class in which the rotating portion is enclosed in an eccentric frame of iron, so that as it rotates, it gradually approaches nearer. Little motors, working on this principle of "oblique approach," were invented by Wheatstone, and have long been used for spinning Geissler tubes, and other light experimental work. More recently, Trouvé and Weisendanger have sought to embody this principle in motors of more ambitious proportions, but without securing any great advantage.

It would be impossible, within the limits of a lecture, to deal with a tithe of all the various stages of discovery and invention, and if it were my intention to deal with the subject from the historical point of view, I might speak of many interesting and curious machines that have from time to time been tried. I might tell you how Page, after inventing his machine in 1834, succeeded, in 1852, in constructing a motor of such a size that he was able to drive a circular-saw and a lathe by it. I might describe the electric-motor of Davidson, which, in 1842, enabled him to propel a carriage at the speed of four miles an hour, between Edinburgh and Glasgow. I might describe the engine built in 1849, by Soren Hjörth, at Liverpool, which was of ten-horse power.

All these early attempts, however, came to nothing, for two reasons. At that time there was no economical method of generating electric currents known. At that time, moreover, the great physical law of the conservation of energy was not recognized, and its all-important bearings upon the theory of electric machinery were not available.

While voltaic batteries were the only available sources of electric currents, economical working of electric-motors was hopeless. For a voltaic battery, wherein electric currents are generated by dissolving zinc in sulphuric acid, is a very expensive source of power. To say nothing of the cost of the acid, the zinc—the very fuel of the battery—costs more than twenty times as much as coal, and is a far worse fuel; for whilst an ounce of zinc will evolve heat to an amount equivalent to 113,000 foot-pounds of work, an ounce of coal will furnish the equivalent of 695,000 foot-pounds.

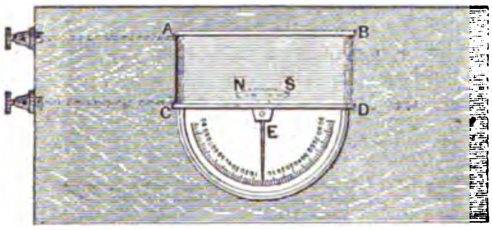
The fact, however, which seemed most discouraging, and which, if rightly interpreted in accordance with the law of conservation of energy, would have been found to be (on the contrary) a most encouraging fact, was the following:—If a galvanometer was placed in the circuit with the electric-motor and the battery, it was found that when the motor was running it was impossible to force so strong a current through the wires as that which flowed when the motor was standing still. Now there are only two causes that can stop such a current flowing in a circuit; there must be either an obstructive resistance or else a counter-electromotive force. At first, the common idea was that, when the motor was spinning round, it offered a greater resistance to the passage of the electric current than when it stood still. The genius of Jacobi enabled him, however, to discern that the observed diminution of current was really due to the fact that the motor, by the act of spinning round, began to work as a dynamo on its own account, and tended to set up a current in the circuit in the opposite direction to that which was driving it. The faster it rotated the greater was the counter-electromotive force (or "electromotive force of reaction") which was developed. In fact, the theory of conservation of energy requires that such a reaction should exist.

We know that, in the converse case, when we are employing mechanical power to generate currents by rotating a dynamo, directly we begin to generate cur-

rents, that is to say, directly we begin to do electric *work*, it immediately becomes much harder work to turn the dynamo than is the case when no electric work is being done. In other words, there is an opposing reaction to the mechanical force which we apply in order to do electric work. An opposing reaction to a mechanical force may be termed a "counter-force." When, on the other hand, we apply (by means of a voltaic battery, for example) an electromotive force to do mechanical work, we find that here again there is an opposing reaction; and an opposing reaction to an electromotive force is a "counter-electromotive force."

The experiment of showing the existence of this counter-electromotive force is a very easy one. All one requires is a

Fig. 51



S. P. THOMPSON'S LANTERN GALVANOMETER.

little motor, a few cells of battery, and a galvanometer. The galvanometer I shall use to-night is the one which I brought out some years ago, and which has proved itself very convenient for lecture work, because it can be put into any ordinary lantern and projected on the screen (Fig. 51). I have here, as a battery, four small accumulators of the Faure-Sellon-Volckmar type, and I have connected them with a little motor, also of my own design, the current being arranged so as to run through the galvanometer. I hold the spindle of the motor fast, so that it cannot rotate, and you see that the pointer of the galvanometer indicates 44°. I now release the motor; it begins to rotate, and as its speed increases, you observe the needle descends the scale to 23°, and eventually to about 15°. If I load the motor and cause it to slacken speed, the needle at once returns.

The existence of this counter-electromotive force is of the utmost importance, in considering the action of the dynamo as a motor, because upon the existence and magnitude of this counter-electromotive force, depends the degree to which any given motor enables us to utilize electric energy that is supplied to it in the form of an electric current. In discussing the dynamo as a generator, I pointed out many considerations, the observance of which would tend to improve the efficiency of such generators. It is needless to say that many of these considerations, such as the avoidance of useless resistances, unnecessary iron masses in cores and the like, will also apply to motors. The freer a motor is from such objections, the more efficient will it be. But the efficiency of a motor in utilizing the energy of a current depends not only on its efficiency in itself, but on another consideration, namely, the relation between the electromotive force which it itself generates when rotating, and the electromotive force—or, as some people call it, the electric pressure—at which the current is supplied to it. A motor which itself in running generates only a *low* electromotive force cannot, however well designed, be an *efficient* or economical motor when supplied with currents at a *high* electromotive force. A good low-pressure steam-engine does not become more "efficient" by being supplied with high-pressure steam. Nor can a high-pressure steam-engine, however well constructed, attain a high efficiency when worked with steam at low pressures. Analogous considerations apply to dynamos used as motors. They must be supplied with currents at electromotive forces adapted to them. Even a perfect motor—one without friction or resistance of any kind—cannot give an "efficient" or economical result, if the law of efficiency is not observed in the conditions under which the electric current is supplied to it.

Now it can be shown, mathematically, that the efficiency with which a perfect motor utilizes the electric energy of the current depends upon the ratio between this counter-electromotive force and the electromotive force of the current that is supplied by the battery. No motor ever succeeds in turning into useful work the whole of the currents that feed it, for it is impossible to construct machines without resistance, and whenever resistance is offered to a current part of the energy

of the current, it is wasted in heating the resisting wire. Let the symbol W stand for the whole electric energy of a current, and let w stand for that part of the energy which the motor takes up as useful work from the circuit.* All the rest of the energy of the current, or W—w, will be wasted in useless heating of the resistances.

But if we want to work our motor under the conditions of greatest economy, it is clear that we must have as little heat-waste as possible; or, in symbols, w must be as nearly as possible equal to W. It can be shown, mathematically, that the ratio between the useful energy thus appropriated, and the total energy spent, is equal to the ratio between the counter-electromotive force of the motor, and the whole electromotive force of the battery that feeds the motor. The proof will be given later. Let us call this whole electromotive force with which the battery feeds the motor E, and let us call the counter-electromotive force e. Then the rule is

$$w : W = e : E$$

or, if we express the efficiency as a fraction,

$$\frac{w}{W} = \frac{e}{E}.$$

But we may go one stage further. If the resistances of the circuit are constant, the current c, observed when the motor is running, will be less than C, the current while the motor was standing still. But from Ohm's law, we know that

$$c = \frac{E - e}{R},$$

hence

$$\frac{C - c}{C} = \frac{e}{E} = \frac{w}{W}.$$

From which it appears that we can calculate the efficiency at which the motor is working, by observing the ratio between the fall in the strength of the current and the original strength. Now, this mathematical law of efficiency has been known for twenty years,* but has been strangely misapprehended. Another law, discovered by Jacobi, not a law of efficiency at all, but a law of maximum work in a given time, has usually been given instead.

Jacobi's law concerning the maximum work of an electric-motor supplied with currents from a source of given electromotive force, is the following:—The mechanical work given out by a motor is a maximum when the motor is geared to run at such a speed that the current is reduced to half the strength that it would have if the motor was stopped. This, of course, implies that the counter-electromotive force of the motor is equal to half the electromotive force furnished by the battery or generator. Now, under these circumstances, only half the energy furnished by the external source is utilized, the other half being wasted in heating the circuit. If Jacobi's law were indeed the law of efficiency, no motor, however perfect in itself, could convert more than 50 per cent. of the electric energy supplied to it into actual work. Now Siemens showed,† some years ago, that a dynamo can be, in practice so used as to give out more than 50 per cent. of the energy of the current. It can, in fact, work more efficiently if it be not expected to do its work so quickly. Dr. Siemens, to whom we owe the honor of having first shown us the true physical signification of the mathematical expressions which, until then, had been regarded as mere abstractions, has, in fact, proved that if the motor be arranged so as to do its work at less than the maximum rate, by being geared so as to do much less work per revolution, but yet so as to run at a higher speed, it will be more efficient; that is to say, though it does less work, there will also be still less electric energy expended, and the ratio of the useful work done to the energy expended will be nearer unity than before.

The algebraic reasoning is as follows: If E be the electromotive force of the generator when the motor is at rest, and

* This symbol w must be clearly understood to refer to the value of the work taken up by the motor, as measured electrically. The whole of this work will not appear as useful mechanical effect, however, for part will be lost by mechanical friction, and part also in the wasteful production of eddy-currents in the moving parts of the motor. What proportion of w appears as useful mechanical work depends on the efficiency of the motor *per se*, which we are not here considering. In all that follows immediately we shall suppose such causes of loss not to exist; or the motor will be considered as a perfect motor.

* See Verdet's "Théorie Mécanique de la Chaleur," where, however, Verdet makes the very mistake so often made, of supposing that the greatest possible efficiency of a motor, working with a given electro motive force, is 50 per cent., or its efficiency when working at the maximum rate.

† The matter was also very well and clearly put by Prof. W. E. Ayrton, in his lecture on "Electric Transmission of Power," before the British Association in Sheffield, in 1879.

c be the current which flows at any time, the electric energy W, expended in unit time, will be (as expressed in Watts) given by the equation

$$W=Ec=E\frac{(E-e)}{R} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now, when the motor is running, part of this electric energy is being spent in doing work, and the remainder is wasting itself in heating the wires of the circuit. We have already used the symbol w for the useful work (per second) done by the motor. All the energy which is not thus utilized is wasted in heating the resistances. Let the symbol H represent this heat. Its mechanical value will be HJ, where J stands for Joule's equivalent. Then clearly we shall have

$$W=w+HJ.$$

But, by Joule's law, the heat-waste of the current, whose strength is c running through resistance R, is expressed by the equation

$$HJ=c^2R.$$

Substituting this value above, we get

$$W=w+c^2R \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which we may also write

$$w=W-c^2R.$$

But by equation (1) $W=Ec$, whence

$$w=Ec-c^2R \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and, writing for c its value, $\frac{E-e}{R}$, we get

$$w=\frac{(E-e)\,(E-[E-e])}{R}$$

or

$$w=e\frac{E-e}{R} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Comparing equation (5) with equation (1), we get the following:

$$\frac{w}{W}=\frac{e\,(E-e)}{E(E-e)}.$$

or, finally,

$$\frac{w}{W}=\frac{e}{E}.$$

This is, in fact, the mathematical law of efficiency, so long misunderstood until Siemens showed its significance. We may appropriately call it the law of Siemens. Here the ratio $\frac{w}{W}$ is the measure of the efficiency of the motor, and the equation shows that we may make this efficiency as nearly equal to unity as we please, by letting the motor run so fast that e is very nearly equal to E: which is the true law of efficiency of a perfect motor supplied with electric energy, under the condition of constant external electromotive force.

Now go back to equation (3), which is:

$$w=Ec-c^2R.$$

In order to find what value of c will give us the maximum value for w (which is the work done by the motor in unit time) we must take the differential coefficient and equate it to zero.

$$\frac{dw}{dc}=E-2cR=o,$$

whence we have

$$c=\tfrac{1}{2}\frac{E}{R}.$$

But, by Ohm's law, $\frac{E}{R}$ is the value of the current when the motor stands still. So we see at once that, to get maximum work per second out of our motor, the motor must run at such a speed as to bring down the current to half the value which it would have if the motor were at rest. In fact, we here prove the law of Jacobi for the maximum rate of doing work. But here since

$$c=\frac{E-e}{R}=\tfrac{1}{2}\frac{E}{R},$$

it follows that

$$E-e=\tfrac{1}{2}\,E;$$

or

$$\frac{e}{E}=\tfrac{1}{2},$$

whence it follows also that

$$\frac{W}{w}=\tfrac{1}{2}.$$

That is to say, the efficiency is but 50 per cent. when the motor does its work at the maximum rate.*

* It may be worth while to recall a precisely parallel case that occurs in calculating the currents from a voltaic battery. Every one is familiar with the rule for grouping a battery which consists of a given number of cells, that they will yield a maximum current through a given external resistance when so grouped that the internal resistance of the battery shall, as nearly as possible, equal the external resistance. But this rule, which is true for maximum current (and, therefore, for maximum rate of using up the zincs of one's battery) is not the case of greatest economy. For if external and internal resistance are equal, half the energy of the current will be wasted in heat in the cells, and half only will be available in the external

Now, though several graphic constructions have been suggested to convey these facts to the eye, none have hitherto been, to my mind, quite satisfactory. I have, therefore, worked out a new construction, which enables me, in one diagram, to exhibit graphically both Jacobi's law of maximum rate of working, and Siemen's law of efficiency.

Let the vertical line, A B (Fig. 52) represent the electromotive force E, of the electric supply. On A B construct a square, A B C D, of which let the diagonal

Fig.52

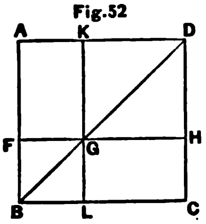
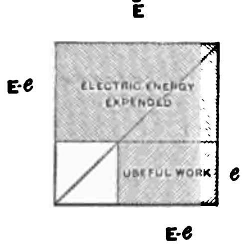


Fig.53



B D be drawn. Now measure out from the point B, along the line B A, the counter electromotive force e of the motor. The length of this quantity will increase as the velocity of the motor increases. Let e attain the value B F. Let us inquire what the actual current will be, and what the energy of it; also what the work done by the motor is. First complete the construction as follows: Through F draw F G H, parallel to B C, and through G draw K G L, parallel to A B. Then the actual electromotive force at work in the machine, and producing the current, is $E-e$, which may be represented by any one of the lines A F, K G, G H, or L C. Now the electric energy expended per second is

$$\frac{E(E-e)}{R},$$

and the work done by the motor is

$$\frac{e(E-e)}{R}.$$

Since R is a constant, the relative values of the two may be written respectively:

$$E(E-e)$$

and

$$e(E-e).$$

Now

area A F H D $=E(E-e)$,

and

area G L C H $=e(E-e)$.

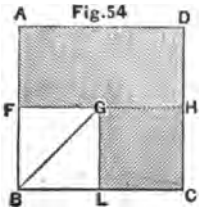
The ratio of these two areas on the diagram is the efficiency of the motor.

Let us turn to Fig. 53, in which these areas are shaded. This figure represents a case where the motor is too heavily loaded, and can turn only very slowly, so that the counter-electromotive force e is very small compared with E. Here the area, which represents the energy expended, is very large; while that which represents useful work realized in the motor is very small. The efficiency is obviously very low. Two-thirds or more of the energy is being wasted in heat.

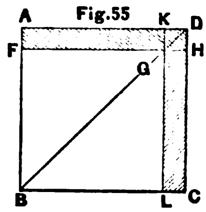
Next, let us turn our attention to the smaller area, G L C H. Obviously, the value of this area will depend upon the position of the point F; for if F is very near B, the area will be very small. If F moves upward, the area, G L C H, will have a value that increases up to a certain point, and then diminishes again; for if F be taken very near A (that is, if e be very nearly equal to E), the area, G L C H, will become again a very narrow strip. Of all the possible cases, the area of this rectangle will be a maximum when G is midway between B and D; for of all possible rectangles that can be inscribed in the triangle, B C D, that rectangle which is a true square will have the greatest value, as drawn in Fig. 54. But if G is midway between B and D, the rectangle, G L C H, will be exactly half the area of the upper rectangle, A F H D; which is, in fact, Jacobi's law of the efficiency of a motor doing its work at its greatest possible rate. Also F will be half way between B and A, which signifies that $e = \frac{1}{2}$ E.

circuit. If we want to get the greatest economy, we should group our cells so as to have an internal resistance much less than the external. We shall not get so strong a current, it is true; and we shall use up our zincs more slowly; but a far greater proportion of the energy will be expended usefully, and a far less proportion will be wasted in heating the battery cells. The maximum economy will, of course, be got by making the external resistance infinitely great as compared with the internal resistance. Then all the energy of the cement will be utilized in the external circuit, and none wasted in the battery. But it would take an infinitely long time to get through a finite amount of work in this extreme. The same kind of reasoning is strictly applicable to dynamos used as generators, the resistance of the rotating part of the circuit being the counterpart to the internal resistance of the battery cells. For good economy the resistance of the armature should be very low as compared with that of the external circuit. Mr. Ed'son seems to have been struck by this point, as his endeavors to reduce the resistance of his armature coils to the utmost limit of possibility show.

Again, consider these two rectangles when the point G moves indefinitely near to D (Fig. 55). We know, from Euclid's Second Book, familiar from our school-days, that the rectangle, G L C H, is equal to the rectangle, A F G K. The area (square), K G H D, which is the excess of A F H D, over A F G K, must, therefore, represent that part of the electric energy which is wasted in heating the resistance of the motor and circuit. In Fig. 53 this corner square, which stands for the heat-waste, was enormous. In Fig. 54 it was exactly half the energy. In Fig. 55 it is only about one-eighth. Clearly, we may make the heat-waste as small as we please,



Geometric illustration Jacobi's law of maximum rate of doing work.



Geometric illustration Siemens's law of efficiency.

if only we will take the point F very near to A. The efficiency will be a maximum when the heat-waste is a minimum. The ratio of the areas, G L C H and A F H D, which represents the efficiency, can only become equal to unity when the square, K G H D, becomes indefinitely small; that is, when the motor runs so fast that its counter-electromotive force, e, differs from E by an indefinitely small quantity only. Further, it is clear that if our diagram is to represent any given efficiency—for example, an efficiency of 90 per cent., then the point, G, must be taken so that the area G L C H = 9-10ths of the area A F H D; or G must be 9-10ths of the whole distance along B D. This involves that e shall be equal to 9-10ths of E, or that the motor shall run so fast as to reduce the current to 1-10th of what it would be if the motor were standing still. Thus we verify, geometrically, Siemen's law of efficiency.*

In all the preceding discussion, I have supposed that the motor is to be worked with a supply of current furnished at a fixed electromotive force. It is not only convenient, but I believe wise, to make such a condition the basis of the argument, because this is, probably, the condition under which electric power will, in the not very distant future, be distributed over large areas. It would be absurd, in the present stage of electro-technical science, to deal with such a question as the construction and use of motors, without taking into account the practical conditions under which they will be used. It is true that the condition of having a constant fixed electromotive force is not the only condition of supply; for, as we have seen in preceding lectures, a generator or system of generators may be worked so as to yield a constant current. And it would be quite possible to formulate a set of rules for the efficiency and maximum duty of motors under this condition. But this method of distributing electric power is far less likely to be of importance in the near future, than the system of distribution with constant electromotive force; though for the case of transmission of power to an isolated station, the case becomes of importance. One simple problem connected with this case is worthy of mention. Suppose that one is desirous of working a motor, so as to do work at the rate of a specified number of horse-power, and that the wire available to bring the current cannot safely stand more than a certain current, without being in danger of becoming heated unduly. It might be desirable to know what electromotive force such a motor ought to be capable of giving back, and what electromotive force must be applied at the transmitting end of the wire. Let N stand for the number of horse-power to be transmitted, and c for the maximum strength of current that the wire will stand (expressed in ampères.) Then, by the known rule for the work of a current, since—

$$\frac{ec}{746} = \text{N},$$

* We have all along supposed the motor to be a "perfect" one, that is to say, one which gave out as available mechanical work all the electric work it took from the supply of energy of the current. No motor does this, because of friction, &c. But it is easy to adapt the construction to the case. Suppose when working the motor as a generator, so as to give an electromotive force, e, we found that it was capable of converting 80 per cent. of mechanical energy into electrical energy; then we may put down the efficiency of the motor, *per se*, under the given conditions at 80 per cent. All we have, then, to do, is to take an area 8-10ths of the size of the area G L C H (or cut off 1-5th of G L C H), and this will really represent the available mechanical work of the motor.

$$e=\frac{746N}{c},$$

gives the condition as to what electromotive force the machine must be capable of giving, when running at the speed it is eventually to run at as a motor. Moreover, the primary electromotive force, E, must be such that

$$\frac{E-e}{\Sigma R}=c$$

where ΣR is the sum of all the resistances in the circuit. Whence,

$$E=e+c\Sigma R$$

which is the required condition.

Another problem in the application of motors to transmission of power, which vitally affects their construction, is the determination of the relation of the heat-waste to the electromotive force at which the current is supplied to the motor.

If, as before, ΣR stands for the sum of all the resistances in the circuit, then, by Joule's law, the heat-waste is (in mechanical measure)

$$HJ=C^2\Sigma R.$$

And, since $c=\frac{E-e}{\Sigma R}$ we may write the heat-waste as

$$HJ=\frac{(E-e)^2}{\Sigma R}.$$

Now suppose that without changing the resistances of the circuit we can increase E, and also increase e, while keeping $E-e$ the same as before, it is clear that the heat loss will be precisely the same as before. But how about the work done? Let the two new values be respectively E' and e'. Then the electric energy expended is—

$$W'=\frac{E'(E'-e')}{\Sigma R},$$

and the useful work done is—

$$w'=\frac{e'(E'-e')}{\Sigma R}.$$

That is to say, with the no greater loss in heating, more energy is transmitted, and more work done. Also the efficiency is greater, for

$$\frac{w'}{W'}=\frac{e'}{E'},$$

and this ratio is more nearly equal to unity than $\frac{e}{E}$—because both E and e have received an increment arithmetically equal. Clearly, then, it is an economy to work at high electromotive force. The importance of this matter, first pointed out by Siemens, and later by Marcel Deprez, cannot be overrated. But how shall we obtain this higher electromotive force? One very simple expedient is that of driving both the generator and motor at higher speeds. Another way is to wind the armatures of both machines with many coils of wire having many turns. This expedient has, however, the effect of putting great resistances into the circuit. This circumstance may, nevertheless, be no great drawback, if there is already a great resistance in the circuit—as, for example, the resistance of many miles of wire through which the power is to be transmitted, in this case, doubling the electromotive force will not double the resistance. Even in the case where the line resistance is insignificant, an economy is effected by raising the electromotive force. For, as may be deduced from the equations, when $E-e$ is kept constant, the effect of doubling the electromotive force is to double the efficiency, when the resistance of the line is very small as compared with that of the machines, and to quadruple it when the resistance of the line is very great as compared with that of the machines. It is, in fact, worth while to put up with the extra resistance, which we cannot avoid, if we try to secure high electromotive force by the use of coils of fine wire of many turns. It is true that the useful effect falls off, *cœteris paribus*, as the resistance increases: but this is much more than counter-balanced by the fact that the useful effect increases in proportion to the square of the electromotive force.

In the recent attempt of M. Marcel Deprez to realize these conditions, in the transmission of power from Miesbach to Munich, through a double line of telegraph wire, over a distance of thirty-four miles, very high electromotive forces were actually employed. The machines were two ordinary Gramme dynamos, similar to one another, but their usual low-resistance coils had been re-

placed by coils of very fine wire. The resistance of each machine was consequently 470 ohms, whilst that of the line was 950 ohms.* The velocity of the generator was 2,100 revolutions per minute; that of the motor, 1,400. The difference of potential at the terminals of the generator was 2,400 volts; at that of the motor, 1,600 volts. According to Professor von Beetz, the President of the Munich Exhibition, where the trial was made, the mechanical efficiency was found to be 32 per cent. M. Deprez has given the rule that the efficiency $\frac{w}{W}$ is obtained, in the case where two identical machines are employed, by comparing the two velocities at the two stations. Or

$$\frac{w}{W}=\frac{n}{N}$$

Where N is the speed of the generator, n that of the motor. There is, however, the objection to this formula, that the electromotive forces are not proportional to the speeds, unless the magnetic fields of the two machines are also equally intense, and the current running through each machine the same. This is not the case if there is leakage along the line. Moreover, when there are resistances in the line, the ratio of the two electromotive forces of the machines is not the same as the ratio of the two differences of potentials, as measured between the terminals of the machines.

I now turn back from these somewhat abstract questions to consider, by the light we have derived already, some points in the design and construction of motors. We shall find that many of the rules suggested in my former lecture are applicable also to the case of motors, and supply answers to many of the questions that naturally arise.

In the first place, shall we build motors large, or shall we build numbers of small ones? In my first lecture I proved that, in the dynamo used as a generator, the capacity for doing work increased as the fifth power of the linear dimensions; that by doubling a dynamo in length, breadth, and thickness, we had a machine weighing eight times as much, costing less than eight times as much, but capable of doing thirty-two times the work, and that with a great gain in economy in working. The same thing is true of motors. Suppose instead of building eight small motors, we build one large one, of doubled dimensions. It will not cost so much as the eight, will get through four times as much work as the eight put together, and will be more economical in working. I assume here, of course, that the large machine can be placed under equally advantageous conditions of supply.

This is by no means the only one of the points in the theory of the dynamo which can be applied to practice in the cases in which the dynamo is used as a motor. I will give another example.

In the prospect of an immediate field of usefulness opening out for motors, so soon as we have such a thing as regular town supplies of electric currents laid on, it is most important that motors should be designed, not simply to work with the constant electromotive force supplied at the electric mains, but designed also to work at uniform speeds. It is highly important, for example, in driving a lathe, and, indeed, many kinds of machinery, that the speed should be regular, and the motor should not "run away" as soon as the stress of the cutting tool is removed. Now, in my second lecture, I spent some time explaining the methods by which M. Marcel Deprez and Professor Perry had solved a converse problem to this, namely, that of getting a dynamo to feed a circuit with currents, at a constant electromotive force, when driven with a uniform speed. The solution to that problem we saw consisted in using certain combinations for the field magnets, which gave an initial magnetic field, independently of the actual current furnished by the dynamo itself. Now, it is not hard to see that this problem may be applied conversely, and that motors may be built with a combination of arrangements for their field magnets, such that, when supplied with currents at a certain constant electromotive force, their speed shall be constant, whatever the work or no work which they may be doing. The one difficulty in the problem—and this is a mere matter for experiment and calcula-

* These figures, and those which follow, are given on the authority of the President of the Munich Exhibition, Professor von Beetz. It is but fair to add that M. Deprez is not satisfied of their accuracy.

tion—is to find the critical number of volts of electromotive force at which this will hold good, It is, in fact, the converse to the operation of finding the critical velocity at which one of Deprez's or of Perry's combination dynamos must be driven, in order that it may give a constant electromotive force. M. Marcel Deprez has, himself, constructed motors upon this plan. Some three years ago, I saw one of Deprez's motors, which ran at a perfectly uniform speed, quite irrespective of the work it was doing. Whether it was lifting a load of five kilogrammes from the ground, or was letting this load run down to the ground, or ran without any load at all, the speed was the same. At the Paris Exposition Electrique of 1881, a large number of Deprez's motors were shown, running at uniform speed and driving various machines, lathes, sewing-machines, &c.

Amongst others who have aimed at producing a motor to work at uniform speed, are Professors Ayrton and Perry. Professor Ayrton informs me that one of these motors, weighing only 350 pounds, will give an effective power equal to 8-horse power, and that "without any mechanical governor; without anything, in fact, in the nature of a moving governor, it always goes at the same speed, whatever work it has to do." Certain little legal matters connected with the protection of this invention have debarred Professor Ayrton from informing me more fully of the new machine, which, if it fulfils the promise of its inventors, will be a great step forward in the mechanical application of electricity. But I can hazard a guess that the arrangement for obtaining uniform speed is dependent on the prior condition that the motor be supplied, either with a constant electromotive force, or with a constant current of a certain "critical" value; and that the solution of the problem is virtually one of the cases of combination, the counterpart of which, as applied to generators, I discussed last week.

It is, of course, possible to use as a motor any direct current dynamo, whether the field-magnets be series-wound, shunt-wound, separately excited, or permanently magnetized. There is this curious point of difference in different cases. Suppose the dynamo to be arranged so as to work as a generator, and then to be supplied with currents from an exterior source, to make it work as a motor. If the dynamo is series-wound, it will run the reverse way (or against its brushes), no matter which way the currents run through it. If the dynamo be shunt-wound, it will run with its brushes, whichever direction the current runs through it. The direction of rotation taken by the separately excited and the magneto-machine will also be with the brushes, if the current is in the right direction, through the armature. These points have to be taken into account in any attempt to combine the different systems.

If we attempt to apply to motors rules and suggestions, such as these applied to generators in the first of these lectures, we shall find that, whilst some of them apply directly, others are singularly in contrast. For example, we found it advisable, for the sake of steadying the currents in generators, to use large and long field-magnets with plenty of iron, and with heavy pole pieces. In the case of motors, there is no such necessity laid upon us; for we want here to produce a uniform steady rotation. Now, even if the impulses be intermittent, the mechanical inertia of the moving parts will steady the motion. Electric curents have no such inertia (except in so far as the self-induction in a circuit exerts an influence like that of inertia), and hence the precautions for generators. In the case of generators, we found that, to produce steady currents, we had to multiply coils on the armature in many separate paths, grouped round a ring or a drum, involving a complicated winding, and a collecting apparatus, consisting of many segments. In motors no such necessity exists, provided only we arrange the coils that there shall be no dead points. I do not say that, for large motors, it may not be advisable to multiply the paths and segments for other reasons—as, for example, to obviate sparking at the collectors—but, for securing steady running, the inertia of the moving parts spares us—at any rate, in small machines—the complication of parts which was expedient in the generator. Some of the most successful of the little motors that have recently appeared—those, for example, of Deprez,

Trouvé, and Griscom—have for their armatures the simple old shuttle-wound Siemens armature of 1856, and in these motors, of course, there is the disadvantage of dead-points to take into account. Deprez, in his first motors, placed this armature longitudinally between the poles of a horse-shoe magnet, with the axis parallel to the limbs. He has also constructed motors with two such armatures on one spindle, one of the coils being 90° in advance of the other, so that while one was at the dead point the other should be in full action. The same suggestion has been carried out in Akestor's motor. Trouvé has tried to get over the dead-points, by utilizing the method of oblique approach mentioned earlier in my lecture. The Griscom motor, which has little copper rollers as commutator brushes, has, for field-magnets, a compact tubular electro-magnet wound in series with the armature. It has the disadvantage of dead-points. There is, in all these motors, the disadvantage that at every half-revolution the magnetism of the armature core is reversed; and as in all these forms this core is of solid iron, there must be waste by heating in the cores. In fact, to the rotating armatures of motors, as to those of generators, apply all the rules about slitting to get rid of induced eddy-currents, avoiding idle coils and useless resistances, &c. The rules about proper pole-pieces, adjustable brushes, multiplication of contacts, also are mostly applicable to motors as well as to generators.

In order to meet the case of a handy and reliable motor, I have designed a machine, the smallest size of which is on the table before you. The field-magnets, which also constitute the bed-plate of the motor, are of malleable cast iron, of a form that can be cast in one, or at most, two pieces. The form of them is that of a Joule's magnet, with large pole-pieces, and wound with coils, arranged partly in series, partly as a shunt, in certain proportions, so as to give me a constant velocity when worked with an external electromotive force of a certain number of volts.

As an armature, I employ a form which, I think, unites simplicity with efficiency for the end desired. I do not want dead points, and yet I do not want to have a complicated armature. I have, therefore, modified the old Siemens armature by embedding, as it were, one of the shuttle-shaped coils within another at right angles to one another and having duplicated the coils, I must, of course, duplicate the segments of the commutator, which, therefore, becomes either a four-part collector, or else a double collar, according to circumstances. There are no solid iron parts in the armature, but the cores are made of thin pieces of sheet iron, stamped out and strung together. Of the efficiency of this little motor you were witnesses a little while ago, when I employed it in the experiment with the galvanometer.

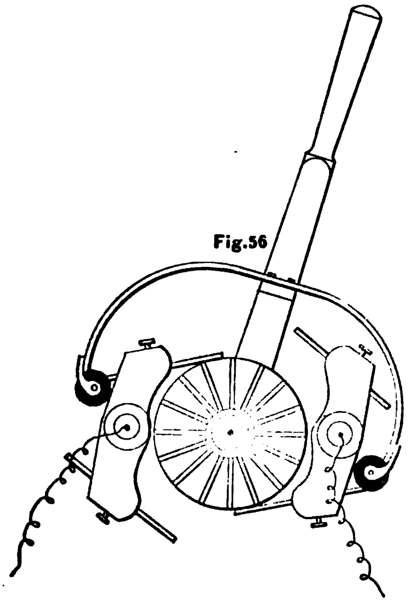
There are here on the table two other forms of motor, one of which is the design of Mr. A. Reckenzaun, C.E., and which is interesting because its armature, though a drum-armature in form, in reality consists of independent coils, connected, like those of the Brush dynamo, to separate commutators. There are, in fact, four commutators grouped as two twos, and the two pairs of brushes in contact with them.

The other motor is one of the pattern invented by De Meritens, who employs a ring-armature very like that of Gramme, but places it between very compact and light field-magnets, which form a framework to the machine. There is one point about this machine of great interest, which is, however, a later addition. It is provided with a reversing gear, which, when I move the handle, reverses the direction in which it runs. This gear, which is the invention of Mr. Reckenzaun (to whom I am indebted for the loan of this motor) is shown in Fig. 56. In it there are two pairs of brushes; the two upper are fixed to a common brush-holder, which turns on a pivot, and can be tilted by pressing a lever handle to right or to left. The two lower brushes are also fixed to a holder. Against each brush-holder presses a little ebonite roller, at the end of a bent steel spring, fixed at its middle to the handle. The result of this arrangement is that, by moving the lever, the brushes can be made to give a lead in either direction, and so starting the motor rotating in either direction. Such a reversing gear is obviously a most essential adjunct for industrial applications of motors, and, if the difficulties of sparking at the brushes, caused by the sudden removals of them from the

collector, be obviated, must prove much better than any mechanical device to reverse the motion, by transferring it from the axle of the motor through a train of gearing to some other axle. One great advantage of electric motors is, that they can be so easily fixed directly on the spindle of the machine they are to drive; an advantage not lightly to be thrown away.

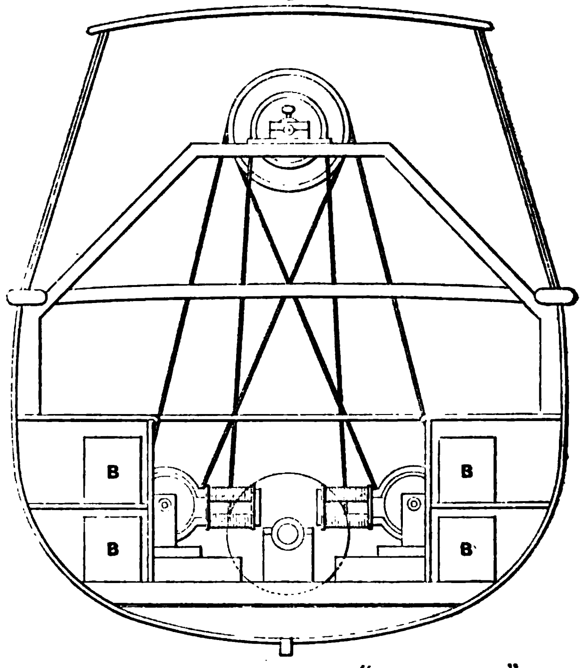
There is, indeed, an immense field for useful industrial application of the electric motors, so soon as we once have at our disposal regular town-supplies of cates a slow but powerful motion to a drum on which the hauling chain is wound. If town-supplies of electricity were accomplished facts, such lifts would be multiplied. Indeed, there very many purposes for which hydraulic power is used, for which electric power might be advantageously substituted. The electric railway, dimly foreshadowed by George Little in 1844, when he patented an "electromotive," has become an established fact. From the electric railway first established in Berlin in 1880, by Siemens and Halske, we already have had

Fig.56



BECKENZAUN'S REVERSING GEAR.

Fig.57



MACHINERY OF THE LAUNCH "ELECTRICITY."

electric currents. Already we have got the little motors of Griscom, Howe, and others, adapted to work sewing-machines, and instruments requiring very small power. Larger motors for driving lathes and heavier machinery, though not yet much known to the public, are in the market. Messrs. Siemens have brought out an electric lift or elevator,* in which a small dynamo, itself running very quickly, drives an endless screw, and communi- the further developments of the electric tramway in Paris, and the experimental electric railway at the Crystal Palace. Edison has constructed an electric railway at Menlo Park, and built electric locomotives to ply upon it. Nor must I omit to mention how, in the bleaching fields of France, an electric tramway does most satisfactory duty; nor how, finally, in the sister isle, at the Giant's Causeway, Messrs. Siemens Brothers have estab- a line of electric railway, which is now actually in operation.

Latest of all the applications of the dynamo as a motor is that of electric

* Dr. Hopkinson has also invented an electric lift, in which the armature of the motor, running at a high speed, works the chain of the lift by a train of toothed wheels, which reduce the speed. This elevator was shown in operation at the Paris Exposition of 1881.

navigation. Jacobi, as we know, fitted up, in 1838, an electric paddle-boat, which he propelled on the Neva, first with Daniell's and afterwards with Grove's cells, and was very able when he used 128 large cells, to carry fourteen persons against the stream, at a speed of about $1\frac{1}{4}$ miles per hour.

M. Trouvé, of Paris, was the next to apply electric power, in the shape of Planté's accumulators, to a pleasure-boat, with which he navigated the Seine. But M. Trouvé has more recently abandoned storage cells, and has returned to the use of a bichromate battery, wherewith to drive the little boats, which he fits up with his little motors, mounted upon the top of the rudder, and turning a light screw-propeller by a driving belt, which passes down behind the rudder into the water.

Still more recent is the electric launch which made its trial trip on the Thames,* last September. This is an iron boat twenty-five feet long, fitted with a screw, and able to carry twelve persons, while running against the tide, at a pace of eight miles an hour. No existing motor of requisite power having yet been made of a form convenient to place directly upon the axis of the propeller, the expedient has, in this case, been resorted to of driving the screw by belts from an overhead counter-shaft, which receives its motion from two small Siemen's dynamos placed below. The arrangements of the machinery of the boat are depicted in Fig. 57. Each of these dynamos has been provided by Mr. Reckenzaun, who designed the fittings of the launch, with reversing gear like that shown in Fig. 56. The current, which actuates these dynamos, is derived from forty-five accumulators of the Sellon-Volckmar type, which are charged while the launch lies at its moorings. Though this is a great step in electric navigation, I venture to believe that it is yet but a beginning of greater things. As with steamboats so with electric-boats, we shall find that we begin with small experimental attempts, and increase in size as experience dictates. It is but a few days since I was reading, in an old volume of the *Philosophical Magazine* for the year 1801, of Symington's early little steamboat—the forerunner both of Fulton's and of Bell's—and the writer of the article hardly dared to think of this as more than an ingenious experiment, and doubted whether large vessels could ever be practically driven by steam. When I read this curious narrative, I bethought me of the trial trip of the little launch *Electricity*, and began to think that I was not so very hopelessly wrong, when I wrote in September last: "Who shall say to what proportions this latest application may not attain in the next decade?"* Could we look forward, and see with our eyes to-night the electric boats of fifty years hence, we should not, perhaps, be more surprised than those who beheld the first rude steamboat would be, if they could now rise from their graves to behold the great ocean-going steamships of to-day. Indeed, when I look around, and see so many opportunities for the future application of electric motive power, so many instances in which the dynamo would supply a welcome and economical† means

* In 1856, Mr. G. E. Dering constructed, at Messrs. Searle's, an electric boat worked by a motor, in which the rotation was effected by magnets arranged within coils, like galvanometer needles, and acted on successively by currents from a battery. It succeeded, I am informed, in making headway, though at small speed, against the tide at its swiftest.

* It is well known that a no less authority than Dr. Lardner publicly pronounced it impossible for any steamboat every to carry coals enough to enable her to cross the Atlantic. It is also recorded that a very eminent public man offered to swallow the boilers of the first steamboat that should succeed in crossing the Atlantic. But he would be a bold man who would now-a-days promise to swallow the accumulators or the dynamos of the first electric boat that shall successfully make the same trip across the water.

† As to the economy of electric motive power, I may perhaps be allowed to quote a passage from a recent lecture of my own in Bristol, in which a concrete case is presented: "When it is possible thus to obtain power by merely turning on a tap, there will be an enormous impetus to all kinds of small machinery. Motors to work sewing-machines already exist, and would be used literally by millions if the source of electricity were but at hand, in the form of a friendly wire always ready to bring the needed power. . . . Again, the establishment of a town supply of electricity will tend unquestionably to develop the industries of small workshops and home workshops, where a little power is required. I have already explained how, with steam power, small engines of one horse-power or less are uneconomical. But with electricity it is not so. The smallest electro-motors may be made just as economical as are the largest. There is, moreover, another point in their favor. They may be made far smaller, lighter, more compact, and far cheaper than steam-engines of corresponding power. . . . It is certainly possible to construct an electric motor of two horse-power for ten pounds, and such a motor would be ample for the purposes of many small workshops. Why do we not, it may be asked, hear and see more of these little machines, if they are so excellent? Why do not people adopt them? The answer is simply they cannot be used until we have a central supply of electricity. I do not hesitate to affirm that such motors will be found to present an enormous economy in comparison with steam-engines of equal power. Take the case of a small workshop which wants just two horse-power to turn a few

of doing that which is now done by less advantageous means, I begin to wonder why we do not establish central supplies of electric currents for this purpose, which is surely as important as electric lighting. Here we have the possibility of introducing machinery that is portable and light, giving up power without the inconveniences and risks hitherto attending the employment of power. Surely there must be many workshops in every town where the owner would be glad of having power without the trouble and danger of a furnace, boiler, and steam-engine, or even of a gas-engine, in his place; where a boiler cannot be put for want of space, and where a steam-pipe cannot be carried because of the inevitable condensation. So thoroughly am I convinced of the great future that lies before dynamo-electric machinery, from this point of view, that I am disposed to think electric lighting a very small item by comparison.* When once we get a proper system arranged for the general distribution of electric currents in a regular town supply, we shall begin to realize the hundreds of ways, as yet undreamed of in which electricity may be utilized to meet our wants, and to subserve the ends of civilization.

lathes or drilling machines. A small steam-engine and boiler will, together, cost about £70, and the consumption of coal will be about 18 lbs. weight per hour. Instead of this, put in a motor costing, let us say, £20, which allows for handsome profits on manufacture. Here is a saving in capital outlay at once. Moreover, the motor may be put right into the workshop, for it is small—perhaps two feet long, eighteen inches broad, and one foot high—and is not liable to leak or explode. The next point to consider is the cost of running it. Let us suppose that it has only an efficiency of 70 per cent., or that it only turns 7-10ths of the energy of the current into useful mechanical work. (Many motors will do far more than this.) Let us also suppose that the currents are furnished by a dynamo whose efficiency is 90 per cent. (which figure is also capable of being attained in practice), worked by a large central steam-engine, consuming only 1½ lbs. of coal per horse-power. Allow 4 per cent. for loss, by resistance, in the conducting cables; this reduces the total efficiency of the generator to 86 per cent., and of that 86 per cent. only 7-10ths—*i. e.*, 60.2 per cent — is eventually converted back into useful work by the motor. We have, then, 40 per cent. wasted in order to get 60 per cent. of useful work. How does this affect the coal consumption for two horse-power? At the central station 3 lbs. of coal per hour would suffice were there no loss in conversion into electric energy, and back again into mechanical power. As it is, 5 lbs. of coal per horse-power will be needed, 3 lbs. of which represent the 60 per cent. of useful power transmitted, and 2 lbs. to represent the 40 per cent. wasted. In practice, I believe, a far less margin of waste might be looked for. Yet even with this large waste there is still an immense economy effected, for we get our two horse-power by an expenditure of 5 lbs. of coal at the central station, instead of 18 lbs. of coal in the furnace of a small steam-engine. It is clear, then, that electricity offers an enormous future to small motor machinery."

* What is the proportion of coal used for the manufacture of gas in Great Britain, compared with the amount consumed in procuring motive power? The answer to this query amply justifies my view.

ON THE ADJUSTMENT OF CONDITION-OBSERVATIONS IN THE METHOD OF LEAST SQUARES, WITH ITS APPLICATION TO GEODETIC WORK, AND WITH SPECIAL REFERENCE TO AMERICAN METHODS OF THE ADJUSTMENT OF A SYSTEM OF TRIANGULATION.

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I.

1. In the treatises on the method of least squares published so far in the English language the subject of condition-observations has received but slight notice. Though really a very important application of the method, it has not been systematically discussed by English or American authors mainly for the reason that their treatises were written for a special purpose, and the point aimed at stopped short of that here mentioned. Chauvenet, Airy, and Watson treated the subject chiefly so far as it related to astronomical work, while Merriman wrote professedly for the beginner. Chauvenet's treatise, the oldest and most complete, was first published twenty years ago. The systematic development of the subject of condition-observations was not made till quite recently, in fact not till Hansen published his "*Geodätische Untersuchungen*," and more especially since the publication of the *Danske Gradmaaling* by Andrae. These two works contain the only great advances made since Bessel's time. The fruit of

Andrae's and Hansen's work is seen in the very elegant methods of reduction now in use in the Prussian, Italian and other European surveys. The publications of the Great Trigonometrical Survey of India deserve special mention. Among other good points in these most admirable volumes is a record of failures as well as successes.

2. The only two surveys in America in which extended geodetic work of the highest precision has been carried on with modern instruments, are the Coast Survey and the Lake Survey. The former has been engaged for many years in mapping the Atlantic and Pacific coasts while similar work has been done by the latter on the southern shores of the great lakes. Incidentally arcs of the meridian and arcs of the parallel have been measured on both surveys and others are in progress. The recent change of name of the Coast Survey to Coast and Geodetic Survey, foreshadows a complete survey of the whole country. As an accurate triangulation lies at the basis of every survey, it cannot be out of place to lay together in a connected form the modern methods in use in reducing a triangulation and to illustrate the same from examples of actual work.

The form used on the Coast Survey is scattered through different annual reports, but will be found mainly in the reports of the years 1854, 1864, 1865 and 1868; while the form used on the Lake Survey will be found in the annual report of the Chief of Engineers for 1872, and also in the final report of the Lake Survey lately published.

3. As the adjustment of a triangulation is only a special case of condition-observations, I shall for greater clearness show first how these are treated, and then pass to the special problem, taking up first the rigorous method of adjustment, and next the Lake Survey and Coast Survey forms. I shall also give the Hansen-Schreiber method which is in many respects the best yet proposed, and conclude by deriving a set of rules for adjusting a system of triangulation which can be applied by any one who does not care to learn the method of least squares.

The reader is assumed to be familiar with Chauvenet's treatise on the method of least squares. I shall, as far as possible, use Chauvenet's notation and nomenclature. Some minor variations will be noticed such as "mean-square error" for "mean error," "observation-equation"* for "equation of condition" and the like.

4. If a series of observed quantities are not independent, that is if they must satisfy exactly certain relations imposed in some way outside of the observations they are said to be *conditioned* by these relations.

If a series of quantities whether directly observed or functions of observed quantities are independent of one another the method of least squares shows that we may pick out the most probable system of values from all possible systems by making the weighted sum of the squares of the residual errors of the observed quantities a minimum.

There is really no new principle introduced in the treatment of condition-observations. It is merely an extension of the general principle just stated and all that is done is to apply various devices for getting over difficulties and for shortening work demanded by the straightforward process.

Thus, if

$$F = f\,(x\;y\;z\ldots) \qquad (1).$$

is a function of n variables $x, y, \ldots$ and is to be made a minimum subject to the n_c conditions

$$\begin{aligned} \varphi_1\,(x, y, z\ldots) &= 0 \\ \varphi_2\,(x, y, z\ldots) &= 0 \\ \ldots\ldots&\ldots\ldots \end{aligned} \qquad (2).$$

it is plain that n_c of the variables can be expressed in terms of the remaining $n-n_c$ and that by substituting these values in the original equation we have to make a minimum the resulting function.

$$F = \psi\,(x, y, z\ldots.)$$

containing $n-n_c$ independent variables.

The $n-n_c$ equations resulting from differentiating this equation with reference to the $n-n_c$ independent variables, taken in connection with the n_c equations of condition determine the n quantities $x, y, z\ldots.$ This is the direct solution of the problem and may be used with advantage when the condition-equations are simple.

The solution of the equations contain-

* On this phrase see some good remarks by Merriman, *Least Squares*, London, 1877.

ing the $n-n_c$ independent unknowns may be carried through as in Chauvenet, p. 544, &c., though the forms there given are not always the best for computation.

5. In general, however, the elimination here indicated is carried out by the method of undetermined multipliers. Call k', k'', . . . the multipliers of the condition-equations in order. Then the function

$$f(x, y, z, \ldots) = a \text{ minimum},$$

is determined by making a minimum the equivalent function,*

$$f(x, y, z \ldots) + k'\varphi_1(x, y, z \ldots) + k''\varphi_2(x, y, z \ldots) + \ldots \quad (3).$$

with reference to the $n-n_c$ independent variables.

The differential coefficients for the $n-n_c$ independent variables will then be equal to zero and to determine the n_c multipliers k', k'' . . . we may assume the differential coefficients for the n_c variables not independent also equal to zero. Hence we have in all the n $(=\overline{n-n_c} + n_c)$ equations

$$\frac{df}{dx}=0$$
$$\frac{df}{dy}=0$$
$$\ldots\ldots\ldots\ldots$$

together with the n_c condition-equations from which to determine the $n+n_c$ unknown quantities.

The multipliers k', k'' . . . are called by Gauss the *correlates* of the condition-equations.

6. For simplicity I shall divide the practical consideration of the problem into two parts.

1. When the condition-equations are solved simultaneously.

2. When the condition-equations are solved in groups.

1.—*The Simultaneous Solution of the Condition-Equations.*

7. It is a general principle that functions of observed quantities must first be reduced to the linear form before being treated by the method of least squares.

Let then V_1, V_2, . . . be the most probable values of n directly observed quantities M_1, M_2 . . . whose weights are p_1, p_2 . . . respectively. Let the n condition-equations to be exactly satisfied by the most probable values of the observed quantities when reduced to the linear form be

$$\begin{aligned} a'V_1 + a''V_2 + \ldots - L' &= 0 \\ b'V_1 + b''V_2 + \ldots - L'' &= 0 \quad (4). \\ \ldots\ldots\ldots\ldots \end{aligned}$$

If v_1, v_2 . . . denote the most probable corrections to the observed values, then

$$\begin{aligned} V_1 - M_1 &= v_1 \\ V_2 - M_2 &= v_2 \quad (4\tfrac{1}{2}). \\ \ldots\ldots\ldots\ldots \end{aligned}$$

and we have the reduced condition-equations

$$\begin{aligned} a'v_1 + a''v_2 + \ldots - l' &= 0 \\ b'v_1 + b''v_2 + \ldots - l'' &= 0 \quad (5). \\ \ldots\ldots\ldots\ldots \end{aligned}$$

where

$$\begin{aligned} l' &= L' - [aM] \\ l'' &= L'' - [bM]. \\ \ldots\ldots\ldots\ldots \end{aligned}$$

The most probable system of corrections is that which makes

$$[pv^2] = a \text{ minimum},$$

that is which makes a minimum (see Art. 5)

$$(a'k' + b'k'' + \ldots)\, v_1 + (a''k' + b''k'' + \ldots)\, v_2 + \ldots \tfrac{1}{2}(p_1v_1^2 + p_2v_2^2 + \ldots) \ldots (6).$$

with reference to v_1, v_2 . . as independent variables.

Hence differentiating we have the n equations

$$\begin{aligned} a'k' + b'k'' + \ldots - p_1v_1 &= 0 \\ a''k' + b''k'' + \ldots - p_2v_2 &= 0 \quad (7). \\ \ldots\ldots\ldots\ldots \end{aligned}$$

from which with the n_c condition-equations to determine the quantities k', k'' . . . v_1, v_2 . . .

Substituting for v_1 v_2 . . . in the condition-equations their values derived from the above equations we have n_c equations containing n_c unknowns k', k''. They are the normal equations

$$\left.\begin{aligned} \left\{\frac{aa}{p}\right\}k' + \left\{\frac{ab}{p}\right\}k'' + \ldots - l' &= 0 \\ \left\{\frac{ab}{p}\right\}k' + \left\{\frac{bb}{p}\right\}k'' + \ldots - l'' &= 0 \end{aligned}\right\} (8).$$
$$\ldots\ldots\ldots\ldots$$

Solving three equations we obtain k', k'' . . . and thence v_1, v_2 . . . from (7) and V_1, V_2 . . . from $(4\frac{1}{2})$.

Hence the problem is solved.

* See Todhunter's Diff. Calc., Arts. 288, 289.

8. The form of the normal equations shows that it is convenient to use the reciprocals of the weights instead of the weights themselves. They may be written

$$\begin{aligned}[uaa]k'+[uab]k''+\ .\ .-l'&=0\\ [uab]k'+[ubb]k''+\ .\ .-l''&=0\ .\ (9).\end{aligned}$$

. . . .

when $u_1\ u_2$ . . . denote the reciprocals of the weights p_1, p_2 . . .

Hence in computing the coefficients of the normal equations it is of great service to first of all write down from the condition-equations a tabular form as follows:

		k'	k''	k'''
v_1	u_1	a'	b'	c'
v_2	u_2	a''	b''	c''
v_3	u_3	a'''	b'''	c'''

. . . .

9. If the elimination of equations (9) is performed by the method of substitution (Chauvenet, § 42) we have by collecting the first equations of the successive groups,

$$\begin{aligned}[uaa]k'+[uab]k''+[uac]k'''+.-l'&=0\\ [ubb1]k''+[ubc1]k'''+\ .\ -l''1&=0\ (10).\\ [ucc2]k'''+\ .\ -l'''2&=0\end{aligned}$$

. . . .

the notation being analogous to the ordinary Gaussian form. Divide each of these equations by the first unknown and we have

$$k'+\frac{[uab]}{[uaa]}k''+\frac{[uac]}{[uaa]}k'''+.-\frac{l'}{[uaa]}=0$$

$$k''+\frac{[ubc1]}{[ubb1]}k'''+\ldots-\frac{l''1}{[ubb1]}=0\ (11).$$

$$k'''+\ldots-\frac{l'''2}{[ucc2]}=0$$

$$+\ldots\ldots\ldots\ldots$$

These equations being precisely similar in form to Chauvenet's equations (70), using the quantities, R′, R″ . . . S″ . . . corresponding to his A′, A″, . . . B″ . . . we have, as in §§ 48, 49,

$$k'=\frac{l'}{[uaa]}+\frac{l''1}{[ubb1]}R'+\frac{l'''2}{[ucc2]}S''+\ \ldots$$

$$k''=\qquad+\frac{l''1}{[ubb1]}+\frac{l'''2}{[ucc2]}S''+\ \ldots$$

. . . .

$$\begin{aligned}l''1&=l'R'+l''\ \ldots\ldots\ldots\qquad(12).\\ l'''2&=l'R''+l''S''+l'''\ \ldots\end{aligned}$$

. . . .

and

$$[uaa]=\frac{1}{[uaa]}+\frac{R'^2}{[ubb1]}+\frac{R''^2}{[ucc2]}+\ \ldots$$

$$[u\alpha\beta]=\qquad\frac{R'}{[ubb1]}+\frac{R'S''}{[ucc2]}+\ \ldots$$

. . . .

$$[u\beta\beta]=\qquad\frac{1}{[ubb1]}+\frac{S''^2}{[ucc2]}+$$

$$[u\beta\gamma]=\qquad\frac{S''}{[ucc2]}+\ \ldots.$$

. . . .

where $[u.aa]$, $[u\alpha\beta]$ . . satisfy the relations

$$\begin{aligned}[u.aa][.uaa]+[u.ab][u.\alpha\beta]+\ .\ &=1\\ [u.ab][u.aa]+[u.bb][u.\alpha\beta]+\ .\ &=0\\ [u.aa][u.\alpha\beta]+[u.ab][u.\beta\beta]+.\ &=0\quad(13).\\ [u.ab][u.\alpha\beta]+[u.bb][u.\beta\beta]+\ .\ &=1\end{aligned}$$

. . . .

10.—*To find the precision of the adjusted values.*

The first step is to find the mean-square error μ of an observation of weight unity.

In Chauvenet, Art. 37, it is shown that in a system of observation-equations the mean-square error μ of an observation of the unit of weight is found from

$$\mu=\sqrt{\frac{[pvv]}{n-n_i}},$$

where

$[pvv]$ is the sum of the weighted squares of the residual errors v, n is the number of observation-equations and n_i the number of independent unknowns.

Hence in a system of condition-observations, n being the number of observed quantities and n_c the number of conditions, the number of independent unknowns is $n-n_c$, and

$$\mu=\sqrt{\frac{[pvv]}{n-(n-n_c)}},$$

$$=\sqrt{\frac{[pvv]}{n_c}}\ \ldots\ldots\ldots\ (14).$$

The m. s. e. of an observation of weight p is

$$\mu\sqrt{u}\ \ldots\ldots\ldots\ldots\ (15).$$

where u is the reciprocal of p.

Hence to find the m. s. e. of any adjusted value we must make two distinct computations one of μ and one of u.

11.—*Checks of* $[pvv]$.

When the number of residuals is large, in order to guard against mistakes $[pvv]$

should be computed in at least two different ways. The following check method will be found convenient.

(1). Equation (7) may be written

$$\sqrt{p_1}.v_1=\sqrt{u_1}a'k'+\sqrt{u_1}b'k''+\ \ldots$$
$$\sqrt{p_2}.v_2=\sqrt{u_2}a''k'+\sqrt{u_2}b''k''+\ \ldots$$
$$\cdots\cdots\cdots\cdots$$

Square and add, and

$$\begin{aligned}[pvv]=[u.aa]k'k'&+2[uab]k'k''\\ &+2[uac]k'k'''+\ \ldots\\ +[ubb]k''k''&+2[ubc]k''k'''+\ \ldots\\ &+[ucc]k'''k'''+\ \ldots\\ &+\ \ldots\\ =[kl]\ \text{from (11)}\ \ldots\ldots&\quad(16).\end{aligned}$$

(2) $[pvv]=kl$

$$\begin{aligned}&=l_1'k'+l''k''+\ \ldots\ldots\\ &=l'\left(\frac{l'}{[uaa]}+\frac{l''1}{[ubb1]}R'+\frac{l'''2}{[u.cc2]}R''+\ldots\right)\\ &\quad+l''\left(\frac{l''1}{[ubb1]}+\frac{l'''2}{[ucc2]}S''+\ \ldots\right)\\ &\quad+\ldots\ldots\ldots\\ &=\frac{l'^2}{[uaa]}+\frac{[l''1]^2}{[ubb1]}+\frac{[l''2]^2}{[ucc2]}+\ \ldots\ \ (17).\end{aligned}$$

by addition attending to equations (12).

This is very readily computed from the solution of the correlate normal equations (11).

12.—*To find the weight and mean-square error of any function of the adjusted values* V_1, V_2,...

Let the function be

$$F=f(V_1,\ V_2,\ \ldots\ )$$

and let it be conditioned by the equations

$$\varphi_1(V_1,V_2,\ \ldots\ )=0$$
$$\varphi_2(V_1,V_2,\ \ldots\ )=0$$
$$\cdots\cdots\cdots\cdots$$

Expressing F in terms of the values which are independent of one another and reducing to the linear form, we have

$$dF=\frac{dF}{dM_1}.dM_1+\frac{dF}{dM_2}dM_2+\ \ldots$$

Hence

$$\mu^2_F=\left(\frac{dF}{dM}\right)^2\mu_1^2+\left(\frac{dF}{dM_2}\right)^2\mu_2^2+\ \ldots\ \ (19).$$

when μ_1, μ_2, ... are the m. s. e. of M_1, M_2, ... respectively.

As it usually requires a very long elimination to get F expressed directly in terms of M_1, M_2, ... it is better to compute $\frac{dF}{dM_1}$, $\frac{dF}{dM_2}$, ... from the forms

$$\frac{dF}{dM_1}=\frac{dF}{dV_1}.\frac{dV_1}{dM_1}+\frac{dF}{dV_2}.\frac{dV_2}{dM_1}+\ \ldots$$
$$\frac{dF}{dM_2}=\frac{dF}{dV_1}.\frac{dV_1}{dM_2}+\frac{dF}{dV_2}.\frac{dV_2}{dM_2}+\ \ldots$$
$$\cdots\cdots\cdots\cdots$$

13. It is, however, usually much more convenient in practice to proceed as follows:

Let the function reduced to the linear form be

$$F=f'v_1+f''v_2+\ \ldots\ldots\ \ (20).$$

This is conditioned by the equations

$$a'v_1+a''v_2+\ \ldots\ -l'=0$$
$$b'v_1+b''v_2+\ \ldots\ -l'=0$$
$$\cdots\cdots\cdots\cdots$$

with $[pv^2]=$ a minimum.

Referring to the principle of Art. (5) we find by using the correlates k', k'' ... that we can express the function F in terms of all the quantities v_1, v_2.. which must now be taken as independent. Hence

$$\begin{aligned}dF&=(f'-a'k'-k'k''-\ \ldots)v_1+(f''-\\ &\quad a''k'-b''k''-\ \ldots)v_2+\ \ldots\\ &=F'v_1+F''v_2+\ \ldots\ \text{suppose,}\quad(21).\end{aligned}$$

and therefore

$$u_F=[u.FF]\ \ldots\ldots\ \ (22).$$

the required result.

It remains now to find k', k'' ... Calling k the correlate of equation (21) we have from this condition-equation and the minimum equation

$$kF'-p_1v_1=0$$
$$kF''-p_2v_2=0$$
$$\cdots\cdots\cdots\cdots$$

Hence

$$[uFF]=\left\{\frac{FF}{p}\right\}=[pv^2]=\text{a minimum,}$$

and therefore k', k'' ... are found from the following equations which are of the form of observation equations:

$$a'k'+b'k''+\ \ldots\ -f'=F'\ \text{weight}\ u_1$$
$$a''k'+b''k''+\ \ldots\ -f''=F''\quad \text{"}\quad u_2$$

The resulting normal equations are

$$[uaa]k'+[uab]k''+\ \ldots\ -[uaf]=0$$
$$[uab]k'+[ubb]k''+\ \ldots\ -[ubf]=0\quad(23).$$
$$\cdots\cdots\cdots\cdots$$

These equations being precisely of the form of Chauvenet equations (65) we have, proceeding as in Art. 44,

$$u_F=[uff]-\left\{\frac{[uaf]^2}{[uaa]}+\frac{[ubf1]^2}{[ubb1]}+\right\}.\ (24).$$

which formula will be found of very convenient application in problems of this kind. The formula itself is due to Gauss.

14. The correlate equations (23) being the same as the regular correlate equations in all butthe constant term, we may find u_F from the following scheme in which (uaf), (ubf) . . are added as an extra column in the solution of the correlate equations. Three correlates are taken,

$$\begin{array}{llllll}
k' & k'' & k''' & & & \\
(uaa) & (uab) & (uac) & -l' & -(uaf) & \\
 & (ubb) & (ubc) & -l'' & -(ubf) & \\
 & & (ucc) & -l''' & -(ucf) & (uff) \\
 & (ubb1) & (ubc1) & -l''1 & -(ubf1) & \\
 & & (ucc1) & -l'''1 & -(ucf1) & (uff1) \\
 & & (ucc2) & -l'''2 & -(ucf2) & (uff2) \\
 & & & & & (uff3)=u_F
\end{array}$$

15.—*To find the average value of the ratio of the weight of an observed quantity to that of its adjusted value in a system of condition observations.*

Here

$$F=V_1.$$

Hence, since

$$f_1'=1,\ f''=f'''=\ldots=0$$
$$[uaf]=u_1a_1$$
$$[ubf]=u_1b_1.$$
$$\cdots\cdots\cdots\cdots$$

and using the forms in Chauvenet, Art. 49, we have

$$[ubf1]=(R'a'+b')u_1$$
$$[ucf2]=(R''a+s''b'+c')u_1$$
$$\cdots\cdots\cdots\cdots$$

Substituting these values in

$$u_F = u_1 - \frac{u_1^2a'^2}{[uaa]} + \frac{u_1^2(R'a'+b')^2}{[ubb1]} + \frac{w_1^2(R''a'+s''b'+c')^2}{[ucc.2]} + \ldots$$

or,

$$\frac{p_1}{P_1} = \frac{u_p}{u_1} = 1 - \left\{ \frac{u_1a'^2}{uaa} + \frac{u_1(R'a'+b')^2}{ubb1} + \frac{u_1(R''a'+s''b'+c')^2}{[ucc2]} + \ldots \right\}$$

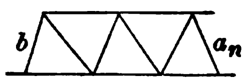
where P_1 is the weight of the adjusted value of V_1.

Now, summing the corresponding expressions for all of the quantities from V_1 to V_n, we have, after expanding and arranging,

$$\left[\frac{p}{P}\right] = n - [uaa]$$
$$\left\{\frac{1}{[uaa]} + \frac{R'^2}{[ubb1]} + \frac{R''^2}{[ucc2]} + \ldots\right\}$$
$$--[uab]\left\{\frac{R'}{[ubb1]} + \frac{R''s''}{[ucc2]} \ldots\right\}\ldots$$
$$-[uab]\left\{\frac{R'}{[ubb1]} + \frac{R''s''}{[ucc2]} + \ldots\right\} - [ubb]$$
$$\left\{\frac{1}{[ubb1]} + \frac{s''^2}{[ucc2]} + \ldots\right\}\ldots\ldots$$
$$\cdots\cdots\cdots\cdots\cdots\cdots$$
$$= n - [uaa][u\alpha\alpha] - [uab][u\alpha\beta] - \ldots$$
$$- [uab][u\alpha\beta] - [ubb][u\beta\beta] - \ldots$$
$$\cdots\cdots\cdots\cdots\cdots\cdots$$
$$= n - n_c$$

= the number of observations — the number of conditions (25). This result was, as far as I know, first published in Vol. 2 of the G. T. Survey of India. It has been used in the precision determination of the Lake Survey triangulation.

16. A special case of the general theorem of Art. 12 of frequent occurrence is the finding of the m. s. e. of a triangle side in a triangulation chain, all of the angles of each triangle having been measured.



Let b be the measured value of the base and let a_1 a_2 . . . A_n be the sides of continuation in order as computed from b, a_n being the side whose m. s. e. is required.

If A_1, B_1 . . . are the measured values of the angles used in computing the terminal side from the base, B_1, B_2 . . . being opposite to the bases and A_1 A_2 . . opposite to the sides of continuation, then

$$\frac{a_1}{b} = \frac{\sin.A_1}{\sin.B_1},\ \frac{a_2}{a_1} = \frac{\sin.A_2}{\sin.B_2},\ \ldots,\ \frac{a_n}{a_{n-1}} = \frac{\sin.A_n}{\sin.B_n}$$

Hence, by multiplication,

$$a_n = b.\frac{\sin.A_1.\sin.A_2\ \ldots\ldots}{\sin.B_1.\sin.B_2\ \ldots\ldots} \qquad (26).$$

We may proceed in two ways:

(*a*) Differentiating directly,

$$da_n = \frac{a_n}{b}.db + a_n.\cot.A_1\sin 1''.dA_1 - a_n$$
$$\cot.B_1\sin.1''.dB_1 + \ldots\ldots$$

$$=\frac{a_n}{b}(b)+a_n\sin.1''\Big(\cot.\ A\ (A)-\cot.\ B(B)\Big)\ .\ .\ .\ (27).$$

where (A), (B) . . . denote the corections dA, dB, . . . This expression is of the standard form (20).

(b) Taking logs of both members of (26) and then differentiating,

$$d\log.\ a_n=d\log.\ b+\text{mod.}\cot.\ A_1\sin.\ 1''\ dA_1-\text{mod.}\cot.\ B_1\sin.\ 1''\ dB+\ .\ .\ .$$

$$=(\log.b)+[\delta_A(A)-\delta_B(B)].$$

where δ_A, δ_B, . . are the log. differences corresponding to 1″ for log. sin. A, log. sin. B, . . in a table of log sines.

This expression is also of the standard form (20). The coefficients of the corrections in these equations taken in connection with those of the condition-equations when substituted in the general formula (19) give the relation required. Having found the mean square error of log. a_n we easily find that of a_n. For

$$d\log.a_n=\frac{\text{mod.}}{a_n}da_n.$$

Hence,

$$\mu^2\log.\ a_n=\left(\frac{\text{mod.}}{a_n}\right)^2\mu^2 a_n.$$

The logarithmic form is the better of the two in practice.

Example 1.—A single triangle.

Here $\qquad a=b\dfrac{\sin.\ A}{\sin.\ B}$,

$$\therefore\quad da=\frac{a}{b}(b)+a\sin.1''[\cot.A(A)-\cot.\ B(B)].$$

First consider the base exact, that is, omit the first term.

We have the condition equation

$$(A)+(B)+(C)=l$$
$$\therefore\ a'=a''=a'''=1.$$

Also,
$$f'=a\cot.A.\ \sin.\ 1''$$
$$f''=-u\cot.\ B.\ \sin.\ 1''$$
$$f'''=0.$$

Hence if p_1, p_2, p_3 denote the weights of the measured angles,

$$[uaa]=u$$
$$[uaf]=u_1a\cot.A.\sin.1''-u_2a\cot.B.\sin.1''$$
$$[uff]=u_1a^2\cot.^2A.\sin.''-u_2a^2\cot.^2B.\sin.'',$$

and

$$v_a=[uff]-\frac{[uaf]^2}{[uaa]}$$

$$=a^2\Big(_1\cot.u^2A+u_2\cot.^2B-\frac{(u_1\cot.A-u_2\cot.B)^2}{[u]}\Big)\sin.^21''\quad(29).$$

If μ is the m. s. e. of a measured angle coresponding to the unit of weight, then

$$\mu_a=\mu\sqrt{v_a}$$

$$=\mu a\sin.1''\sqrt{u_1\cot.^2A+u_2\cot.^2B-\frac{(u_1\cot A-u_2\cot.B)^2}{[u]}}$$

Also if μ_b denotes the m. s. e. of the base b the expression for μ^2_a must be increased by

$$\frac{a^2}{b^2}.\mu^2_b.$$

If the weights p_1, p_2, p_3 are equal, then

$$\mu^2_a=\frac{a^2}{b^2}.\mu^2_b+\frac{2}{3}a^2\mu^2\sin.^2\ 1''(\cot.^2A+\cot.\ ^2B+\cot.\ A\ .\cot.B)\quad(30).$$

and if the triangle is equilateral

$$\mu^2_a=\mu^2_b+\frac{2}{3}a^2\mu^2\sin.^21''\qquad(31).$$

Example 2.—Next consider a chain of triangles, the base being free from error, and the corresponding angles of the several triangles of the same weight.

We have

$$a_1=b.\frac{\sin.A_1}{\sin.B_1}=b\lambda_1\text{ suppose}$$
$$a_2=a_1.\frac{\sin.A_2}{\sin.B_2}=a_1\lambda_2$$
$$=b\lambda_1\lambda_2$$
$$.\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .$$
$$a_n=b.\lambda_1\lambda_2\ .\ .\ \lambda_n$$

By differentiation

$$da_n=a_n\left(\frac{d\lambda_1}{\lambda_1}+\frac{d\lambda_2}{\lambda_2}+\ .\ .\ .\ .\ .\ .\right)$$

$$\therefore\quad\mu^2_{a_n}=a^2_n\left(\frac{\mu^2\lambda_1}{\lambda_1^2}+\frac{\mu^2\lambda_2}{\lambda_2^2}+\ .\ .\ .\ .\right)$$

But from

$$\lambda\mu^2_{\lambda_1}=\mu^2\Big(u_1\cot.^2A_1+u_2\cot.^2B_1-\frac{(u_1\cot.A_1-u_2\cot.B_1)^2}{[u]}\Big)\sin.\ 1'',$$

and since the corresponding angles of the triangles are of the same weight, we have

$$\frac{\mu^2_{\gamma_1}}{\lambda_1^2}=\frac{\mu^2_{\lambda_2}}{\lambda_2^2}=\ .\ .\ .\ .$$

Hence,

$$\mu^2_{a_n} = n.a^2_n \frac{\mu^2_\lambda}{\lambda^2} \qquad (32).$$

that is, in a chain of similar and similarly measured triangles $\frac{\mu_{a_n}}{a_n}$ increases as the square root of the number of triangles in the chain.

If the chain consists of equilateral triangles and the weights of the angles are each equal to unity, then

$$\mu_a = \sqrt{\tfrac{2}{3}}.\ \sin.\ 1''.\mu b\sqrt{n}$$
$$= 0.000004\mu b\sqrt{n} \text{ nearly.} \qquad (33).$$

Hence in a chain of equilateral triangles the weights of the sides decrease as we proceed from the base through the successive triangles, as

$$1,\ \tfrac{1}{2},\ \tfrac{1}{3},\ \tfrac{1}{4},\ldots$$

If in a chain of equilateral triangles the m. s. e. of the base is μ_b, then

$$\mu^2_a = \mu^2_b + \tfrac{2}{3}\,\mu^2 \sin.^2 1''.b^2 n \qquad (34).$$

Now in the measurement of a primary base a precision may be attained such that the m. s. e. of the length does not exceed $\frac{1}{1,000,000}$ part of the length. The m. s. e. of angle of the triangulation may be taken as 0''.25, and therefore the m. s. e. of a side computed through n equilateral triangles is

$$0.000004 \times 0.25\sqrt{n}$$
$$= 0.000001\sqrt{n}$$

part of the side.

Hence it only takes a very few triangles from the base in order that the error arising from the angles exceeds that from the base, and in computing a long chain the base error may be neglected, that is, the measurement of the base may be considered perfect in comparison with the measurement of the angles.

17. A very important principle as to the best form of triangle ABC to be employed in order to reach the greatest precision in the length of a side a computed from another side b as base, may be derived from the equation.*

$$\mu_a = \mu a \sin.'' \sqrt{\tfrac{2}{3}}(\cot.^2 A + \cot.^2 B$$
$$+ \cot. A \cot. B).$$

when μ is the m. s. e. of an observed angle.

* Zachariæ, Die geodätische Hauptpunkte, Berlin, 1876.

If the side c is determined with the same precision as a then,

$$\frac{\mu_c}{c} = \frac{\mu_a}{a},$$

and therefore C=A

and B=180−2A.

Hence,

$$\mu_a = \mu a \sin.\ 1'' \sqrt{\frac{2}{3}}.\ \frac{3 + 4\tan.^4 A}{4\tan.^2 A}.$$

The precision is greatest when this expression is a minimum, that is, when

$$A = 52°\ 46',$$

and then $\mu_a = \frac{3.7}{10^6}.\mu_a.$

With an equilateral triangle on the other hand, $\mu_a = \frac{4.0}{10^6}.\mu_a.$

Since this value of μ_a is greater than the preceding, we might conclude that the above isosceles triangle was the normal form.

But in a chain of triangles similar to this triangle, each would be smaller than the preceding. Thus, since

$$\frac{a}{b} = \frac{\sin.52°46'}{\sin.74°28'} = 0.83,$$

each triangle is 0.7 of the preceding one in the chain, and therefore with a long chain but little progress would be made.

On the other hand, in a chain of equilateral triangles a greater area is covered than in a chain of the same number of scalene triangles. Hence, since with equilateral triangles there is least loss of precision with the greatest covering of ground, we conclude that the equilateral is the normal form.

Solution of the Condition Equations in Groups.

18. The preceding solution is simple, straightforward, and rigorous. It has the serious drawback of being in general very laborious. When the number of condition equations is large, as in a triangulation adjustment, the amount of labor required to make a direct solution would be enormous. Accordingly, methods have been devised of solving the equations in groups. I shall notice two of these: the first, which is due to Bessel, is rigorous; and the second, due to Gauss,

leads to results as accurate as we please through successive approximations.

BESSEL'S METHOD OF SOLUTION.

19. In many kinds of work it happens that the conditions to which observations are subject may be divided into two classes for purposes of solution. The division is arbitrary, as may be seen, for example, in Bessel's and Schleiermacher's* solutions of the same problem in triangulation. Bessel's method, which is the more general, consists in solving the first set of condition-equations by themselves and then utilizing the work in determining the further connections due to the remaining conditions. In this way the work is kept in manageable shape.

Thus let the reduced observation-equations n in number and containing n_u unknowns be

$$\begin{aligned} a_1x+b_1y+\ldots\ldots-l_1&=v_1 \text{ weight } p_1\\ a_2x+b_2y+\ldots\ldots-l_2&=v_2 \quad\text{"}\quad p_2 \quad (35).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

and the condition-equations n_c in number, involving the same unknowns be

$$\begin{aligned} a'x+a''y+\ldots\ldots-l'&=0\\ b'x+b''y+\ldots\ldots-l''&=0 \qquad (36).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

The most probable values of $x, y, \ldots$ are those which satisfy the condition

$$[pv^2]=\text{a minimum}$$

it is required to find them.

20. The total correction to the value of an unknown may be divided into two parts:

1. That depending on the observation-equations.
2. That depending on the condition-equations.

Denote the first by the correction in parenthesis and the second by the numbers 1, 2, ... also in parenthesis, thus

$$\begin{aligned} \text{Total correction } x&=(x)+(1)\\ \text{" \quad " } \quad y&=(y)+(2) \quad (37). \end{aligned}$$

Now overlooking the condition-equations and taking the observation-equations only (x), (y) ... would be found by solving these equations in the usual way. We have, therefore, by reducing all to the same weight for convenience in writing, the normal equations,

$$\begin{aligned} [aa](x)+[ab](y)+\ldots\ldots-[al]&=0\\ [ab](x)+[bb](y)+\ldots\ldots-[bl]&=0 \quad (38).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

The solution of these equations gives (see Chauvenet, equation 50).

$$\begin{aligned} (x)&=[\alpha\alpha][al]+[\alpha\beta][bl]+\ldots\\ (y)&=[\alpha\beta][al]+[\beta\beta][bl]+\ldots \quad (39).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

Again, substituting for v_1, v_2 ... in the minimum equation, we find

$$\begin{aligned} [aa]x^2+2[ab]xy+\ldots-2[al]x&\\ +\ [bb]yy+\ldots-2[bl]y& \quad (40).\\ \ldots\ldots\ldots\ldots\ldots\ldots&\\ +[ll]=\text{a minimum}.& \end{aligned}$$

This is conditioned by (36). Hence the solution is the same as in Art. 7.

Calling I, II ... the correlates of equations (36), we have

$$\begin{aligned} [aa]x+[ab]y+.-[al]-a'\mathrm{I}-b''\mathrm{II}-.&=0\\ [ab]x+[bb]y+.-[bl]-a''\mathrm{I}-b''\mathrm{II}-.&=0\\ \ldots\ldots\ldots\ldots\ldots\ldots& \end{aligned}$$

These equations taken with (37) and (38) give the relations

$$\begin{aligned} [aa](1)+[ab](2)+.&=a'.\mathrm{I}+b'\mathrm{II}+\ldots\\ &=[1] \text{ suppose}\\ [bb](2)+\ldots&=a''\mathrm{I}+b''\mathrm{II}+\ldots\\ &=[2] \text{ suppose } (41).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

Since these equations are of the same form as (38) their solution gives

$$\begin{aligned} (1)&=[\alpha\alpha][1]+[\alpha\beta][2]+\ldots\\ (2)&=[\alpha\beta][1]+[\beta\beta][2]+\ . \quad (42).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

or substituting for [1], [2], .. their values from (41)

$$\begin{aligned} (1)&=\mathrm{A'I}+\mathrm{B'II}+\mathrm{C'III}+\ldots\\ (2)&=\mathrm{A''I}+\mathrm{B''II}+\mathrm{C''III}+\ . \quad (43).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

where

$$\begin{aligned} \mathrm{A}'&=[\alpha\alpha]a'+[\alpha\beta]a''+\ldots\\ \mathrm{B}'&=[\alpha\alpha]b'+[\alpha\beta]b''+\ldots \quad (44).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

We have $\therefore$ expressed the condition-corrections in terms of the correlates. Substituting for x, y ... their values from (37) in the condition-equations and we find

$$\begin{aligned} a'(1)+a''(2)+\ldots-l'_0&=0\\ b'(1)+b''(2)+\ldots-l''_0&=0 \quad (45).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

where

$$\begin{aligned} -l_0'&=a'(x)+a''(y)+\ldots-l'\\ -l_0''&=b'(x)+b''(y)+\ldots-l'' \quad (46).\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

* See Fischer's Geodasie, Part III.

The values of l_e', l_e''..are known since (x), (y)..are known.

Substitute the values of (1), (2) ..from (43) in (45) and we have the correlate normal equations,

$$\begin{aligned}[\overline{aA}]\mathrm{I}+[\overline{aB}]\mathrm{II}+..-l_e'&=0\\ [\overline{aB}]\mathrm{I}+[\overline{bB}]\mathrm{II}+..-l_e''&=0 \qquad (47).\\ &.................\end{aligned}$$

where

$$\begin{aligned}[\overline{aA}]&=[\overline{\alpha\alpha}]a'a'+[\overline{\alpha\beta}]a'b'+..\\ &+[\overline{\alpha\beta}]a'b'+[\overline{\beta\beta}]b'b'+..\\ &.................\\ \&c.&=\&c.\end{aligned}$$

The solution of these equations gives the correlates I, II...

21. In carrying the preceding solution into practice, the following order of procedure will be found convenient:

(1). The formation and solution of the observation-equations (35). The partially adjusted resulting values (x), (y)...are now to be used.

(2). The formation of the condition-equations.

$$\begin{aligned}a'(1)+a''(2)+...-l_e'&=0\\ b'(1)+b''(2)+...-l_e''&=0\\ &.................\end{aligned}$$

(3). The formation of the weight-equations. They are at once written down from the general solution of the observation-equation equations, and are

$$\begin{aligned}(1)&=[\alpha\alpha][1]+[\alpha\beta][2]+...\\ (2)&=[\alpha\beta][1]+[\beta\beta][2]+..\\ &.................\end{aligned}$$

(4). The formation of the correlate equations,

$$\begin{aligned}[1]&=a'.\mathrm{I}+b'\mathrm{II}+..\\ [2]&=a''\mathrm{I}+b''\mathrm{II}+..\\ &.................\end{aligned}$$

(5). The expression of the corrections in terms of the correlates by substituting from (4) in (3),

$$\begin{aligned}(1)&=\mathrm{A}'.\mathrm{I}+\mathrm{B}'\mathrm{II}+..\\ (2)&=\mathrm{A}''\mathrm{I}+\mathrm{B}''\mathrm{II}+..\end{aligned}$$

(6). The formation of the normal equations by substituting from (5) in (2),

$$\begin{aligned}[\overline{aA}]\mathrm{I}+[\overline{aB}]\mathrm{II}+..-l_e'&=0\\ [\overline{aB}]\mathrm{I}+[\overline{bB}]\mathrm{II}+..-l_e''&=0\\ &.................\end{aligned}$$

(7). The determination of the corrections by substituting the values of the correlates in (5).

22.—*To find the m. s. e. of an observation of weight unity.*

We have

$$\mu^2=\frac{[vv]}{n-(n_u-n_e)} \qquad (48).$$

where n is the number of observed quantities and n_u-n_e the number of independent unknowns. In practice it is better to write the denominator in the form

$$(n-n_u)+n_e,$$

$n-n_u$ being the number of superfluous observation-equations and n_e the number of condition-equations.

Let v_1°, v_2°...denote the residual errors arising from taking the observation-equation only, then

$$\begin{aligned}v_1^\circ&=a'(x)+b'(y)+...-l'\\ v_2^\circ&=a''(x)+b''(y)+..-l'' \qquad (49).\\ &.................\end{aligned}$$

and from the normal equations (38)

$$[av^\circ]=[bv^\circ]=..=0$$

that is (see Chauvenet p. 512),

$[v^\circ v^\circ]$ is formed in the usual way from the observation-equations.

Again,

$$\begin{aligned}v_1&=a_1[(x)+(1)]+b_1[(y)+(2)]+..\\ &=v_1^\circ+a_1(1)+b_1(2)+..\\ v_2&=v_2^\circ+a_2(1)+b_2(2)+.. \qquad (50).\\ &.................\end{aligned}$$

Hence,

$$\begin{aligned}[vv]&=(v^\circ v^\circ)+[(a(1)+b(2)+..)^2]+\\ &\qquad 2(av^\circ)(1)+2(bv^\circ)(2)+..\\ &=[v^\circ v^\circ]+[\mathrm{WW}]\text{ suppose.} \qquad (51).\end{aligned}$$

where

$$\begin{aligned}[\mathrm{WW}]&=[(a(1)+b(2)+..)^2]\\ &=(1)[(aa)(1)+(ab)(2)+.]\\ &+(2)[ab(1)+(bb)(2)+...]\\ &+.................\\ &=(1)[1]+(2)[2]+...... \qquad (52).\end{aligned}$$

from (41).

Substitute for (1) [1], (2) [2]... their values from equations 41, 43 and expand: then

$$\begin{aligned}[\mathrm{WW}]&=[[\overline{aA}]1+[\overline{aB}]11+..]\mathrm{I}\\ &+[\overline{aB}]1+[\overline{bB}]11+...]\mathrm{II}\\ &+.................\end{aligned}$$

which may be transformed as in Art. 9 into the form

$$[\mathrm{WW}]=\frac{l_e'^2}{[aA]}+\frac{l_e''^2}{(bB1]}+\frac{l_e'''^2}{[cC2]}+... \qquad (53).$$

or from equation (47) into the form

$$[\mathrm{WW}]=l_e'\mathrm{I}+l_e''\mathrm{II}+... \qquad (54).$$

23.—*To find the weight of a function of the adjusted values.*

Let the function reduced to the linear form be

$$dF = g_1 x + g_2 y + \ldots . \quad (55).$$

Put for $x, y, \ldots$ their values $(x)+(1)$, $(y)+(2), \ldots$ then

$$dF = g_1(x) + g_2(y) + \ldots - g_1(1) + g_2(2) + \ldots$$

Substitute for (1), (2).. their values from (43) and

$$dF = g_1(x) + g_2(y) + \ldots + [gA]\mathrm{I} + [gB]\mathrm{II} + .$$

where I, II,.. satisfy the equations

$$[\overline{aA}]\mathrm{I} + [\overline{aB}]\mathrm{II} + \ldots - l_0' = 0$$
$$[\overline{aB}]\mathrm{I} + [\overline{bB}]\mathrm{II} + \ldots - l_0'' = 0 \quad (47).$$
$$\ldots\ldots\ldots\ldots\ldots$$

Hence using the undetermined multipliers $k_1, k_2 \ldots$ in order to eliminate the correlates I, II,... we have as in (3)

$$dF = g_1(x) + g_2(y) + \ldots l_0'k_1 + l_0''k_2 + \ldots$$
$$+ [(gA) - (\overline{aA})k_1 - (\overline{aB})k_2 - \ldots]\mathrm{I} +$$
$$[(gB) - (\overline{aB})k_1 - (\overline{bB})k_2 \ldots]\mathrm{II} + \ldots \quad (56).$$

We may determine $k_1, k_2 \ldots$ so as to satisfy

$$[\overline{aA}]k_1 + [\overline{aB}]k_2 + \ldots - [gA] = 0$$
$$[\overline{aB}]k_1 + [\overline{bB}]k_2 + \ldots - [gB] = 0 \quad (57).$$
$$\ldots\ldots\ldots\ldots\ldots$$

and then

$$dF + g_1(x) + g_2(y) + \ldots l_0'k_1 + l_0''k_2 + . \quad (58).$$

Substitute for $l_0', l_0'' \ldots$ their values from (46) and

$$dF = g_1(x) + g_2(y) + \ldots + k_1[l' - a'(x) - b'(y) \ .] + k_2[l'' - a''(x) - b''(y) \ldots] + \ldots$$
$$= (lk) + (x)(g_1 - a'k_1 - a''k_2 \ldots) + (y)(g_2 - b'k_1 - b''k_2 \ldots) + \ldots .$$
$$= (lk) + G_1(x) + G_2(y) + \ldots \text{suppose.} \quad (59).$$

where
$$G_1 = g_1 - a'k - a''k_2 - \ldots$$
$$G_2 = g_2 - b'k_1 - b''k_2 - \ldots \quad (60).$$
$$\ldots\ldots\ldots\ldots\ldots$$

Since $(x), (y) \ldots$ satisfy the equations

$$[aa](x) + [ab](y) + \ldots = [al] \quad (38).$$
$$[ab](x) + [bb](y) + \ldots = [bl]$$
$$\ldots\ldots\ldots + \ldots$$

the problem is reduced to Art. 12.

Hence if U is the reciprocal of the required weight,

$$U = [GQ] \quad (61).$$

when

$$Q_1 = [\alpha\alpha]G_1 + [\alpha\beta]G_2 + \ldots$$
$$Q_2 = [\alpha\beta]G_1 + [\beta\beta]G_2 + \ldots \quad (62).$$
$$\ldots\ldots\ldots\ldots\ldots$$

Putting for $G_1, G_2 \ldots$ their values from (61), we find

$$Q_1 = q_1 - A_1'k_1 - B_1'k_2 - \ldots$$
$$Q_2 = q_2 - A''k_1 - B''k_2 - \ldots \quad (63).$$
$$\ldots\ldots\ldots\ldots\ldots$$

when

$$q_1 = [\alpha\alpha]g_1 + [\alpha\beta]g_2 + \ldots$$
$$q_2 = [\alpha\beta]g_1 + [\beta\beta]g_2 + . \quad (64).$$
$$\ldots\ldots\ldots\ldots\ldots$$

Hence substituting for $G_1, G_2 \ldots Q_1, Q_2 \ldots$ their values,

$$[GQ] = [gq] - [gA]k_1 - [gB]k_2 - \ldots$$
$$- [aq]k_1 - [bq]k_2 - \ldots$$
$$+ [[\overline{aA}]k_1 + [\overline{aB}]k_2 + .]k_1$$
$$+ [[\overline{aB}]k_1 + [\overline{bB}]k_2 + .]k_2$$
$$+ \ldots\ldots \quad \ldots\ldots\ldots \quad (65).$$

But from (44) and (64)

$$[aq] = [gA], \ [bq] = [gB], \ldots$$

Hence attending to equation (57) the above expression reduces to

$$[GQ] = [gq] - [gA]k_1 - [gB]k_2 \ldots \quad (66).$$

24. We may, however, put equation (66) in a form more convenient for computation.

For substitute for $[gA]$, $[gB]$,.. their values from (57) and

$$[GQ] = [gq] - [[\overline{aA}]k_1 + [\overline{aB}]k_2 + \ldots]k_1$$
$$- [[\overline{aB}]k_1 + [\overline{bB}]k_2 + \ldots]k_2$$
$$- \ldots\ldots\ldots\ldots\ldots$$

which may be reduced to the form (see Art. 13),

$$[GQ] = [gq] - \frac{[gA]^2}{[aA]} - \frac{[gB1]^2}{[bB1]} - \frac{[gC2]^2}{[cC2]} - \ldots \quad (67).$$

25. The form of the last expression for [GQ] suggests that it may be found from the following scheme, adding [Ag], [Bg], ... as an extra column in the solution of the normal equations. It is analogous to Art. 15.

$$\begin{array}{cccccc}
\mathrm{I.} & \mathrm{II.} & \mathrm{III.} & & \\
[aA] & [aB] & [aC] & [Ag] & \\
 & [bB] & [bC] & [Bg] & \\
 & & [cC] & [Cg] & [gq] \\
\end{array}$$
$$\ldots\ldots\ldots\ldots\ldots$$

Also
$$\mu_F = \mu\sqrt{U}$$
$$= \mu\sqrt{[GQ]} \quad (71).$$

where μ is the m. s. e. of an observation of weight unity.

26.—*Check of* (gq).

From equations (64) by transposition

$$g_1 = [aa]q_1 + [ab]q_2 + \ldots$$
$$g_2 = [ab]q_1 + [bb]q_2 + \ldots \quad (72).$$
$$\ldots\ldots\ldots\ldots\ldots$$

Hence multiplying in order by q_1, q_2.. and adding, we have

$$[gq]=[aa]q_1\,q_1+2[ab]q_1q_2+\,..\\ \qquad +[bb]q_2q_2+\,...$$

$$\cdots\cdots\cdots\cdots\cdots\cdots$$

$$=\frac{l'^2}{[aa]}+\frac{l''1^2}{[bb1]}+\frac{l'''2^2}{[cc2]}+\,.. \qquad (73).$$

when $[aa]$, $[bb1]$,... are found from the observation-equations.

2.—Gauss' Method of Solution by Successive Approximation.

27. It may be stated as follows:

A large number of condition-equations being given for solution by the method of least squares we may divide these equations into groups, and adjust the groups separately in any order we please, when the values found from the successive groups will be closer and closer approximations to the most probable values as found from the simultaneous adjustment of all of the groups.

For suppose we have the condition-equations

$$\left.\begin{array}{l} a'v_1+a''v_2+\,..-l_a=0\\ b'v_1+b''v_2+\,..\,-l_b=0\\ \cdots\cdots\cdots\cdots\\ h'v_1+h''v_2+\,..-l_h=0\\ k'v_1+k''v_2+\,..=l_k=0\\ \cdots\cdots\cdots\cdots \end{array}\right\} \qquad (74).$$

with

$$[pv^2]=\text{a min.}$$

Let v_1', v_2'.. be the values of v_1, v_2... obtained from solving the first group alone, that is from

$$\left.\begin{array}{l} a'v_1'+a''v_2'+\,..-l_a=0\\ b'v_1'+b''v_2'+\,..-l_b=0\\ \cdots\cdots\cdots\cdots \end{array}\right\} \qquad (75).$$

$$pv'^2]=\text{a min.}$$

If now (v_1'), (v_2').. are the corrections to these values resulting from the remaining equations, then since

$$(pv^2)=[p[(v'+(v')]^2]\\ =[p\,v'^2]+[(pv')^2]$$

The condition-equations are reduced to

$$\left.\begin{array}{l} a'(v_1')+a''(v_2')+\,..-l'_a\\ b'(v_1')+b''(v_2')+\,..-l'_b\\ \cdots\cdots\cdots\cdots\\ h'(v_1')+h''(v_2')+\,..-l'_h\\ k'(v_1')+k''(v_2')+\,..-l'_k\\ \cdots\cdots\cdots\cdots \end{array}\right\} \qquad (76).$$

with $\qquad [p(v')^2]=\text{a min.}$

and the values of (v') found from the simultaneous solution of these equations added to the values of v' found from the solution of the first set would be equal to the value of v found directly. Similarly if v_1'', v_2''.. be the values of (v') obtained by solving the second set alone and (v_1''), (v_2'')... be the corrections to these values resulting from the remaining equations, then since

$$pv^2=[pv'^2]+[pv''^2]+[p(v'')^2]$$

the condition-equations are reduced to

$$\left.\begin{array}{l} a'(v'')+a''(v'')+\,..-l''_a\\ b'(v'')+b''(v'')+\,..-l''_a\\ \cdots\cdots\cdots\cdots\\ h'(v'')+h''(v'')+\,..-l_h\\ k'(v'')+k''(v'')+\,..-l_k\\ \cdots\cdots\cdots\cdots \end{array}\right\} \qquad (77).$$

with $\qquad [p(v'')^2]=\text{a min.}$

The quantities $[pv'^2]$, $[pv''^2]$, ... being positive, the minimum equation is reduced with the solution of each set, and thus we gradually approach the most probable set of values. Beginning with the first set a second time and solving through again we should reduce the minimum equation still farther, and by continuing the process we shall finally reach the same result as that obtained from the rigorous solution. In practice the first approximation is in general close enough.

It is plain that the most probable values can be found after any approximation, by solving simultaneously the whole of the groups, using the values already found as approximations to the most probable values.

28. As an example we may cite the reduction of the principal triangulation of the English Ordnance Survey. Here there were 920 condition-equations, and therefore if the whole were to be reduced at once the solution of 920 equations would be necessary. The following was the method employed: "The triangulation was divided into a number of parts or figures, each affording a not unmanageable number of equations of condition. One of these being corrected or computed independently of all the rest, the corrections so obtained were substituted (so far as they entered) in the equations of condition of the next figure and the sum of the squares of the remaining corrections in that figure made a minimum.

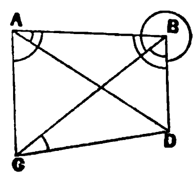
The corrections thus obtained for the second figure were substituted in the third, and so on."*

ADJUSTMENT OF A TRIANGULATION.

29. As already stated this is a special case of the method of condition-observations. I shall treat an example according to the strict principles already laid down and then pass on to the American methods, which are more or less approximate, in some of their parts at least.

The preliminary work for the formation of the condition-equations must first be explained. This will be given in some detail and will contain the results of considerable research and experience in the subject.

30. In a triangulation there must be at least one measured base as AB. Starting from this and measuring the angles CAB, ABC, we may compute the sides AC, BC by ordinary trigonometry. In



plotting the figure the point C can be located in but one way. Having only the necessary measurements there are no contradictions and therefore no room for adjustment.

Similarly by measuring the angles CBD, DCB, we may compute the sides DB, DC and the point D can be plotted in but one way.

If, however, while occupying the station A, the observer had read the angle BAD, then in plotting the figure we should find in almost all cases that the lines AD, CD, BD would not intersect in the same point. In computing the side BD or CD from AB, we should find different values according to the triangles through which we passed.

If additional angles were measured the case would be more complex. Thus if the whole angle ABD had been measured, we should have a contradiction arising from the fact that the measured value of ABD would not be equal to the sum of the measured values of ABC, CBD.

* Account of the Main Triangulation p. 272.

So if the blunt angle DBA had also been measured, we should have another contradiction arising from the non-satisfaction of the relation

$$ABC + CBD + DBA = 360°.$$

And not only this. For in any triangle we have considered so far, that only two angles were measured. If in the first triangle the angle BCA were also measured, we know from spherical geometry that the three angles should satisfy the relation

$$CAB + ABC + BCA = 180 + \text{sph. excess of triangle},$$

which the measured angles will not do.

So with the other triangles.

The number of contradictions to be removed will depend on the measurements made. The more unsystematic these have been the greater will be that number. We can conceive that the angles can be so changed as to satisfy all the conditions required, and that this can be done in an infinite number of ways. The method of least squares will show us how to pick out the most probable values from this infinite number of possible values.

31. In a triangulation then with one measured base in which the sides are to be computed from this base, using the most probable values of the angles, we canclude that these angles must be adjusted for two classes of conditions:

(1.) Those arising at each station from the relations of the angles to one another at that station. These are known as *local* conditions and the failure of the observed angles to satisfy these conditions arise mainly from imperfect elimination of instrumental errors and errors of observation.

(2.) Those arising from the geometrical conditions demanded by the figure,

(*a*) that the sum of the angle of each triangle shall be equal to 180° increased by the spherical excess of the triangle.

(*b*) that the length of any side as computed from the base is the same whatever route is chosen.

These are known as *general* conditions, and the failure of the observed angles to satisfy these conditions arises mainly from errors of observation and when the

lines are short from imperfect centering of instrument and when the lines are long from horizontal refraction, &c.

The general statement of the method of solution is this: Adjust the angles so as to satisfy simultaneously the local and general conditions. The method of least squares shows that of all possible systems of corrections to the observed quantities which satisfy these conditions, the most probable is that which makes the sum of the squares of these corrections a minimum.

32. The form of the reduction depends on the method employed in making the observations. To make this clear we shall explain the two principal methods of measuring angles, confining ourselves to non-repeating theodolites read with microscopes. These are the method of independent angles and the method of directions, an angle being the difference of two directions.

(1.) *The method of independent angles.* This method was developed by Gauss,* and may be used either with a repeating or a non-repeating theodolite.

The mode of measurement is the same as in the method of directions, the number of signals read to in any case being limited to two.

(2.) *The method of directions.* This was recommended by Gauss, but its developement is mainly due to Bessel, Hansen and Andrae.

The telescope being in position, it is directed to a signal and the micrometers read. Similar pointings and readings are made to each of the signals in the order that they are ranged round the horizon. The telescope is now reversed, that is, it is turned 180° in the azimuth and 180° in altitude leaving the same pivots in the same wyes, and the observations are made in the reverse order, beginning with the last signal and ending with the first. These readings differ from the former corresponding ones by 180° nearly. The means from the two positions of the telescope give the directions of the signals, relative to one of them taken as an initial station by subtracting the mean for the initial station from the mean for each of the others. These means are free from errors arising from lack of collimation of the instrument, from inequality in the heights of the wyes, and from any uniform twisting of the instrument on its support.

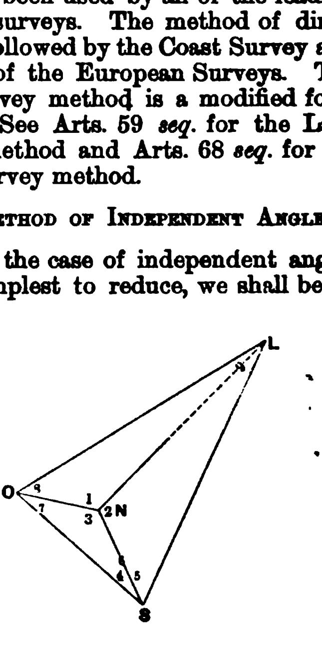
The reading of the limb on the first signal is changed by some aliquot part of the distance between consecutive microscopes, if their number is even, whenever the telescope is transitted, the same number of means being obtained on each of the equal arcs making up that distance. If the number of microscopes is odd, the circle should be shifted by an aliquot part of the half distance between consecutive microscopes after every two sets of measures with the telescope direct and reversed, the same number of measures being obtained on each of the equal arcs making up that half distance.

A single series of means is called an "arc" (*satz: mise*). It is usually the case that the conditions are such that it is not possible to measure complete *arcs* only. To save time, the stations are sighted at just as they happen to be visible, the initial station being changed as often as may be necessary. This, though economy in the field, causes a great amount of extra work in the reduction, as we shall see presently.

33. One or other of the above methods of measuring angles with slight modifications has been used by all of the leading modern surveys. The method of directions is followed by the Coast Survey and by most of the European Surveys. The Lake Survey method is a modified form of this. See Arts. 59 *seq.* for the Lake Survey method and Arts. 68 *seq.* for the Coast Survey method.

The Method of Independent Angles.

34. As the case of independent angles is the simplest to reduce, we shall begin with it.



I shall for illustration take the following example, making use of such parts of

* Astrom. Nachr., 2117.

it from time to time as may properly belong to the subject in hand.

In the triangulation of Lake Superior the following angles were measured in the quadrilateral N. Base, S. Base, Lester, Oneota.

			°	′	″		
ONL	=	M_1 =	124	09	40.69	weight	2
LNS	=	M_2 =	113	39	05.07	"	2
SNO	=	M_3 =	122	11	15.61	"	14
OSN	=	M_4 =	23	08	05.26	"	23
NSL	=	M_5 =	47	31	20.41	"	6
OSL	=	M_6 =	70	39	24.60	"	7
NOS	=	M_7 =	34	40	39.66	"	31
LON	=	M_8 =	43	46	26.40	"	1
SLO	=	M_9 =	30	53	30.81	"	8

The length of the line N. Base—S. Base is 6056^m.6 and the latitudes of the four stations are

N. Base	46°	46′
S. Base	46	43
Lester	46	52
Oneota	46	45

THE LOCAL OR STATION-ADJUSTMENT.

35. When in a system of triangulation the horizontal angles read at a station are adjusted for all of the conditions existing among the angles at the station then these angles are said to be locally adjusted.

From the considerations set forth in 31 it is readily seen that at a station only two kinds of conditions are possible,

(*a*) that an angle can be formed from two or more others, and

(*b*) that the sum of the angles round the horizon should be equal to 360°. The second of these is included in the first and the method of adjustment may be stated in general terms as follows:

An inspection of the figure representing the angles at the station will show how all of the measured angles can be expressed in terms of a certain member of them which are independent of one another. It is to be kept in mind that angles at a station may be observed independently of one another and yet not be independent of one another in the reduction.

If then M_1, M_2,... denote the single measured angles and v_1, v_2,... their most probable corrections then if any of them M_h, M_k can be formed from others we have by equating the measured and computed values the conditions

$$M_h + v_h = M_1 + v_1 + M_2 + v_2 + \ldots$$
$$M_k + v_k = M_1 + v_2 + M_2 + v_2 + \ldots$$
$$\ldots\ldots\ldots\ldots\ldots$$

or
$$v_1 + v_2 + \ldots - v_h = l_h \text{ suppose}$$
$$v_1 + v_2 + \ldots - v_k = l_k \quad " \qquad (78).$$
$$\ldots\ldots\ldots\ldots\ldots$$

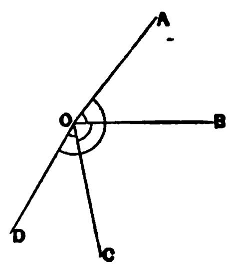
with

$$p_1 v_1^2 + p_2 v_2^2 + \ldots + p_h v_h^2 + p_k v_k^2 + \ldots = \text{a minimum},$$

where p_1, p_2,... denote the weights of the angles.

The solution may be carried out by the method of correlates as in Chauvenet, Art. 55. The following special cases are of frequent occurrence:

(1). At a station O the $m-1$ single angles AOB, BOC,... are measured and also the sum — angle AOL, to find the adjusted values of the separate angles all of them being of the same weight.



The condition-equation is

$$M_1 + v_1 + M_2 + v_2 + \ldots + M_{m-1} + v_{m-1} = M_n + v_m$$

or

$$v_1 + v_2 + \ldots + v_{m-1} - v_m = M_m - (M_1 + M_2 + \ldots + M_{m-1}), = l$$

with $\qquad (v^2) = \text{a minimum}.$

The solution gives

$$v_1 = v_2 = \ldots = -v_m = \frac{l}{m},$$

that is, the correction to each angle is $\frac{1}{m}$ of the discrepancy and the sign of the correction to the sum angle is opposite to that of the single angles.

(2). At a station O the m single angles AOB, BOC,... LOA are measured thus closing the horizon, to find the adjusted values of the angles.

The condition-equation is

$$v_1+v_2+\ldots+v_m=360-(M_1+M_2+\ldots+M_m)$$
$$=l \text{ suppose,}$$

with $[pv^2]=$a minimum.

The solution gives

$$v_1=u_1.\frac{l}{[u]}$$
$$v_2=u_2.\frac{l}{[u]},$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$

where $u_1=\frac{1}{p_1}$, $u_2=\frac{1}{p_2}$.. and $[u]=\left[\frac{1}{p}\right]$.

If the weights are equal, then

$$v_1=v_2=\ldots=\frac{l}{m},$$

that is, the correction to each angle is $\frac{1}{m}$ of the discrepancy.

Example.—The angles at station N. Base close the horizon: required to adjust them.

We have

$$\begin{array}{ll} & \;\;\;\;\;\;\;\;\;\;\;\;\;\;\;^\circ\;\;\;'\;\;\;\;'' \\ M_1+v_1=124\ 09\ 40.69+v_1 & \text{weight } 2 \\ M_2+v_2=113\ 39\ 05.07+v_2 & \;\;\;\;\text{“}\;\;\;\; 2 \\ M_3+v_3=122\ 11\ 15.61+v_3 & \;\;\;\;\text{“}\;\;\;\; 14 \end{array}$$

$$\text{sum } 360\ 00\ 01.37+v_1+v_2+v_3$$
$$\text{Theoret. sum } 360\ 00\ 00.00$$

$\therefore$ Horizon eq. is $0 = 1.37+v_1+v_2+v_3$.

Hence,

$$v_1=-\frac{\frac{1}{2}}{\frac{1}{2}+\frac{1}{2}+\frac{1}{14}}\times 1.37,$$
$$=-0.64$$
$$v_2=-0.64$$
$$v_3==0.09$$

and the adjusted angles are

$$\begin{array}{c} ^\circ\;\;\;'\;\;\;\;'' \\ 124\ 09\ 40.05 \\ 113\ 39\ 04.43 \\ 122\ 11\ 15.52 \end{array}$$

Check sum $= 360\ 00\ 00.00$

Example.—Precisely as in the preceding we may deduce at station South Base,

Measured Angles.		Weights.		Adjusted Angles.
° ′ ″				° ′ ″
23 08 05.26	..	23	..	23 08 05.13
47 31 20.41	..	6	..	47 31 19.91
74 39 24.60	..	7	..	70 39 25.04

THE GENERAL ADJUSTMENT.

36. With a single measured base the number of conditions arising from the geometrical relations existing among the different parts of a triangulation net can be readily estimated. For if the net contains s stations two are known being the end points of the base and $s-2$ are to be found.

Now since two angles determine a point, $2(s-2)$ are necessary to fix the $s-2$ points. Hence if n is the total number of measured angles, the number of superfluous ones, that is the number of conditions is

$$n-2s+4.$$

37.—*The Angle Equations.*

The sum of the angles of a triangle drawn on a plane surface is equal to 180°. The sum of the angles of a spherical triangle exceeds 180° by the spherical excess of the triangle, which latter is found from the relation*

$$\varepsilon=\frac{\text{area of triangle}}{R^2.\sin.1''},$$

R being the radius of the sphere.

From extended surveys carried on during the past two centuries the earth has been found to be nearly spheroidal in form and its dimensions have been determined pretty closely.

Now a spheroidal triangle of moderate size may be computed as a spherical triangle on a tangent sphere whose radius is

$$\sqrt{\rho_1\rho_2},$$

when ρ_1, ρ_2 are the radii of curvature of the meridian and of the normal section perpendicular to the meridian respectively, at the point corresponding to the mean of the latitudes φ of the triangle vertices.

Hence we may wrap one triangulation on the spheroid in question by conforming it to the spherical excess computed from the formula

$$\varepsilon \text{ (in seconds)}=\frac{ab\sin.C}{2\rho_1\rho_2\sin.1''} \quad . \quad (79).$$

when $a.b$ are two sides and C is the included angle of the triangle. For convenience of computation we may write

$$\varepsilon=\lambda.\ ab\sin.C$$

when log. λ may be tabulated for the argument φ.

*Todhunter's Spherical Trigonometry, Art. 106.

To find a, b, φ, a preliminary geodetic computation must first be made of the triangulation to be adjusted, starting from a base or from a known side. The values found from using the unadjusted angles will be close enough for finding ε. The latitudes need only be computed to the nearest minute.

Any gross error may be checked by the relation:

Spherical excess in seconds $= \frac{1}{200}$ area of triangle in sq. kilometers.

Another useful check results from the principle that the sums of the excesses of triangles covering the same figure should be equal.

In our example the spherical excesses of the triangles ONS, SNL, ONL, will be found to be 0″.06, 0″.19, 0″.12, respectively.

In each single triangle then the condition required to wrap it on the spheroid, that is, that the sum of the three measured angles shall be equal to 180° together with the spherical excess gives a condition-equation. This is called an *angle-equation*, or by some a *triangle-equation*.

Example.—In the triangle N. Base, S. Base, Oneota, if v_3, v_4, v_8 denote the corrections to the three angles, we have for the most probable values:

$$
\begin{array}{rl}
 & \;\;\circ \;\;\; ' \;\;\;\; '' \\
 & 122\;11\;15.61 + v_3 \\
 & 23\;08\;05.26 + v_4 \\
 & 34\;40\;39.66 + v_8 \\
\hline
 & 180\;00\;00.53 + v_3 + v_4 + v_8 \\
\text{Theoretical sum} & 180\;00\;00.06 \\
\hline
\end{array}
$$

and the angle-equation is

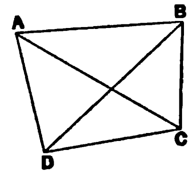
$$0 = 0.47 + v_3 + v_4 + v_8$$

There is no other relation independent of this existing among the angles, and if one side is taken as base the other sides can be found from it in but one way.

38. Complex figures must be treated by considering their component triangles. Thus, let us take the quadrilateral ABCD, in which all right angles are measured. If the spherical excesses of the triangles ABC, BCD, CDA, DAB, be computed, we have the relations:

Sum of angles of

$$
\begin{aligned}
ABC &= 180 + \varepsilon_1 \\
BCD &= 180 + \varepsilon_2 \\
CDA &= 180 + \varepsilon_3 \\
DAB &= 180 + \varepsilon_4 \\
ABCD &= 360 + \tfrac{1}{2}(\varepsilon_1 + \varepsilon_2 + \varepsilon_3 + \varepsilon_4)
\end{aligned}
$$



Only three of these conditions however are independent, since from any three the others necessarily follow. The three independent conditions may be selected from the five conditions in

$$\frac{5.4.3}{1.2.3} = 10$$

ways, of which however only 8 can be used, since the two

ABD, BCD, ABCD
ADC, ABC, ABCD

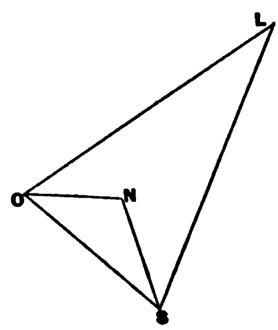
contain really two and not three different conditions.

The eight sets of polygons which can be chosen are:

ABD,	ABD,	ABD,	BAD,
ABC,	ABC,	ADC,	BCD,
ADC,	ABCD,	ABCD,	BAC,
BCD,	CBD,	CBD,	DBA
BAC,	CAB,	CDA,	DCB
ABCD,	CDA,	ABCD,	DCA

In selecting the three conditions it is in general most convenient to neglect those containing ABCD.

As another example let us take our Lake Superior quadrilateral. If all of the



angles had been measured any three of the four triangles would have furnished

independent angle-equations. But as the line NL has not been sighted over from L, we have only two angle-equations, namely, those from the triangles ONS and OLS, just the same as if the figure had been of the form.

39. It is to be expected that in a triangulation net some of the lines will be measured over in both directions, and some in only one direction. If these latter are omitted the number of angle-equations will remain the same. In general we may estimate the number of independent angle-equations in any net as follows:

If s_1 is the number of stations occupied, the polygon forming the outline of the net will give one angle-equation. Each diagonal will give an angle-equation. Hence, if there are l_1 lines in all the number of diagonals will be l_1-s_1 and the *number of angle-equations*

$$l_1-s_1+1 \quad . \quad . \quad . \quad . \quad . \quad (80).$$

REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA. Record of regular meeting, March 17th, 1883.

President Henry G. Morris in the Chair. Mr. Chas. A. Ashburner read a paper on "A New Method of Estimating the Contents of Highly Plicated Coal Beds as Applied to the Anthracite Fields of Pennsylvania." The questions of the future production and ultimate exhaustion of these fields, are of the greatest importance. In 1860 the population of the United States was 31,443,321, and 8,513,123 tons of coal were produced, *i. e.*, actually shipped to market; in 1870 the population had increased 22 per cent. (38,558,371) and the production of anthracite was dearly doubled, being 16,182,191 tons. In 1880, with a population of over 50 millions, the product was 23,437,242+tons. In 1882 the actual production was over 30,000,000 tons. It has been variously estimated that the 470 square miles containing this coal in Pennsylvania, will be entirely exhausted in from 140 to 204 years. While Mr. Ashburner does not estimate the ultimate exhaustion, he has devised a method for estimating the contents of these fields, from data now being obtained by the careful and practical geological and mining examinations of the State Survey. The exact position and detailed structural shape of the coal beds are first mapped by 50 feet contour lines along the floor of the beds, giving, completely and satisfactorily, their geometrical construction and shape. These surfaces are then developed into planes, by the development into straight lines of the line of the bed as cut by paralleled section planes 1600 feet apart. This graphical method is attended with errors which are mathematically discussed, and which have been formulated by Mr. Arthur Winslow, Member of the Club. This method does not give the true area of the surface of a sphere, cone or triangular trough. In the case of a sphere, it gives $\frac{\pi}{4}$ of the true area; in a cone, the error increases directly as the secant of the angle which the pitch of the cone makes with its axis; and in a triangular trough, which more nearly represents the shape of the anthracite basins, the error is very much less. A practical test has been made of this method in the Panther Creek basin, between Mauch Chunk and Tamaqua, and the maximum possible error in estimating the surface area of the coal beds was found to be .905 of 1 per cent. After the areas are thus found, the contents are obtained by careful measurements made in the mines to ascertain the actual number of tons of coal which are contained in a unit (1 acre) of bed area. In this way it has been estimated that the above basin originally contained 1,082,000,000 ± tons; that the area under development originally contained 92,000,000 ± tons, out of which latter area 54,000,000 ± tons have been taken.

The Secretary presented, for M. John Marston, an illustrated set of formulæ for railroad turnouts and crossings.

Mr. John T. Boyd exhibited ribbons of phosphor-bronze with which he had experimented with a view to its use for tape lines, in mine, shop and other work, where the danger of breaking the ordinary steel lines is very great, and where the contact of tape with substances, in themselves injurious to it, renders frequent wiping, and consequent scouring off of the figures, necessary. The phosphor-bronze ribbon was found to be extremely tough, but, in addition to the difficulty in its manufacture into this shape, it was found that after it was bent at a sharp angle, it would not straighten out, thus decreasing the length of the line. As using the hammer to straighten it would increase the length, the experiment was not prosecuted further.

The Secretary presented a system of reduction tables which he had made to facilitate long and tedious multiplications and divisions.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The regular meeting of the Society was held Wednesday evening, March 21st, Vice-President Wm. H. Paine in the Chair, John Bogart, Secretary.

The death on March 8th was announced of James O. Morse, one of the earliest members, and who had been Secretary of the Society for 15 years, and Treasurer 21 years. An interesting collection of specimens of native wood was presented by John M. Goodwin, member of the Society. The subject of a continuance of tests of structural materials was considered. The Secretary made a statement of what had been done up to the present. Mr. O. Chanute, Chairman of a Committee on this subject, related the effort that had been made to secure larger appropriations from Congress; and the subject of the best method for conducting and continuing tests and of collating results so as to secure desirable information, was discussed

by Messrs. A. P. Boller, L. L. Buck, John Bogart, O. Chanute, T. C. Clark, Theodore Cooper, Charles E. Emery, Robert L. Harris, Charles Macdonald, Wm. H. Paine, and S. H. Shreve. Letters were read from General S. V. Benét, Chief of Ordnance, stating that the programme adopted for continuing tests of structural materials would be carried out on the Watertown Testing Machine to the extent of the very small amount appropriated by Congress, and the circular from the Chief of Ordnance, embracing that programme was also read.

A resolution was adopted to the effect that it was the sense of the meeting that a Special Committee should be appointed by the Board of Directors, to prepare and promote such a programme of tests of structural materials, as to secure the best results possible from the Watertown Arsenal experiments.

ENGINEERING NOTES.

ALEXANDRA DOCK.—Among important engineering works in England is the new dock at Hull, which has been designated the Alexandra Dock. Before describing its construction, however, it may be as well to notice briefly the accommodation Hull at present possesses in this respect. There is, of course, the old harbor or river Hull, which runs inland from the Humber and, winding through the town, has a very large number of quays and landing places along its banks. These, however, are only available for vessels of comparatively small tonnage and the waters of the river, moreover, are shallow and tidal. Situated along the bank of the Humber there are altogether seven wet docks, one graving dock, and two timber ponds. The wet docks, with their respective approximate water areas, are as follows :—Commencing from the western end of the range there is first the graving dock, which is 450ft. long by 50ft. wide, next the William Wright Dock of 7½ acres, adjoining which is the Albert Dock of 24½ acres, which is the largest existing dock in Hull. Next comes the North-Eastern Railway Dock of 2¾ acres after which, running inland in a curved line, are the Humber Dock of 7 acres, the Princess Dock of 6 acres, and the Queen's Dock of nearly 9 acres. Crossing the mouth of the river Hull we come to the Victoria Dock of 20 acres, beyond which are the timber ponds. The new Alexandra Dock, which will have a water area of 46½ acres, forms the eastern end of the range. Hull, therefore, at present, possesses 76½ acres of wet dock accommodation, besides which there is at the western extremity of the range a dock 10½ acres in extent now in course of construction. When all is completed Hull will possess about 134 acres of dock area, but with the great accession of trade which the increased accommodation may reasonably be expected to attract, it may be questioned whether a further extension may not be required before long. In fact, the new dock company have anticipated this by obtaining Parliamentary powers last Session to extend their accommodation to the eastward of their present works. With regard to the Alexandra Dock it will have a water area nearly twice as large as that of the Albert Dock. This latter dock is at present the only one practically available for the large vessels engaged in the Californian and East Indian trades, there being no warehouses of any extent available for grain at the Victoria Dock, and the other docks being too small for such vessels.

In order to construct the Alexandra Dock about 150 acres of land have been reclaimed from the Humber by embanking, so that the docks are really situated upon the foreshore of the river. Of these 150 acres about 100 will be occupied by the wet dock and two graving docks, with their quays, warehouses, roads, railways, and other adjuncts. It has a river frontage of 6,000ft., or more than a mile, with a depth to the rear of about 3,500ft. The dock itself has a total length of 2,300ft., and a width of 1,000ft., the area being, as we have already stated, 46½ acres. It is entered by a lock 550ft. in length and 85ft. in width, and having three pairs of gates and a caisson at the entrance. The lock is approached from the river through a trumpet-shaped entrance 360ft. wide, and having a wharf 300ft. long and constructed of timber on either side. These wharves are built on piles 60ft. long of creosoted timber. The water is shut out from the dock by a sea-bank one and a quarter mile in length, composed of 200,000 tons of chalk, and faced with Bramley Fall stone, with a slope of two to one on the sea face. The entrance to the lock works is at present protected by a cofferdam 500ft. long, constructed on a curve having a radius of 256ft. The dam is constructed of two rows of piles driven 6ft. apart, the intermediate space being filled in with puddled clay. There are 1,000 piles in the cofferdam of lengths varying from 50ft. to 60ft. There will be a depth of 34ft. of water over the sill of the dock at high water of ordinary spring tides, so that the largest class of shipping can be accommodated. In the dock, situated to the east of the entrance from the lock, will be two jetties, each 400ft. long and 80ft. wide. There will be a third jetty running out from the western side of the dock 450ft. long and 100ft. wide. The walls of the dock are 40ft. 6in. high from ground level, their depth below that point varying from 10ft. to 15ft. They are 20ft. wide at the base and 6ft. 9in. at the top. They are constructed of chalk rubble masonry faced with ashlar and finished with a granite coping. The jetties are also of masonry and of similar construction to the dock walls Generally the dock will be furnished with the necessary cranes, capstans, and other appliances, which, with the bridges, hoists, sluices, gates, and valves, will be worked by hydraulic power. This power, in fact, is already supplied and in use for driving a large amount of the machinery and appliances at present employed in the construction of the docks. The engine-house is situate at the north-eastern corner of the dock, and contains two double-cylinder high-pressure condensing engines, each of 150-horse power, by Sir W. Armstrong and Co. They take steam from six horizontal multitubular boilers of locomotive type,

working up to 80lb. per square inch. This develops in the accumulator a pressure of 700lb. per square inch for hydraulic purposes. The accumulator is 18in. in diameter, with a 85ft. stroke, the total weight on the water being 100 tons.

At the north-eastern angle of the main dock are two graving docks, the smaller of which, or No. 1, is 500ft. long by 60ft. wide on the floor, and 512ft. long by 81ft. wide on the top, with 19ft. 6in. of water on the sill. The walls of this dock, which are constructed of rubble masonry faced with Bramley Fall stone, are built up to within about 8ft. of the finished level. No. 2 dock is 550ft. long by 65ft. wide on the floor, and 564ft. long by 89ft. wide on the top, with 21ft. 6in. of water on the sill. The walls are similarly constructed to those of No. 1, and have reached to within 7ft. of the finished level. The water will be pumped out of these docks by pumping engines, which will also be used for maintaining the water at the proper level in the main dock. The supply of water for this purpose will be drawn from an inland stream which drains the surrounding country and empties itself into the Humber The reason why this course is adopted in preference to replenishing the dock from the river is that the water from the latter source carries with it a large percentage of alluvial matter, which would deposit itself in the dock and necessitate frequent dredging. Thus two useful purposes are served, the drainage water is drawn off from the adjacent lands and a muddy deposit is prevented from accumulating in the dock.

The chief interest in the works at present centers upon the entrance lock, for constructing the walls and sluices of which two extensive trenches have been excavated. Each of these trenches is 680ft. long, 45ft. wide, and 45ft. deep, the sides being supported by an enormous mass of heavy timbering. The work is well forward in these trenches, and it is in them that the ceremony of setting the heel stones took place yesterday. Each of these stones consists of a block of Penrhyn granite from Cornwall, and each weighing eight tons. They will carry the gates which will close the entrance to the lock. The work of excavation in the dock is proceeding rapidly, a new feature being the introduction of hydraulic navvies, with which the contractors have superseded the steam navvy, which was extensively used by them in the construction of the Albert Dock at Millwall. It was found that if the steam navvy had more work to do than it fairly could it was strained, and soon got out of order. On the other hand, the hydraulic navvy cannot get strained, inasmuch as if set to do more work than it can accomplish, it simply stops dead short. The hydraulic navvy, moreover, is more economical to work, it requires two men only to control its movements, and performs an enormous amount of work. It is capable of removing from 600 to 700 cubic yards of soil per day, and is self-propelling, eating its way as it goes, and depositing the earth in tip wagons on either side as fast as they can be placed in position and removed. There is also another successful adaptation of hydraulic power for drawing the barrows of earth up an incline to bank from the bottom of the excavations, and lowering the empty ones. The chalk for the work is brought direct from some quarries, purchased by the contractors, the sand being taken from pits some 15 miles away.

Works of the extent of those under notice necessarily involve the services of several engineers. In the present instance, the engineer-in-chief for the dock is Mr. James Abernethy, Past President of the Institution of Civil Engineers, who is assisted by Messrs. Oldham and Bohn, of Hull, the acting engineers being Mr. A. Hurtzig and Mr. G. Abernethy.—*London Times.*

RAILWAY NOTES.

THE question of the necessity of introducing automatic couplings for railway purposes in England similar to those in use on some American, Colonial, and Continental railways, was brought before the members of the Manchester Association of Employers, Foremen and Draughtsmen, at their meeting in a paper read by Mr. J. Nasmith. Models of automatic combined buffers and couplings and of central automatic link couplings as used in the colonies were exhibited by Mr. Nasmith, who urged that an automatic coupling being proved to be mechanically practicable, the English companies ought to alter their present system without delay. The opinion of the meeting was, however, that an automatic coupling must be of such a character as to admit of gradual introduction to have any chance of adoption in that country. It must be designed so as to be applicable to existing stock without much alteration.

THE Channel Tunnel Railway Bill seeks to empower the Channel Tunnel Company, limited, to make the following railways in Kent in connection with the Channel tunnel, and for other purposes: Railway (1) 3 miles 1 furlong 2 chains, commencing in the parish of Temple Ewell by a junction with the London and Chatham Railway, and terminating in the parish of Guston, and at the Dover and Deal-road-railway (2) 2 furlongs 8 chains, in Temple Ewell, commencing at the junction with the London, Chatham, and Dover railway and terminating by a junction with with railway (1); railway (3) 1 mile six furlongs 2 chains, commencing in Guston by a junction with No. 1 and terminating at low-water mark in the parish of West Cliffe. The capital of the company for the purposes of this act to be £750,000 divided into 37,500 shares of £20 each. The bill gives borrowing powers to the extent of £250,000, and the works are to be completed within ten years from the passing of the act.

A TABLE, showing the result of the working of the St. Gothard Railway from June to December, has been published. Excepting June, which was a broken month, August yielded the best returns—998,000f.—and December the lowest—685,000. On the other hand, while the working expenses were the lowest in August and September, not exceeding 300,000f., they rose in December, owing to the necessity of keeping the line clear of snow, to 426,000f. The excess of receipts over expenses

during the period in question makes a total in round numbers of three and a-half million francs. The goods traffic shows a steady increase, the receipts from this source being 500,000f. in December, against 380,000f. in August. The falling off in the winter returns arises exclusively from the diminution in the number of passengers. In August 117,000 passengers travelled over the line; in December only 50,000. The number of passengers carried by the company since the opening of the line last June is 601,000.

ORDNANCE AND NAVAL.

THE ITALIAN NAVAL REVIEW OF 1882.—An official account of the naval review held by King Humbert at Spezzia on October 17, and of the conclusions drawn from it, has lately been published. His Majesty examined the 45-centim. breechloading cannon destined for the Italia, and the heavy artillery on the fortifications, reviewed the marines and soldiers of the garrisons, and witnessed several experiments with submarine mines and torpedoes, including a sham fight between the Duilio and Dandolo, and four first-class torpedo boats, which formed the escort of the royal launch. The points considered to be established by these experiments are the following: A stationary vessel would be certainly struck by a torpedo, discharged from a torpedo boat approaching her, within 220 yards distance. There is great probability that during an actual engagement torpedo boats would be able to come unperceived within this distance of an ironclad, in spite either of daylight or the electric light, under the cover of the smoke of her own guns. First-class torpedo boats will form, therefore, one of the principal elements in the coast defence of the future, and will render the operations of bombardment both dangerous and difficult. It is very probable that mistakes in the working of the torpedo-discharging apparatus may occur during the excitement of an engagement, and consequently it is highly desirable that the men should be well practiced in the duties. The first-class torpedo boats at present in use, although behaving well at sea, cannot develop their full speed if it is rough. Submarine torpedoes, laid to defend the anchorage of ships, entrance of harbors, &c., can be innocuously exploded by a system of counter-mines, dropped rapidly by swift steamers, and discharged without loss of time. The war material furnished for the defence of the Gulf of Spezzia is in excellent condition, and well adapted to its purpose. The artillery, electric lights, and torpedoes of the Duilio and Dandolo are entirely satisfactory, as is also the working of their engines.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

FROM MR. JAMES FORREST we have received the following papers of the Institution of Civil Engineers:

The Horizontal Thrust of a Mass of Sand. By George Howard Darwin, F. R. S.

The River Indus. By Charles Swaine Fahey, M. I. C. E.

Modern Corn-Milling. By Professor Cesare Saldini

Various Methods of Determining Dimensions. By Dr. James Weyrauch.

The Capacity of Storage Reservoirs. By W. Ripple.

REPORT OF THE NEW YORK STATE SURVEY FOR 1881. By James T. Gardiner.

MONTHLY WEATHER REVIEW FOR FEBRUARY. By Maj-Gen. W. B. Hazen.

TWENTY YEARS WITH THE INDICATOR. By Thomas Pray, Jr., C. E. Vol. I. Boston: Journal of Commerce Publishing Co.

This is a treatise on the construction and use of the Indicator, with examples from actual use exhibited by diagram.

As an instruction book for the learner, this seems by reason of the large number of examples to be well adapted.

HYDRAULIC MANUAL. By Louis D'A. Jackson. London: Crosby, Lockwood & Co.

A summary of the text of this book is briefly given as follows:

I. Explanation of Principles and Formulas.

II. Field Operations and Gauging, with brief accounts of modes adopted.

III. Miscellaneous Paragraphs on Hydraulics.

About one-third of the book is filled with the tables commonly used for hydraulic calculations.

The treatise is designed as a guide to the survey of rivers and canals rather than to the minutiæ of supply and drainage.

THE FORESTS OF ENGLAND. By J. C. Brown, LL.D.

This is an attractive historical sketch of the English forests, going back to the time of the Roman invasion.

Forests that have been submerged since the beginning of historic times come in for their share of attention.

"The Devastation of Forests," and "Forest Legislation" are topics upon which the writer is a leading authority, and upon which he discourses at some length in this volume.

DETAILS OF MACHINERY. By Francis Campin. London: Crosby, Lockwood & Co.

This is the latest addition to the Weale's Rudimentary Series. The plan of the book is somewhat novel, as the elementary parts of machinery are separately considered.

Arithmetical computations of strains and dimensions abound throughout the book, and everywhere elaborate rules are made to take the place of formulas. No algebraic expressions are employed, not even a plus sign.

The work will prove serviceable for the class of artisans for whom it is specially designed, as it furnishes them with a collection of useful rules and illustrative examples, using only arithmetical computations.

THE GREAT PYRAMID. By Richard A. Proctor. New York: R. Worthington.

To those who have read the previous essays of Mr. Proctor relating to the Pyramid, this

will prove an interesting supplement. All who have regarded the theory of Prof. Smyth or of Abbé Moigns at all favorably, should read this new essay carefully. It does not need, however, an acquaintance with previous writings to read this book with pleasure and profit. It is written in the author's best vein.

The book also contains the following essays: The Origin of the Week, The Sabbath of the Jews, Astronomy, and the Jewish Festivals, and The History of Sunday.

MISCELLANEOUS.

BEFORE the Paris Academy of Sciences a paper was recently read on "Hydraulic Silica, and on the part it plays in the Hardening of Hydraulic Compound," by M. Landrin. The pure silica obtained by decomposing a solution of silicate of potash with an acid, and repeatedly washing and drying at a dark red heat, he names hydraulic silica, and he considers it the cause of the final hardening of hydraulic mortars. The aluminate of lime cannot concur in this effect, because of solubility, but at the moment of immersion it facilitates the intimate union of the hydraulic elements, hinders water from penetrating the mass of mortar, and so aids the slow reciprocal action of the lime and hydraulic silica.

OWING to its greater strength, phosphor-bronze is used sometimes instead of copper for conducting electricity, since much smaller wires possesses the necessary strength. The resistance offered by phosphor-bronze is considerably greater than that of copper, so that while it answers well for telephone wire, it is not adapted to long telegraph lines. Herr L. Weiller, of Angouleme, has recently alloyed copper with silicon instead of phosphorus, and made a silico-bronze, the conductivity of which, the *Scientific American* says, is twice that of phosphor-bronze, while its strength is not less, and hence seems well adapted to electric conductors. The relative strengths of copper, silico-bronze, and phosphor-bronze, are said to be as 28, 70, and 90; conductivity as 100, 61, and 80.

AT a recent meeting of the Paris Academy of Sciences a paper was read describing some researches on the relative corrosion of cast iron, steel, and soft iron, by M. Gruner Various plates, suspended in a frame by their four corners, were immersed simultaneously in water acidulated with 0.5 per cent. of sulphuric acid, or sea water, or were simply exposed in moist air of a terrace. In moist air, chromate steels were corroded most rapidly, and tungsten steels less than carbon steel. Cast iron, even, with manganese, is oxidized less than steel and soft iron, and white specular iron less than grey cast iron. Sea water, on the other hand, attacks cast iron more than steel, and with special energy white specular iron. Tempered steel is less attacked than the same steel annealed, soft steel less than manganese steel or chromate steel, &c Acidulated water, like sea water, dissolves grey cast iron more rapidly than steel, but not white specular iron; the grey impure cast iron is most strongly attacked. These results agree with the complete experiments on the subject by Mallet in 1843.

MANGANESE BRONZE.—The use of this alloy as a material for screw propeller blades is rapidly extending. The first run made by the Alaska between Queenstown and New York in less than seven days was immediately after her steel blades had been replaced by others of manganese bronze. Nearly every clipper ship afloat is fitted, or is being fitted, with them; the Galia, the Orient, the Austral, the Normandie, the Stirling Castle, and many other less known names, figure in the list issued by the Manganese Bronze and Brass Company, Limited, together with one or two war vessels belonging to our own and other governments. The qualities that render the metal peculiarly suitable for propellers, are its great strength and its non-liability to corrosion. Experiments carried out at the works of Messrs. Maudslay, Sons, and Field, show that the bronze has a transverse strength about double that of gun-metal, and up to the elastic limit, double that of steel. In the tests that were made, the steel took a permanent set of .01 in. with a strain of 10 cwt., as against 20 cwt. required to give the manganese bronze a similar set. In manufacturing steel blades an extra thickness has to be provided, to allow of the rapid pitting that takes place on the back, and even with this provision the life of the blade rarely exceeds three years. But with bronze blades no such allowance need be made; they can be cast of the thickness to give them the requisite strength, and put in their places without undergoing the annealing which often results in the distortion of steel blades. As regards cost, steel blades can be bought for 50*l.* to 60*l.* per ton, while if constructed of bronze their price is 125*l.* to 135*l*, or if allowance be made for the difference of weight, about double that of steel. Assuming the steel blades to require renewal every three years, it is calculated that by the time the second set are fitted the expense has run up to the price of bronze blades, which are stated to be good for the life of the vessel, and then to have considerable value as old metal.

THE steamship "Tartar," lately built for the Union Steamship Company, has the following dimensions. Length, 392ft.; breadth, 47ft.; depth, 33ft 6in. Estimated gross tonnage, 4,359, with a displacement of 8,000 tons. The "Tartar" is divided into thirty water-tight compartments, and has a double bottom constructed on the cellular system, extending the whole length of the ship, and capable of containing 500 tons of water as ballast. The ship is built to meet all the requirements of the Admiralty, and will be placed on their list of vessels for use in time of war. Her outside plating is doubled, to ensure greater strength amidships. The dining saloon, on the upper deck forward, is 62ft. long, and of great height. Above the saloon is a large music room and ladies' boudoir, opening on the promenade deck, which is nearly 180ft long. The engines have cylinders 50in. and 94in. diameter, with a stroke of 60in., and are 650-horse power nominal.

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VAN NOSTRAND'S

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NO. CLXXIV.—JUNE, 1883.—VOL. XXVIII.

A STANDARD GAUGE SYSTEM.

By GEORGE M. BOND, Hartford, Conn.

Transactions of the American Society of Mechanical Engineers.

In a paper presented at the May meeting of the Society last year, a statement, or "report of progress" was submitted, showing the method adopted by the Pratt & Whitney Company, of Hartford, Conn., by which the question of practically establishing a standard for size gauges was to be scientifically determined, accurately subdividing the Imperial yard into feet and inches and fractional parts of an inch, and describing briefly the extent to which was carried the scientific research found absolutely necessary for such an undertaking; it remains now to present to the consideration of those who may be interested, a statement of the results proceeding from the practical application of all the thorough, conscientious investigation of Professor Rogers, of Harvard College Observatory, whose invaluable experience and professional services, in obtaining for the company the transfer and subsequent subdivision of the British yard, gave the foundation for what has been accomplished, enabling the company to feel warranted in earnestly inviting an inspection of the means now available for the production of standard sizes, and asking for it the indorsement of the engineering profession, should it be found worthy of such necessary support.

The comparator referred to in the previous paper, has been placed in position upon brick piers in a room outside the main building, erected especially for it, and is comparatively free from the jar and tremor of the machinery, even unaffected by the jar of passing trains, the tracks of the New York, New Haven and Hartford, and of the New York and New England Railroads being quite near, the rigidity of the instruments and the excellent workmanship in its construction preventing any perceptible vibration during an observation, even when the high power microscopes, magnifying 150 diameters, are used.

The illumination required in using these microscopes is perfectly attained by reflection, using a plate glass mirror placed outside of the window of the comparing room, at an inclination of 45 degrees, giving clear diffused light, cloudy weather even improving the general effect, as the light is then whiter than that reflected from a clear blue sky, and the lines on the standard bar, as seen through the medium of the Tolles' illuminating prisms with which the objectives

are fitted, are clearly and sharply defined at any time during daylight, and in any position of the microscope plate; by thus avoiding the use of artificial light and consequent effect of a variable temperature, far more satisfactory results are obtained.

The investigation for the determination of the necessary corrections for errors due to horizontal or vertical curvature of the path of the microscope plate, has shown conclusively the unexcelled workmanship in the construction of the comparator; as an instance of how slight these errors really are, it was found after repeated observations the means being carefully collected, that the horizontal curvature, *i. e.*, the bending sidewise of the cylindrical guides or ways upon which the microscope plate slides, at the part investigated, was that having a radius of *eleven miles*, and a consequent correction to be applied of about one ten-thousandth of an inch in eighteen inches, the latter distance referring to the position of the measured standard when placed either side of the line of motion of the center of the microscope plate, moving between fixed stops, and which is the constant quantity to which is referred the subdivisions of the standard bar; as the microscope is usually within one inch, rarely over two and a half, from the center line of the stops or caliper jaws between which the end measure pieces or the cylindrical gauges are placed, this correction evidently becomes too small to be applied practically, within the limit of a six-inch gauge; for a foot or a yard it would become necessary, as the variation of the chords of the subtended arcs then becomes quite perceptible. The errors due to inequality of temperature in the standard steel bar and the hardened steel end measure gauges must be carefully guarded against, the latter effect being far more important, practically, and often very misleading. In the case of a four-inch hardened steel end measure gauge experimented upon, the coefficient of expansion being nearly one one-hundred thousandth of its length,* one degree of change of temperature from that maintained in the reference bar, introduces an error of nearly one twenty-five thousandth of an inch in the total length, and hence, as a change or inequality of five or even ten degrees might be easily overlooked, the four-inch gauge would be found to be from one five thousandth ($\frac{1}{5000}$) to one twenty-five hundredth ($\frac{1}{2500}$) of an inch too short, when equality of temperature is restored; when it is asserted that an actual variation of so minute a quantity as *one thirty-thousandth* of an inch, and even less, can be readily detected by any tool-maker familiar with the use of an ordinary micrometer or a close gauge, the importance of keeping *within* this limit is apparent,—of course, this shortening effect is not so marked in smaller sizes, still the ratio is the same, and this error must be carefully avoided.

The subdivisions upon the six-inch hardened steel standard bar have been carefully investigated upon the new comparator, to determine how nearly these inch spaces equal each other, the total length of the four inches which are ruled upon this six-inch bar being *exactly standard* at 62 degrees Fahrenheit, according to the official report of Professor Rogers, received December 1st, 1881, and in this report, the results obtained by him, determining this relation of the inch spaces to each other, were found to agree closely with the results obtained by me, as the following comparison will show, these minute corrections being necessary in accurately determining the subdivision of the Imperial yard, which they represent:

Corrections.	Prof. Roger's Report.	Results obtained.	
Total 1 inch, add....	0.000008	0.000008	($\frac{1}{36}$ yd.)
Total 2 ins., *subtract.*	0.000026	0.000027	($\frac{1}{18}$ ")
Total 3 ins., *subtract.*	0.000005	0.000006	($\frac{1}{12}$ ")
Total 4 inches......	correct.	correct.	($\frac{1}{9}$ ")

(The errors being counted from the first line.)

The results given in the last column are the *means* of a number of observations, taken at different times and under various conditions of temperature, &c., and cover a period of about four weeks, the final results having been obtained December 31st.

The value of the divisions of the micrometer employed was carefully determined in order to reduce them to the same unit used by Professor Rogers, and was found, using the microscope marked

* 0.0000066+

" B," to equal $\frac{1}{58500}$ of an inch (0.000016 nearly).

When it is considered that the two results were obtained under different conditions, using different microscopes, and with comparators differing in construction, the correctness of the *principle* upon which the comparison is founded, certainly needs no other proof.

The method of obtaining this relation of the separate inches upon the six-inch standard, was referred to in the former paper, and is that of comparing each inch with a constant distance moved over by the microscope plate between fixed stops, a constant pressure of contact being obtained by the use of electro-magnets, the separate inch spaces being thus referred to an *invariable* quantity or distance, and their relation to each other consequently determined.

To explain more fully this operation, the method adopted is as follows: A series of readings are taken at the zero or initial line of the first inch space, using the micrometer referred to, the microscope plate being held firmly against the fixed stop by the electro-magnet; the microscope then moves with the sliding plate until the latter is in contact with the other fixed stop, and held by the electro-magnet, the plate having moved as nearly an inch as it may conveniently be done,—generally a little more than an inch, in order to have the *sign* of the reading always the same,—from three to five readings of the micrometer are then taken at each position of the microscope, and the order reversed, to eliminate possible error; the first inch is thus compared with a fixed quantity, and the same operation repeated for the remaining inch spaces.

The difference between the distance moved by the microscope plate and the distance between the defining lines representing inches, is found by subtracting the *means* of the readings obtained, and thus eliminating the possible error of any single observation.

The following is a series of micrometer readings, and comprises the means of all the observations by which the corrections of the separate inches were obtained, illustrating the system adopted, and which has been used invariably by Professor Rogers in his investigations of the subdivisions of the yard and meter bars, now in the possession of the Pratt & Whitney Company.

Comparison of Inches.

	L.	R.	L.	R.	
	58.1	65.8	68.0	55.5	
1st inch.....	58.6	66.0	58.1	65.8	Reverse
	58.5	65.4	58.0	65.8	order.
Mean........	58.4	65.7	58.0	65.7	
	58.0	65.7			
Mean........	58.2	65.7			

R−L=+7.5 divisions of micrometer.

	L.	R.	L.	R.	
	56.2	65.8	55.4	65.4	
2d inch......	55.6	65.8	56.0	65.4	Reverse
	56.2	66.0	55.4	66.0	order.
Mean........	56.0	65.8	55.6	65.6	
	55.6	65.6			
Mean........	55.8	65.7			

R−L=+9.9.

	L.	R.	L.	R.
	55.0	63.0	57.0	63.0
3d inch......	55.8	63.0	57.0	63.5
	55.7	63.6	57.0	62.8
Mean........	55.5	63.2	57.0	63.1
	57.0	63.1		
Mean........	56.3	63.1		

R−L=+6.8.

	L.	R.	L.	R.
	57.4	64.3	56.0	64.2
4th inch.....	57.8	64.8	56.1	64.5
	57.8	64.8	56.8	64.5
Mean........	57.6	64.6	56.3	64.4
	56.3	64.4		
Mean........	56.9	64.5		

R−L=+7.6

Correction Σ

$+7.5+0.45+0.45$=correction for 1st in.
$+9.9-1.95-1.50$=correction for 1st 2 ins.
$+6.8+1.15-0.35$=correction for 1st 3 "
$+7.6+0.35\pm0.00$=correction for 4 total "

Mean $+7.95$

The differences of the observed inch spaces with respect to the constant quantity obtained by the motion of the microscope plates are added and their means taken, from which the corrections are determined with the proper signs; these corrections for the separate inches are added, the final result being evidently zero, the column under " Σ " showing this algebraic sum and the total error reckoned from the first line.

It may be appropriate here to state that the six-inch bar upon which these four-inch spaces are traced, is a standard which is, without doubt, the only hardened steel *line-measure* bar in existence, which is exactly one-ninth part of the Imperial yard at 62 degrees Fahrenheit, and Professor Rogers guarantees it as such in his report, before referred to.

In order to apply these subdivisions, which include all sizes, from one-sixteenth of an inch to four inches, varying by sixteenths, to a practical form, fixtures have been provided and are in constant use, for reducing to *end* measure the distances thus accurately spaced, a caliper attachment has also lately been added, from plans proposed by Professor Rogers, by which the diameter of existing cylindrical gauges, as well as the length of end-measure pieces from one-sixteenth of an inch to six inches may be tested with the same precision that characterizes the investigation of the linear spacing of the standard bar, also providing means for a rigid inspection of finished gauges before they leave the works, thereby insuring uniformity.

To illustrate how nearly alike two pieces may be made, two standard inch end-measure gauges were worked down under the microscope, independently of each other, using the lines upon the ruled standard reference bar and the fixture referred to, which, when compared with each other by the most careful tests, using close or "snap" gauges, and tested thus by tool-makers experienced in work requiring the utmost practical precision, neither piece could be singled out as the larger, the effect of unequal expansion caused by temperature being avoided during the test. Under the microscope, both pieces were found to be exactly alike by a single observation, while the comparison by a series of readings of the two pieces showed a mean difference of one-tenth of a division of the micrometer, and when it is remembered that one division has a value of only $\frac{1}{58500}$th of an inch, the duplication, it may be assured, is certainly satisfactory, and is clearly within a practical, if not a theoretical limit of accuracy.

Having thus the means for closely reproducing established sizes, and no possible wear occurring to the bar from which these sizes are taken, the accurate duplication becomes a comparatively simple operation.

In order to produce standard work within the limit of a yard or a meter, there has been furnished by Professor Rogers, two steel yard and meter bars, referred to in the previous paper, one of these bars tempered, the other being left soft, but having hardened steel plugs, which are adjustable, for the purpose of bringing the surfaces into focus under the microscope; upon the hardened plugs the lines are ruled, both bars having line and end measure, thus providing means for testing the accuracy of the pitch of screw threads of any desired length, and for standard length gauges up to thirty six inches, or to a meter.

The coefficient of expansion has been determined for each by Professor Rogers with great care, and also the relation, at 62° (F.), between these steel bars and the two bronze standards, both of which are line measure, the latter are described in the previous paper referred to, so that gauges of any size may be made almost independently of temperature, other than the care required in keeping this condition as nearly uniform as possible for both the reference and the measured bar during the time of the transfer, or the determination after having been transferred.

The subject of accurately producing standard leading screws is receiving its share of attention, and those who use micrometers will readily understand its bearing upon the precision with which they may be made, and how unsatisfactory because of the necessary corrections to be applied in many instances, even when standard at some one part of the divided head—a uniform lead or pitch of the screw, however much may be the total error (within a reasonable limit), making a great improvement in their construction.

Besides a complete set of end-measure gauges varying by sixteenths from one-quarter to four inches, there is now ready for inspection a complete plant, consisting of tools and fixtures for producing the standard United States or Franklin Institute thread gauges, every detail having been carefully considered, and every difficulty overcome in the operation for perfecting, not only these standard gauges as to size, but the pitch of the thread, the correct angle, and the width of flat at the

top and bottom of the thread, the accuracy with which these details have been carried out, is now open to the inspection of all who may be interested, and rapid duplication by *machined* work is now an assured success.

It is with the confidence that the "bottom" has finally been reached, that warrants the Pratt and Whitney Company in thus inviting a thorough inspection of the means available, and the methods employed, in producing standard gauges, and earnestly desiring an impartial verdict as to its accuracy and practicability, whether or not, the system as adopted and carried out, has any real merit upon which the confidence of those using gauges for interchangeable work may be safely based.

Discussion.

Professor Robinson: This paper is of the highest merit and of the greatest possible interest to members of the Society, and it is a matter of great satisfaction to the mechanical engineers of this country that there is some one who is able to take hold of this question and treat it so ably as it has been treated; and inasmuch as the methods and results, as stated by the reader, are laid open for inspection by others, I think it is due, in consideration of the amount of effort and expense which has been given to this matter, that the Society, as a matter of duty, should appoint a committee to avail itself of the privileges offered of investigating this, and of making such a statement regarding it as will be found advisable; and undoubtedly this is a thing which the Society will be willing and anxious to indorse in every particular as a standard of the country. For one, I should be glad to see a committee appointed to give this method the attention it deserves.

The President: I suppose the members would be interested if Mr. Pratt would tell us how they went to work in this undertaking. Mr. Pratt told us the story at Hartford, but many gentlemen are here to-day who were not with us then, and I presume it would be very interesting to all to know how Messrs. Pratt and Whitney were led into this prolonged, expensive, and nice investigation.

Mr. Pratt: I will relate the story in the briefest possible way. We were called upon to furnish a set of standard thread gauges, and of course the first thing to do was to get the sizes, and upon examining the different makes of gauges we found no two sets alike, and we were forced to commence, as we thought, at the bottom, and at that time, in sending about a foot-piece that we had obtained, we found that those investigating it did not agree upon its value. Among others Professor Rogers was applied to, to investigate the foot-piece, and he had quite a struggle over it with some of our prominent manufacturers of gauges. They could not agree, and Professor Rogers took it upon himself, at his own expense, to go to Europe, and go to the bottom of the thing. He visited the best authorities in Europe, and spent four months there in investigation. After we had obtained the services of one of the graduates of the Stevens Institute, and in connection with Professor Rogers, we constructed two comparators, one of which Professor Rogers has himself; and one of which we have; they being exactly alike. After Professor Rogers returned he investigated the foot-piece, and found it to be about what he had found it to be before, and then, after the new comparator was finished, he found his statement verified. Previous to this time, fortunately, Professor Rogers had been for several years constructing a ruling machine, and he had it completed about this time. We have gone very carefully into this thing. I do not feel egotistic about it at all. What we want is that every one who is interested in the matter, every society that takes any interest in it, should come and examine our methods and our measurements. If they are good, let us have a standard. If they are not, let us throw them away. It has cost us probably twenty thousand dollars to-day, and I am willing to throw it away if anybody can show us better. We want a standard, and I will not stand in the way of any one else who has a better machine. I feel very much interested in the subject myself; and I think we shall succeed in what we have undertaken.

Professor Egleston: I do not know whether the members of this Society are familiar with the fact that in 1875 the Institute of Mining Engineers appointed a committee on standard gauges, and that committee came to just exactly the

conclusion which is expressed here, that diameters or linear measures ranging in fractions of a millimeter or an inch were the only gauges which were standard or could be standard. It is a peculiar gratification, since a large part of the work fell to my hands, to see that idea so fully sustained. It is a matter of a great deal more importance than perhaps would appear just now. I have heard recently that the English engineers are again agitating the question of standards, and are going back to the old caliper idea. I think this is really a retrograde movement. Most of you may know the fact, but we discovered in the course of our investigations, that in a dozen standard gauges, so-called, of the fixed patterns out of any package no two of them will be alike.

THE CONDITION IN WHICH CARBON EXISTS IN STEEL.

By Prof. F. A. ABEL, C. B., F. R. S., Hon. M. Inst. C. E.

From "Iron."

In a report presented to the Committee on Steel in October, 1881, an account was given of the results of some preliminary experiments, which were carried out with the object of ascertaining, in the first instance, whether any characteristic differences could be established in structure or chemical condition between thin discs of steel cut from one and the same piece of that metal, but differing from each other in regard to the treatment to which they had afterwards been subjected. It will be remembered that it was not possible to throw any light upon the mechanical condition, or structure, of the different specimens, by submitting them to the operation of the solvent (a chromic acid solution) specially selected on account of its gradual action; it being impracticable to check the action at any period when the portions of the discs least acted upon, or not at all attacked, could be retained upon the support on which they were placed, in the positions which they originally occupied in the very thin sheet metal. Considerable differences were found to exist between the total amounts of carbon contained in different discs, from one and the same piece of steel, but in the hardened, tempered, and annealed states respectively. The proportion in the specimens of annealed steel was comparatively very low; and this difference being confirmed by the examination of another series of dics, an inquiry into the course pursued in annealing the steel discs led to further experiments, which appeared clearly to establish the fact that the reduction in the proportion of carbon in the steel during annealing was due to the prolonged exposure of the discs to heat in contact with, or close proximity to, the wrought-iron plates between which they were confined. A thorough confirmation of the correctness of this conclusion being considered desirable by the committee. Mr. Paget was so good as to include, when preparing another series of steel discs from one and the same lot of steel, a number of specimens which were submitted to the annealing process in various ways. In one series, the discs were inclosed in sets of seven, one set between wrought-iron plates, planed and cleaned, and the other between cast-iron plates, planed and cleaned; this combination being again inclosed in wrought-iron and in cast-iron boxes respectively, and packed round with burnt soot. A set of three discs was similarly annealed between black wrought-iron plates, and another set of three between two blocks of fire-clay, inclosed in a cast-iron box and packed round with calcined magnesia. The examination of these sets of discs thus annealed was expected to demonstrate the nature and the extent of the effects of prolonged heating between wrought-iron and cast-iron plates, as to the abstraction of carbon from, or addition of carbon to, thin steel discs in contact with the plates, or separated from them by intervening discs; and also to show what effects, in regard to the condition of carbon in the steel, may be ascribable purely to the process of annealing.

As yet it has been impossible to pro-

ceed far with the examination of these plates, but the extreme decarbonizing effect of prolonged heating of steel in contact with wrought-iron was demonstrated by the following experiment. Some steel discs of the usual dimensions (2.5 inches in diameter and 0.01 inch thick), and containing about 1 per cent. of carbon, were annealed singly between two wrought-iron plates in the manner already described. That is, the arrangement containing the packed plates was raised in an annealing furnace to a bright red heat, sufficient to scale the cast-iron box, but not sufficient to fuse it; the fire was then slackened off, banked up, and the box left in the furnace undisturbed for twenty-four hours. Upon afterwards heating the plates thus treated to redness, and plunging them into cold water, they remained as soft as malleable iron; and the examination of one of the discs by Mr. W. H. Deering (who has carried out the whole of the experimental work connected with this report) showed that the carbon had been reduced to 0.1 per cent. Before proceeding with the comparative examination of the various series of discs, annealed, hardened, and tempered blue and straw, with which the author has been furnished by Mr. Paget, it was considered important to acquire further information regarding the composition and character of the carbon-iron compound which had been obtained, in the experiments described in the last report, by treatment of the thin sheet steel with chromic acid solution (produced by mixing a solution of potassium bichromate, saturated in the cold, with one-twentieth of its volume of pure concentrated sulphuric acid). It was stated in the former report that the cold-rolled and annealed discs thus treated had yielded in different proportions a black scaly or spangly substance, which was attracted by the magnet, and which was found to contain, in combination with iron, an amount of carbon equally practically to the whole amount which had been found to exist in corresponding discs, in the same cold-rolled and annealed condition, taken from the same piece of metal. On the other hand, a disc of hardened steel, which was submitted to the same treatment, yielded only a small quantity of dark particles of similar appearance, in admixture with some lighter-colored sediment, and the carbon in the residue, obtained in this instance, amounted only to about one-sixth of the total carbon in this steel. An examination of the proportion which the carbon bore to the iron in this particular residue showed it to be decidedly higher than in the spangly residues furnished by the cold-rolled and annealed samples. The several residues of the latter class resembled each other very closely in composition, and the ratios of the carbon to the iron in each case corresponded closely to that of the carbon in an iron carbide having the formula $F_3 C_1$. In a second experiment made with one of the cold-rolled discs, the metal was exposed to the prolonged action of a solution consisting of the same kind of chromic acid liquor as used in the previous experiments, but mixed with an additional quantity of concentrated sulphuric acid (40 grammes to 500 cubic centimeters of the solution). The heavy grey-black powder which had been separated from this disc (the solution being completed in twenty-four hours) was allowed to remain in the solvent for nine days. Its analysis showed it to contain a comparatively small proportion of iron, which appeared to indicate that the carbon-iron compound, which is at first separated, does not resist the further action of the chromic liquor, in the presence of a considerable excess of sulphuric acid.

The quantities of steel operated upon in these experiments were unavoidably small; and it appeared interesting, and possibly important, to ascertain whether the indications furnished by the results referred to, that the condition of combination of carbon with iron, in steel, differs in samples of one and the same metal if they have been submitted to decidedly different treatment, were confirmed by more extended experiments; also to learn more regarding the nature of the magnetic carbon-iron product eliminated by the action of a slowly oxidizing solvent upon annealed (or cold-rolled) steel; *e. g.* (1) whether its composition is independent of the strength within particular limits, of the chromic solution employed for its elimination; (2) whether, within those limits, a constant quantity of the carbide is obtained from 100 parts of one and the same de-

scription of steel; and (3) what proportions of the carbon in this carbide would remain unconverted upon treatment of the latter with hot chlorhydric acid. With these objects in view the author obtained from Mr. Paget a thin sheet of steel in the same condition as delivered to him from the Birmingham makers, having been cold-rolled and cross cold-rolled, and annealed several times between the various rollings. The weight of this plate was 175 grammes (2,700.6 grains) and its thickness about 0.008 inch (0.2 mm.) Mr. Deering's analysis of a sample of this steel plate, taken from the same part whence specimens were afterwards cut, showed it to contain—carbon, 1.144 per cent.; silicon, 0.166 per cent.; manganese, 0.104 per cent. The individual pieces of the steel plate submitted to the action of the solvent weighed from 7 to about 7.5 grammes (108 to 115.7 grains). Before treatment, the pieces were rubbed bright with emery-flour, then washed with ether, and dried in a clean cloth. The preparation of the chromic acid solutions employed in these experiments is best illustrated by a description of the production of the solvent used in obtaining what will presently be designated preparation 2. This liquor was prepared by adding to a filtered solution of potassium bichromate, saturated in the cold (67°–68° F.) and containing 99 grammes of the salt per 1000 cubic centimeters of solution, a proportion of concentrated sulphuric acid (having the full specific gravity) corresponding to 0.9 gramme of acid per 1 gramme of the potassium salt. The following chemical equation represents the action of a solution of this description upon iron:

$$Fe_2 + K_2Cr_2O_7 + 8H_2SO_4 = Fe_2(SO_4)_3 + Cr_2(SO_4)_3 + 2KHSO_4 + 7H_2O.$$

According to this equation, the theoretical requirement of acid is 0.84867 gramme to 1 gramme of the bichromate, and 1,000 c. c. of the solution thus prepared would suffice to dissolve 9.226 grammes of iron. Each piece of steel immersed in that quantity of liquid weighed, as stated, about 7 grammes (except in the case of preparation 4, when the proportions were about doubled). Therefore the solution employed was always considerably in excess of the amount required to dissolve the metal. (The strength of the solutions was checked by an estimation of the available oxygen contained in them.) The solution used in obtaining preparation 1 was intended to have been prepared from a bichromate solution saturated in the cold, of the precise nature of that just described; its examination showed, however, that it was somewhat weaker, being 0.8 the strength of the solution for preparation 2. Preparation 3 was produced with a much weaker chromic solution; the strength aimed at was 0.5 that of preparation 2, and its actual strength was 0.44. The chromic liquor used for obtaining preparation 4 was prepared by mixing a hot solution of bichromate with the requisite proportion of sulphuric acid (1 of the former to 0.9 of the latter); and the strength aimed at was double that of preparation 2. Two different quantities of the liquid were prepared, but in both cases the strength exceeded that of preparation 2 only by about one-half (being 1.44 of its strength in one case and 1.65 in the other); a little chromic acid having in each case crystallized out, together with the potassium bisulphate, on the cooling of the liquids. The mode of treatment of the steel by the chromic solutions was in all instances alike. The solvent (1000 c. c. in this particular case) was contained in a capacious, somewhat tall glass vessel; and the weighed piece of sheet steel was supported, at about the center of the liquid, upon a diaphragm, or sieve, of platinum wire gauze. Though the surfaces of the steel were perfectly cleaned, as described, it would remain quite unattacked in the liquid, even for days, if simply immersed and left at rest; but the action was started at once by moistening the steel with the chromic liquor and exposing it to the air in that state for a minute or two before immersion. By supporting the small platinum sieves upon funnels immersed in the liquids, the heavy solution of ferric sulphate passed down through the funnel as produced, and thus a continuous circulation of the solvent was promoted.

Preparation 1.—Four pieces of the sheet steel (from 7 to 7.5 grammes each) were exposed to the action of the solvent described in separate vessels. After the lapse of two days, there only remained

small quantities of a grey-black powder upon the sieves; this was washed off into the chromic liquor and, together with the powder which had collected at the bottom of the vessel, was allowed to remain from eight to fourteen days in the liquor, the time varying with the date at which the several experiments had been commenced. In every case there was found to be a considerable excess of chromic acid in the solution. The four deposits were afterwards transferred to one vessel; 500 c. c. of fresh chromic liquor were placed upon the combined product, which was allowed to remain in the solvent for four days at the ordinary temperature. During this time no reduction of chromic acid took place. The heavy grey-black powder, which was strongly attracted by the magnet, was then washed, first with water several times, then with alcohol, finally with ether, and was afterwards dried over oil of vitriol in a rarefied atmosphere until it ceased to lose weight. The amount of dry residue, or carbide, obtained in these operations per 100 parts of steel was 13.25. To ascertain the proportion of carbon in this carbon-iron product, the method of treatment by solution of copper chloride was first resorted to; but the substance was attacked with difficulty by the solvent, there being no action at the ordinary temperature even after the lapse of twenty-four hours.* It was therefore necessary to keep the solvent heated, to promote its action, and this may have given rise to some slight formation of carbo-hydrogen, tending to reduce the proportion of carbon found. Moreover, this carbon, when the action was completed, was obtained in so very finely divided a condition, that its collection without mechanical loss was a matter of great difficulty. For these reasons this method of analysis was abandoned, and the comparatively simple process adopted of placing and weighing the dry material to be analyzed in a small platinum boat; inclosing this in a Bohemian glass tube; burning in a slow current of dry oxygen; allowing the products to pass over heated cupric oxide; and finally absorbing and weighing, in the usual manner, the carbon dioxide and water obtained. At the close of the operation the residual iron oxide in the boat was dissolved in chlorhydric acid, and the iron estimated. By the copper-chloride process, the percentages of carbon found in preparation 1, in two experiments, were 6.83 and 6.69. The iron, estimated in the liquids (after precipitation of the copper by electricity) amounted in these two experiments to 91.29 and 92.16 per cent. By the combustion process, the following percentage results were obtained:

Carbon	7.31
Iron	90.42
Water	2.37

In order to ascertain what proportion of carbon this product would leave unconverted into carbo-hydrogen, by treatment with chlorhydric acid, from 0.5 to 1 gramme of the carbide was heated upon a water-bath with excess of the acid (sp. g. of the acid 1.10); the portion remaining undissolved was collected upon asbestos (previously ignited), washed successively with cold water, cold alcohol, and warm ether, then dried in a current of hydrogen, while gently warmed and afterwards burned in a current of oxygen. In two experiments the carbon unconverted into hydrocarbon amounted to—

1.410 per 100 of the carbide, or 20.87 per 100 of carbon in the carbide,

And

1.238 per 100 of the carbide, or 16.93 per 100 of carbon in the carbide.

Preparation 2.—Two pieces of steel, weighing about 7.5 grammes each, were treated with the particular chomic solution above described, 1250 c. c. of liquid being used in each case, and the treatment being carried on for four days. The two products, of the same nature as those constituting preparation 1, were then transferred to one vessel, and left for two more days in contact with 250 c. c. of fresh chromic liquor, which appeared unaltered at the end of that time. The amount of carbide which this treatment furnished per 100 of steel was 14.16. The analysis of the dried product by the combustion process furnished the following percentage results:

Carbon	7.21
Iron	90.64
Water	2.27

* This circumstance afforded additional proof that metallic iron had been completely removed by the chromic treatment.

The carbon remaining unconverted into hydrocarbon, by treatment of this product with chlorhydric acid, amounted to—

1.269 per 100 of the carbide, or 17.60 per 100 of carbon in the carbide.

Preparation 3.—Two pieces of steel, weighing about 7.5 grammes each, were submitted in separate vessels to the action of 2000 c. c. of the solution already described (the comparatively weak chromic liquor) for five days. The united products were afterwards left for five days in 500 c. c. of fresh chromic solution, which did not appear at all affected. The amount of carbide obtained from 100 parts of steel was 15.34. The percentage results obtained in three examinations of the product were as follows:

Carbon.......	6.84	6.84	
Iron..........	91.50	91.50	91.50
Water.......	1.63		

The carbon unconverted into hydrocarbon by treatment of the carbide with the chlorhydric acid amounted to—

0.836 per 100 of the carbide, or 12.22 per 100 of carbon in the carbide.

Preparation 4.—Here 14.7 grammes of the steel were exposed for three days to the action of 1900 c. c. of the chromic solution described (the strongest solution), and the product obtained was afterwards left in contact with 350 c. c. of fresh solution, which appeared but very slightly affected at the end of that time. The amount of carbide obtained from 100 of steel was only 4.66. It was found to have the following percentage composition:

Carbon.............	11.77
Iron...............	80.57
Water.............	5.57

There was not sufficent material for a repetition of the analysis, nor for ascertaining the proportion of residual carbon after treatment with chlorhydric acid. For comparison the amount of carbon unconverted into hydrocarbon, by treatment of the original steel with chlorhydric acid, was determined and found to be:

0.039 per 100 of steel, or 3.41 per 100 of carbon in the steel.

Table A is a tabulated view of the results obtained in these four series of experiments. An examination of the foregoing results suggests the following observations:

1. The two chromic solutions used for the production of preparations 1 and 2 (which differed but little from each other in regard to the amount of chromic acid present, and were produced with a bichromate solution saturated, or nearly so, in the cold), furnished results in all respects very similar, though the details of treatment of the steel with these solution differed somewhat. The third, a much weaker solution, furnished results which, allowance being made for the small quantities of products to be dealt with, and difficulties of their analytical examination, must be regarded as closely resembling those obtained with the other two solutions.

TABLE A.—RESULTS OF TREATMENT.

	Preparation 1.	Preparation 2.	Preparation 3.	Preparation 4.
Carbide obtained per 100 of steel.	13.25	14.16	15.34	4 66
Composition per 100 of carbide—				
Carbon...	7.31	7.21	6.84	11.77
Iron......	90.42	90.64	91.50	80.57
Water....	2.37	2.27	1.63	5.57
Atomic ratio of iron to carbon....	Fe 2.65 to C_1	Fe 26.94 to C_1	Fe 2.867 to C_1	
* Parts of carbon obtained in form of carbide per 100 of steel....	0.969	1.021	1.049	0 548
Carbon unconverted into hydrocarbon by treatment of carbide with chlorhydric acid—				
Per 100 parts of carbide......	1.410 / 1.288	1.269	0.836	
Per 100 of carbon in the carbide.	20.87 / 16.93 (Mean 18.9)	17.60	12.22	

* Had the chromic treatment given rise to no formation of hydrocarbon, the amount of carbon obtained as carbide should have been 1.144, that being the total amount of carbon in this steel.

2. The results obtained with the stronger chromic solution (preparation 4) indicate that the limit of the concentration of oxidizing power, which the separated carbide is capable of resisting, has here been exceeded. Not only has there been in this case a comparatively very considerable loss of carbon, as carbo-hydrogen (or possibly also as a soluble product of oxidation), but the iron in the separated carbide has also been to a considerable extent attacked; and but a comparatively small proportion of the carbide remains in admixture with separated carbon, the latter partly in a hydrated form, and possibly also in some partially oxidized insoluble form.

3. The proportion of combined water in the products obtained with solutions 1, 2, and 3, would seem to indicate that, in these also, the carbide exists in admixture with small proportions of a carbon-hydrate, which may be a result of the action of the chromic solutions on the carbide side first separated. This may possibly account for the not very definite, though on the whole uniform, atomic ratio of the iron to the carbon in the products of preparations 1, 2, and 3.

4. Duducting the proportions of carbon, reconverted into hydro-carbon by treatment of the products with chlorhydric acid, from the percentages of carbon in the products obtained in preparations 1, 2, and 3, the results exhibit a uniformity which, if accidental, is somewhat remarkable. Thus:

TABLE B.—FINAL PROPORTION OF CARBON.

	Preparation 1.	Preparation 2.	Preparation 3.
Per cent. of carbon in product......	7.31	7.21	6.84
Less carbon unconverted into hydro-carbon......	18.9%= 1.38	17 6%= 1.27	12.22%= 0.84
There remain of carbon per cent.	5.93	5.94	6.00

The atomic ratio of this residual percentage of carbon is as 1 to 3.270 of iron.

5. It will be observed from table A that the amount of carbon, eliminated in the solid form by the chromic treatment, most nearly approaches the total amount (1.144 per cent.) of carbon contained in the steel, in the case of No. 3, when the weakest chromic solution was employed —a result which was anticipated. Even in this case the two figures do not approach each other quite as closely as they did in the case of the specimen of cold-rolled steel, referred to in the preliminary report; but this may perhaps be in part ascribable to the circumstance that, in the latter case, no search was made for water, in the products obtained by the chromic treatment. It appears conclusively established that, in all instances, some portion of the carbon is expelled as hydrocarbon by the chromic treatment; and that some small and variable proportion of the carbide, separated from the cold-rolled steel by the chromic treatment, is acted upon by chlorhydric acid, with disappearance of iron and formation of some carbon-hydrate, concomitantly with the formation of some oxidized products.

6. On the whole, these results, which are in all respects more complete than those obtained in the much smaller and really preliminary experiments described in the former report, appear to furnish some foundation for the belief that the material separated from cold-rolled steel, by the action of a sufficiently dilute chromic acid solution, contains an iron carbide corresponding or approximating to the formula Fe_3C_1, or to a multiple of that formula. The requirements of such a formula are intermediate between those furnished by the original percentage composition of preparations 1, 2, and 3, and by the composition of these, after deduction of the proportions of carbon unconverted into hydro-carbon by their treatment with chlorhydric acid.

The results of these experiments with cold-rolled steel of a particular composition appear at any rate to confirm the correctness of the view that the carbon in cold-rolled steel made by the cementation process exists, not as simply diffused mechanically through the mass of the steel, but in the form of an iron carbide —a definite product, capable of resisting the oxidizing effect of an agent which exerts a rapid solvent action upon the iron through which this carbide is distributed. Whether this carbide varies in composition to any great extent, in different descriptions of steel, which are in one and

the same condition of preparation (*i. e.* cold-rolled or annealed), remains to be demonstrated by further investigations, if the determination of this point is considered of sufficient importance to warrant the expenditure of the time and labor which it would involve. The preliminary experiments with small specimens of cold-rolled, annealed or hardened steel, described last year, appeared to warrant the belief that the condition of the carbide in the metal is affected to such an extent by the process of hardening, as more or less completely to counteract its power to resist the decomposing effect of such an oxidizing agent as chromic acid solution. How far this may always be the case, and how far it may be possible to prove that similar effects to a modified extent are produced by submission of steel to tempering processes in different degress, may perhaps be determined by further research in this direction.

RECENT HYDRAULIC EXPERIMENTS.

By Major ALLAN CUNNINGHAM, R.E., Fell. of King's Coll., London.

Minutes of Proceedings of the Institution of Civil Engineers.

III.

Correspondence.

Mr. Flamant considered the author's experiments to be extremely important, and that his book, full of practical details of the manner in which he worked, should be read by all those who proposed to undertake similar experiments, or who desired to study the phenomena, still very obscure, which occurred in the flow of water in large open channels. It was for this reason that he had referred to the work in a note inserted in the "Annales des Ponts et Chaussées," desiring thereby to induce French students of the subject to consult Major Cunningham's book. It was impossible to render too much praise to the care and exactitude exhibited by the author in his operations, and the remarks that followed referred wholly to questions of theory or of rightful interpretation.

The author said (chapter v.) that according to theory, the interior pressure of flowing water in permanent motion was less than that in the case of still water, and that it diminished with the velocity. Consequently, if a body of still water were connected by a tube of small aperture with a running stream, the level of the latter would be higher than that of the still water. He had proved this by experiment, but the difference of level shown had been very small (about 0.07 foot). Here, Mr. Flamant thought, was an error of interpretation. The tube in communication with the running stream would project from the bank. It would, in consequence, oblige the particles of water impinging against it to take a curved trajectory instead of preserving their rectilinear direction. In the path of this trajectory, of which the concavity would be turned towards the tube, was developed a centrifugal force, tending to throw the particles of water from the tube. In this way a kind of suction was set up in the tube, which would diminish the pressure, and consequently lessen the height of the water in the adjacent reservoir, making the latter less than that of the stream, which indicated the real pressure, except in the immediate neighborhood of the tube. This effect would be the more marked the greater were the centrifugal force, *i. e.*, the velocity of the stream. The author had been able to show that the difference in the two levels increased with the speed of stream, but he would not have observed any difference at all if the tube had been placed close to the slope of the stream's bank without projecting into the water.

The same would apply, Mr. Flamant thought, to the author's statement in chapter viii., that the surface of the water should be convex transversally. The author endeavored, without success, to measure this convexity. Nevertheless, as the velocity of the water at the banks was nearly *nil*, the level should be, according to his theory, sensibly that of still water,

and, logically, he should have been able to find the same difference between the level at the banks and in mid stream as between the latter and the water in the reservoir. But between the water near the banks and in mid-stream there existed free communication, without the intervention of any tubes to interfere with the conditions of flow, so that the pressure was the same at equal depths, *i. e.*, the surface was necessarily horizontal. The instance of convexity of surface noticed by Baumgarten in the case of the Garonne did not correspond to a permanent condition, but to a period of rise in the stream, at the beginning of which water would mount more quickly at the middle than near the banks.

Lastly, in chap. xvii., the author stated that he had noticed near the banks, and on the surface, a persistent current from the bank towards the middle, which current was greatest inshore, and rapidly diminished as it approached the center. It seemed to Mr. Flamant that this current towards the middle could only be an illusion. When there was placed quite close to the bank a float, whose dimensions could not be neglected in respect of its distance from the bank, the fluid veins which impinged against it differed in force sufficiently to make the float turn on its axis, and drive it from the shore. This effect would diminish in proportion as the distance from the bank being greater the velocities of the fluid veins became more assimilated. Moreover, a current implied a displacement of liquid; it would therefore be necessary that the water flowing from the bank towards the middle should be replaced by the product of a similar current from the center towards the bank. The author thought that there really did exist such a current, for he had noticed that loaded rods placed near the bank were less acted upon than surface-floats, and that they preserved a movement sensibly parallel to the bank. This might be explained by the fact that loaded rods were endowed with a motion comparable to the mean velocity on a vertical, and that all things being equal, this mean velocity varied less than local velocities, and especially than the surface velocity. The author had himself observed that the curve of mean velocities was often more flat than the transverse curves.

These objections referred only to secondary points; they in nowise vitiated the observations themselves, which had been made with a care and good faith which must be generally acknowledged, neither did they affect the main conclusions arrived at by the author concerning the discussion of the formulas of mean velocity, and which were amply justified by the observations.

Mr. Robert Gordon remarked that the hydraulic researches published by the author of the paper at Roorkee, in 1881, formed a most important connecting link between the previous experiments of Messrs. Darcy and Bazin, and others, on small regular channels with uniform flow, and the more numerous experiments on large natural channels with varying flow, of which the elaborate investigations inaugurated on the Mississippi by Messrs. Humphreys and Abbot formed the type, and, perhaps, the most important example. Neither in his aims nor in his results did the author strive to raise any new problems; but throughout his inquiries, as narrated in his Report, he restricted himself to ascertaining what light the careful and accurate experiments conducted by himself, and involving an immense amount of conscientious labor of the highest class, could give to the problems now perplexing hydraulicians, and to placing on record for students of the science the elaborate and clearly arranged data gathered in his inquiries. The Ganges canal, at the part where the experiments were conducted, reached the magnitude of a river, both in its cross-section of nearly 200 feet wide by 12 feet deep, and in its discharge of nearly 7,500 cubic feet of water per second. It verged on the conditions of a river, also, in its varying and irregular flow; while, from the complete control over some of the principal elements influencing the flow, it retained all the advantages of an experimental channel. No person could peruse the Report without recognizing its high value as a permanent contribution to the science. It did not mark out a new era, like the works of Guglielmina or Du Buat, or those of Darcy and Bazin, and Humphreys and Abbot; but it consolidated on a firm basis the results acquired up to the present, enabled the deficiencies of the science to be seen, and might inaugurate a new departure with definite

aims and scope. A brief notice of the present condition of the science, as ascertained by the author, and generally confirmed by other writers, might be presented. Before doing this two exceptions might be taken to Major Cunningham's work, one relating to the matter, the other to the treatment. These were the only ones that occurred to him, and were suggested with diffidence. First, the Ganges canal itself did not offer, in any of the experiments taken, all the conditions for free flow. At the end of each reach of the canal a permanent weir had been constructed to check the velocity of the water; and on the permanent weir temporary weirs were placed, which had the effect of changing the character of the flow throughout the whole reach of the canal above it. These changed conditions were recognized and fully stated by the author, but the fact remained that in not a single instance was the discharge in the condition of free flow normal to ordinary rivers, and also to experimental canals. How far the analytical results were changed, or if at all, especially in the relations of the velocities on verticals, and in the comparison of the measured discharges with those given by formulas, it was impossible to say.

The second exception as to treatment referred to the use made of the method of least squares in displaying the results of the velocities on the verticals. Obviously, before it was used at all, some law must be assumed to exist and to be known, or else an arbitrary formula must be chosen, and all that the method of least squares could do was to assign the most accurate indices and coefficients to the factors already assumed in the law or formula. There was, therefore, an objection to its use before the law had been ascertained by which the order of the elements changed. The author assumed that the order of the velocities on each vertical changed like that of a parabola, and determined the particular curve in each case. He also assigned an arbitrary weight, or value, to the velocities according to their distances from the surface, when taken by the double-float, giving the lower velocities a very much inferior value to the upper ones (chap. xi., p. 4). Theoretically he was correct; but his practice appeared exaggerated, and not sufficiently supported by evidence. As the vertical velocity-curve was the most important of all the analytical results yet obtained, it was desirable to preserve it as free from alteration as possible. Probably the future science of hydraulics would take its principal direction from the teachings of this curve.

That the science needed a new direction was evident from the author's words, where he summed up the results of his comparison of the measured and calculated discharges of the canal. He tested all the best known formulas in ordinary use, and said: "The general result of trial of these formulas, which are all empirical, shows that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labor of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the author hopeless to attempt further improvement." This statement was but the echo of almost every writer of authority on the subject within the last few lusters. It had been the one leading object of almost every investigator for a long time to, in the first place, improve and facilitate the mode of research, and the means for calculation for practical engineers; but the further the inquiry was pursued, and the more accurate and elaborate the data, the wider were the discrepancies between practice and theory. Indeed, hydraulics, as it existed, was a science without a guiding theory, as the old theories had been entirely discarded by writers of judgment and experience. But there seemed a diffidence, or a reluctance, or a shrinking from the responsibility of originating a new theory, or even of examining tentative theories on the part of the investigators, to whom practical men looked for guidance in developing the resources of the science to the urgent requirements of modern life. The experienced practician by tact and judgment might overcome difficulties in the field, often at an immense expenditure of time and money, but he could not transmit his personal acquirements, nor enable schoolmen and professors to embody in their text-books and lectures the rationale of his results, unless they were prepared with a well-grounded theory to fit them in to the series of facts consti-

tuting the science. So far from throwing new light on old problems, each new investigator, from lack of a rational theory, only rendered them more obscure.

That this was no exaggerated view was shown from Major Cunningham's other results. He investigated the amount and value of the surface slope of the canal. That this slope was one of the most important elements in calculating the discharge of a stream was well known, yet this was what the author said of it:— "The general conclusion from over five hundred cases was, that surface-slope measurement is so delicate a matter that the results are of doubtful use." Mr. Gordon could not agree in this conclusion as a general one, though it seemed applicable to the Ganges canal, solely on account of the weirs at the lower end of each reach. From experiments made on the Irrawaddy, now extending over several years, he believed that it was possible, by increasing the number of points and spreading them over a considerable space in order to make continuous or daily observations, to ascertain the general slope of a river or canal with sufficient accuracy to permit it to enter into a formula as a factor. But a very few observations, neither simultaneous nor extended, such indeed as had been too often used in some published results, were most misleading, and, taken with the utter absence of theoretical guidance to indicate the degree in which they should enter into the formula of calculation, landed the user of them in a dilemma of bewilderment.

The author did not leave the discussion on the practical methods of measuring the discharge of an open channel in a satisfactory condition. For small regular channels there was not much difficulty; but for natural channels, and even for the Ganges canal, the final solution as to the best instrument for measurement had not been obtained. He favored the double-float; in this agreeing with Messrs. Humphreys and Abbot and himself. But the author did not treat the results with sufficient respect in his use of the method of least squares. He also approved of the rod, which apparently answered well in experiments like his own, and those of Mr. Francis at Lowell, in regular-shaped channels, not of great depth. But in moderate-sized channels of irregular shape, it had been found by the experimenters in the Rhine in Holland, that the rod was incorrect and unsuitable. They and other continental investigators appeared now to have entirely adopted the Woltman meter in one or other of its various forms. The author, however, seemed unequivocally to condemn these, so far as his experience extended, and quoted General Abbot with approval, who held the same view still more strongly. Mr. Gordon had for several years carried on extensive experiments on the Irrawaddy with double-floats, and had endeavored during the last flood-season (1882) to test these with three of Deacon's electric current-meters. The Indian Government gunboat "Irrawaddy," with a large staff, was at his disposal, and most elaborate tests had been made. Two of the meters had been sent to be re-tested at Torquay by Mr. Froude, who originally ascertained their coefficients at the Admiralty experimental tank, and the result was not yet known. But from a careful examination of the results of the experiments, which were conducted in depths of from 12 to 100 feet, and up to velocities of 8 feet per second, it was feared that the instruments changed their rate in the silt-laden water, which also carried an immense quantity of fibrous vegetable matter in a fine state, the lowering wire and meter often coming up covered and clogged with this.

While, therefore, he believed that hydraulicians were deeply indebted to Major Cunningham for his valuable labors, and for placing the true state of the science so conspicuously forward, it was much to be regretted that it was not found possible to bring within the scope of the work some discussion or indication of the rational theory, by which alone the present confusion could be overcome, and real progress ensured. The author had already shown in separate papers that when he took up this branch of the science he would do much to elucidate it, and when the clue was found it was hoped that the present experiments would find their true interpretation, with those of others, in building up a sound structure of history.

Mr. G. Hagen, of Berlin, held that the laws hitherto discovered on the motion of water in rivers were of no value practically. The subject was too complicated, and very difficult, and the observations had not been sufficiently exact. He had already given his views respecting the

Roorkee hydraulic experiments in "Zeitschrift für Bauwesen."

Mr. J. S. Hollings remarked that, were it possible to ascertain with accuracy the mean evaporation of the whole globe, it would most likely be found to be exactly equal to the rainfall (for rain was but condensed vapor); otherwise the sea would be either advancing or receding on all shores alike, for the ultimate goal of all water not evaporated was the sea. Living as he did in Montserrat, a small island in the tropics, he had ample opportunity of observing how very little rain fell on the sea, compared with the land. He felt sure, if correct measurements of evaporation from the sea could be obtained, they would show a large excess over the rainfall on its surface; whilst on the other hand, the rainfall in a mountainous country would be in excess of the evaporation. So many causes, however, tended to modify evaporation, that it was almost impossible to keep such a gauge correctly. The temperature in the sun in the West Indies rose to about 150° Fahrenheit on hot days, and was rarely lower than 78° in the shade, calling the shade the coolest place available for a thermometer. This latter was also the mean temperature of the sea, whilst 80° was that of the air over the land in the coolest place. On some days with little wind, the wet bulb of the hygrometer in the shade was only 1° less than the dry, whilst on other days, with the same sun-heat, 10°, and occasionally 12°, difference were registered, the only altered condition being the movement of the air. The evaporation from equal surfaces of water on sea and land would most likely also differ materially under the same conditions of sun-heat and air movement, for the vast reserve of unheated water in the sea would continually retard evaporation from the surface water, whilst the comparatively thin stratum exposed to the sun in lakes, rivers, canals, reservoirs or pools, would soon get heated through and rapidly evaporate. It was therefore almost impossible to obtain sufficiently reliable results from any number of evaporation gauges on land, or floating in terraqueous suspension, to form a basis for calculating the mean evaporation of the globe. He believed an evaporation-gauge had been kept at Barbadoes for several years, and that the average evaporation was about 3 inches per month, which agreed with the author's $\frac{1}{10}$ inch per day. The maximum evaporation was, he thought, under 4 inches in any month; whilst the average rainfall was about 54 inches per annum. The hygrometer readings in Montserrat agreed pretty closely with those at Barbadoes, whilst the rainfall at sea-level was about 46 inches; at 250 feet above sea-level, 56 inches, and at 1,200 feet above, 84 inches. Thus, wooded mountains of only a little over 1,000 feet elevation, led to nearly double the rainfall, whilst at the same time they probably lessened evaporation by more than half; for at the higher elevation he had never seen a difference of more than 5° Fahrenheit between the wet and dry bulb, and rarely more than 2°. All this tended to prove that, until a series of observations embracing different grades of level of land, and varying depths of sea, both for evaporation and rainfall, were collected, the question of whether the one was in excess of the other could not be answered.

Mr. James Leslie observed that the experiments showed a smaller velocity at the surface of the water than at some depth. Might not this arise from the floats being slightly above the surface of the water, and so being impeded by the air, if calm, or still more if there was some wind in a direction opposite to that of the water? The average velocity seemed to have been taken with great care from many points at different depths, and at different horizontal distances, and if the sectional areas were given, would afford accurate data for calculating the discharges. The surface fall in any ascertained length might have been given with advantage, and either two or more cross-sections in that length, or the sectional areas and rubbing surfaces, so as to afford means of ascertaining the mean hydraulic depth, and thereby the proper coefficient for any formula adopted, for arriving at an approximation to the velocity and discharge of any river, without having to go through the troublesome operation of finding the average velocity. He thought the alleged difficulty in ascertaining the surface-level, owing to the undulations, might have been obviated by having a vertical tube or cylinder, with a small orifice at the bottom like a marine barometer or a tide gauge.

The experiments on the evaporation

from the surface of water, showing it to be only $\frac{1}{10}$ inch per day, were important, as correcting a widely entertained notion that in Great Britain the evaporation from the surface of water was much greater than that stated, and even considerably more than the loss from agricultural or pasture land.

OBSERVATIONS MADE AT FERNIELAW, COLINTON ON THE NORTH SLOPE OF THE PENTLANDS AND 500 FEET ABOVE SEA-LEVEL.

Date.	Rain Gauge.	Loss from Surface of Water.	Loss by Deep Drainage.	Loss by Surface Drainage.
	Inches.	Inches.	nches.	Inches.
1871	35.2	19.4	16.4	20.4
1872	49.1	13.0	19.4	18.2
1873	34.4	12.8	15.5	14.7
1874	32.0	18.0	18.7	18.9
1875	34.1	14.2	17.6	17.0
1876	42.9	15.4	19.7	19.3
1877	45.7	15.2	17.1	16.3
1878	34.4	18.9	20.0	22.2
1879	39.1	12.7	15.9	15.5
1880	34.4	14.5	18.7	18.3
Average	38.13	15.41	17.90	18.08

RAINFALL, AND EVAPORATION FROM THE SURFACE OF WATER IN A TUB 6 FEET IN DIAMETER, AND THREE FEET DEEP AT GLENCORSE FILTERS ON THE SOUTH PENTLANDS, 650 FEET ABOVE SEA-LEVEL. THE LEVEL OF THE SURFACE OF THE WATER WHEN SET AT ZERO IS 1 FOOT BELOW THE TOP, THUS MAKING THE DEPTH ONLY 2 FEET.

Date.	Rain.	Evaporation.	Date.	R in.	Evaporation.
	Inches.	Inches.		Inches.	Inches.
1857	34.90	12.30	1869	34.15	14.15
1858	28.75	10.90	1870	27.20	11.90
1859	35.05	12.60	1871	34.45	10.30
1860	30 00	9.50	1872	52.20	9.25
1861	38.50	10.00	1873	36.30	10.15
1862	42.80	10.25	1874	37.75	11.60
1863	38.90	12.45	1875	37.90	11.95
1864	35.75	12.15	1876	45.35	12.11
1865	35.60	9.50	1877	54.30	11.05
1866	37.50	9.10	1878	38.40	13.95
1867	37.75	10.45	1879	46.75	11.65
1868	46.00	14.70	1880	45.00	12.10
			Average	38.80	11.42

He had found, from many observations, that in dry summer weather, evaporation from a surface of water in Scotland was only $\frac{1}{12}$ inch per day, and throughout the year it was considerably less than the loss from the surface of land having vegetation on it, showing that it was a mistake to deduct the surface of a lake or reservoir from that of a gathering ground.

As the Indian observations on the evaporation from a surface of water were generally made during dry weather, those observations during wet weather being mostly rejected, it might be assumed that the average evaporation for the whole year would be much less than $\frac{1}{10}$ inch per day.

The evaporation was from the surface of water in a vessel about 1 foot in diameter, and from two zinc tubs about 2 feet in diameter, filled with earth and growing grass, the one outlet being at the surface representing undrained land, and the other at the bottom representing drained land.

MR. R. E. MCMATH, of St. Louis, Mo., U. S. A., observed that the theory upon which slope-formulas were based required uniformity of cross-section as an indispensable condition. The longitudinal section, Plate 1, "Roorkee Hydraulic Experiments," showed that the immediate vicinity of the experimental sites did not satisfy this condition. Therefore verification of slope-formulas could hardly be expected from the experiments, and it would not be fair, upon their evidence, to press the conclusion that empirical formulas were of "uncertain applicability." Limited in application they certainly were.

Likewise the underlying theory of discharge tables did not allow of change in the conditions of out-flow from the reach. The experimental sites were subject to the influence of the "State of control," and consequently observations should alone be compared, which had been made under similar conditions of obstruction in distributaries and at the tail of the reach. Any change in these conditions required a new table rather than complication by "double entry."

As a first impression, it might seem that the tendency of the work was to discredit formulas and discharge-tables. A truer interpretation of its teaching was that both must be used within rigid limits, and not loosely as had been the practice. Both methods were useful when properly applied, and were indispensable to the engineer, the one when designing, the other for continuous-discharge deter-

mination. But this necessity attached to essentials, and not to forms.

The author said in regard to formulas, "Until some guide is obtained from a rational theory, it seems hopeless to attempt further improvement." An important step towards a rational theory and practice would be effected when recognition was taken of the wide difference between an artificial channel of unvarying section and uniform bed-slope, and the natural stream in which both varied continually. The use of a formula containing slope and hydraulic mean depth was strictly limited to the former. Otherwise $S=\frac{\text{Fall}}{\text{Distance}}$, and $R=\frac{\text{Sectional area,}}{\text{Wet border}}$ were not properly associated together, S being for a distance, and R for a single point in that distance. To be logical

$$\frac{\text{Cubical contents of reach}}{\text{Wetted surface}}=\rho$$

should be taken instead of R. In such case the particular section chosen for a site should be that in which $R=\rho$, the latter being determined for the full slope-length. This involved another condition. The local values of R must either increase or diminish throughout the slope-length, in order that ρ might be a hydraulic mean, as distinguished from mere arithmetical mean.

These conditions had not been observed even by the originators of slope-formulas, and yet, he thought, need only be stated to be accepted as controlling.

In a natural stream the unequal distribution of fall suggested that surface gradient, local slope, measured the retarding rather than the accelerating force, answering to the force expended in overcoming resistances, rather than to that which produced motion. Momentum and facility for transmission of pressure had to do with motion in open as well as in inclosed channels, for the current often continued when slope was negative. A rational theory recognizing these facts would have regard to head and not to its accidental distribution.

The possibility of useful discharge-tables—and there were such—was so closely connected with the better theory, that to show the conditions of the regular law of discharge implied in a table was, so far as it went, the development of the theory.

1. For a discharge-table, it was essential that the preliminary observations should be made at the site for which the table was to be prepared; or that simultaneous gauge-observations should be made and referred to a common datum, so as to refer each observed discharge to the proper local height.

2. The cross-section should be a regular figure, at least above the level of no-flow, preferably a rectangle.

3. There should be a considerable wet area remaining when the water was drawn down to the lowest possible level. For area controlled the increments of velocity, and determined a discharge-curve, which, if not simpler, was at least flatter, and therefore determinable with less percentage of error than when area became small. This condition located the discharge section in a "marked hollow in the bed-slope," and required it to be in the obstructed sub-reach.

4. In the obstructed sub-reach, the crest of the fall defined the level of no-flow. In a river, the crest of the shoal defined that level in the reach above. This level was different from the plane of low water. It fixed definitely the origin of the discharge-curve.

5. The conditions of discharge from the reach must be invariable. The utility of discharge-curves rested upon their affinity to a weir-formula; or that the discharge over a given weir, whether free or submerged, was for a given head a definite quantity. If the opening at the weir were changed, in any way, the discharge would be affected. The opening of side-channels (distributaries) might be the equivalent of lowering the crest, as well as of increasing the area of discharge.

6. The conditions of approach to the reach should be such, that currents should follow the same general direction at high and low stages, and that no eddy should at any time exist at the discharge-section.

7. In a river it was important to select a site remote from a tributary, if possible, but below rather than above.

By observing the above conditions the results would be reliable if the local law of discharge was well determined.

For such determination a series of observations would be necessary, covering a range of stage as wide as practicable. The conditions stated were intended to make the discharge conform to an equa-

tion of the second degree. Since adopting the author's notation, $D=AV$, an equation of the second degree implied that the varying values of A and V should each follow the law of a straight line. In a rectangular section this was secured for A.

$$A=bh.$$

Also if the section was irregular below the level of no-flow and rectangular above, for the area below no-flow level might be represented by a constant a, and

$$A=a+Eb.$$

E was the elevation of surface above the level of no-flow.*

For the velocity-law recourse must be had to the discharge at the fall, and to the conditions of motion in the obstructed sub-reach.

The head of the sub-reach was defined by the intersection of the line of no-flow with the "thalweg" profile. Taking the elevation of the surface at the head of the sub-reach above the crest of the fall (the weir, not water-crest), E was obtained, the head which controlled motion in the reach. A certain part of E was consumed in overcoming resistances; the remainder, $E_1-E_2=E_3$, was found at the fall where the discharge D, definite for a given value of E_3, passed through a section of fixed dimensions A, with a mean velocity V, due to the fall E_3. At no other point in the sub-reach was there a velocity approximating that due to the fall, local or aggregate, being clearly too great for the local ΔE, and too small for the aggregate E. At any section with an area A, the mean velocity V, would be the quotient of $\frac{D}{A}=V$, and $\frac{V_1}{V}=\frac{A}{A_1}$. This relation of velocities at the section in the reach and at the fall held at all stages; consequently if there was a law at the fall, there must be at every section in the reach a direct relation between E and V.

If consideration was given to the increments of V at any section for definite increments, e, of E, evidently $\frac{A}{A+eb}$ and $\frac{A+eb}{A+2eb}$ would differ but slightly, and by a nearly constant difference, if the initial value of A was large. This was provided by the third condition, therefore the actual values of V would be closely approximated by the equation of a straight line,

$$V=cE+d.$$

Combining with $A=a+bE$, then

$$D=Av=cbE^2+E(ac+bd)+ad.$$

For the coefficient c, and constant d, the measured discharges must be depended upon.

Within the ordinary range of measured discharges it would be found that, the better the observations, the more closely would the mean velocities approach a straight line. But if accuracy was reached in the first decimal place, the fact that the law of mean velocity was of the second degree became apparent. The curve was very near a hyperbola,

$$V=\left(\frac{A^2}{B^2}E^2+A^2\right)^{\frac{1}{2}}\pm A,$$

A being the transverse axis, and an apparent function of $\sqrt{2gE}$, B being the conjugate axis and an apparent function of mean depth, or hydraulic mean depth.

Since curves were by observation concave or convex to the axis of E, according as mean depth was less or greater than an undetermined limit, it was suggested, that when the hydraulic mean depth of discharge section was equal to

$$\rho=\frac{\text{Cubic contents of reach}}{\text{Wetted surface}},$$

the velocity law was a straight line.* This furnished another condition of choice for a permanent discharge site.

8. The local value of R should approximate that of ρ for the obstructed sub-reach.

According to the view taken, the section at the crest of the fall was the limiting section for the sub-reach; but this was true only as it was the section of least area. For the head, E, in the reach was that required to force the discharge through the smallest area. The relations shown would be destroyed, if the site cho-

* In a paper read before the American Society of Civil Engineers, No. ccxxxix., vol. xi., Transactions, 1882, this element, new to hydraulics, was named "ruling-depth," and was represented by Δ, but as that symbol was appropriated, Mr. McMath therefore now substituted E, an abbreviation for "elevation above weir-crest."

* If this suggestion was not strictly correct, it was at least an approximation and of value as a practical guide in selecting a site.

sen was above a part of the reach so narrow that the area at high water might be less than at the crest of the fall. The limiting section must be least in area and depth. The argument had followed the case of a dam of free overflow, in order that the definite value and influence of E might be unmistakable. The relations would be no less useful if the dam was submerged, or if it was a natural shoal.

Hints of the more rational theory had appeared in the foregoing discussion. It might therefore be appropriate to add, in closing, that the better theory could only be reached and tested by hydraulic experiments, whose results should be cleared of the effect of "unsteady motion." This could not be done by any method of direct velocity measurement; but could be by making an absolute measure of the quantity discharged in a given time. Mr. J. B. Francis, in his Lowell experiments, obtained mean velocities accurate to the fourth decimal place, as was evidenced by systematic residuals when compared with approximate computed curves, good to the second decimal place. With such data to work from, the way to a rational hydraulic theory would not be long.

Mr. John Neville thought the measurements to obtain the average velocities and depths (pp. 7 and 8) should have been extended for longitudinal as well as for cross-sections. The width should have been divided into several sub-runs or sub-channels, moving side by side according to circumstances, but not less than three. Such measurements would give a simple and sure operation for finding the discharges from top to bottom within the widths of each sub-run; and between the banks, by adding the sub-discharges together (similar to the use of offsets to a chain line in surveying), which would obviate the use of formulas (p. 27), by suitable admeasurements.

The means of determining the bed, side, and mean velocities was not very clear. The author said (p. 25), "The decrease of forward velocity is so rapid close to the banks as to make it clear that the forward velocity must be very small:" "it does not admit of direct measurement." And again, "In a float-course only 7½ inches from a straight vertical bank one hundred surface-floats were run before three (3) were obtained in fair course over a 12½-feet run." Mr. Neville had himself often found a backward surface-flow near the banks of rivers. It was stated (p. 10) "that the range of velocities, deduced from a number of similar floats run in rapid succession over nearly the same float course, was commonly 20 per cent. of the mean. In some of Harlacher's experiments a current-meter was fitted with electrical connections so as to record every revolution; the variations amounted to from 20 per cent. in surface-velocities to 50 per cent. in bed-velocities in a few seconds." He had often observed that the flowing section of a river did not always correspond with the section of the channel taken to the water-level. Yet an average forward discharge took place, notwithstanding many changes of direction in the motions. Practically, taking the mean velocity as v, and the maximum surface-velocity as V in feet, the formula deduced from Du Buat's experiments by Prony, viz.,

$$v=\frac{7.783+V}{10.345+V} \cdot V,$$

gave more consistent practical results for all channels in the run of professional work than the more complex formulas of later writers on the subject.

The falls, of about 8 feet, at the lower ends of the reaches on which the observations were made, must have affected the results, and depressed the surface maximum velocities down towards the level of the crest of the overfall or sill of sluices. Great disturbance must also have existed below the foot of an upper, and head of a lower, reach. The depression of the maximum surface-velocity from wind and atmospheric causes was only occasional and partial, although the earth and the atmosphere formed a sort of compound tube for the water section to flow in. Notwithstanding that the vertical velocity-curves were "all very flat" (p. 16), so flat that "any geometric curve could be fitted very close to them" (p. 17). The statement, therefore, that the "last result is of great scientific importance," viz., that "the vertical curve is nearly a common parabola, the error of whose computed parameter is often very large," was scarcely justified. Also the statement that "no confidence can be placed in values of the parameter not formed by the method of least squares;" concluding with: "After many attempts

to construct a new formula, the conclusion is drawn that the data are too uncertain to admit of it!" Page 18 would seem to show that these experiments were productive of little useful result.

That the transverse surface-curves (pp. 14 and 15) must have their convexity or concavity always small, dependent on the depths below and corresponding surface-velocities, has been well known for a long time. The great number of Roorkee experiments added nothing new to the scientific knowledge of these curves.

Observed levels of the water-service at and above the falls, in the stretches, would have given useful data for the construction of the backwater curves of much more practical and scientific value than any vertical-velocity or transverse surface-curves.

The surface-slope or longitudinal inclination of rivers was apt to lead to a great deal of misapplication of hydraulic formulas. It was so flat that, unless at rapids, a considerable change was not always apparent. The range in these observations is from 1½ inch to 30 inches per mile, or from 0.0284 to 0.568 inch, nearly, in a float-run of 100 feet. The levels themselves varied 0.07 foot, or 0.84 inch in the transverse section (p. 8). "From twelve trials of slope-lengths of 2,000 and 4,000 feet, symmetrically situate about the same site," the slopes differed 25 per cent. (p. 12), and slopes at opposite banks differed 50 per cent. (p. 13). To find the value of the slope and for what run to take it so that it should correspond with the hydraulic inclination was practically next to impossible. Mr. Neville often found wind to affect the bank water-levels; and on the callow lands along the Shannon between Athlone and Portumna, he had observed sometimes a difference up to 1½ inch at different times on each bank. There is an undulating wave-surface and ripple on rivers of any depth which affects the forward motion, causing it to vary from time to time in the same transverse section, and even simultaneously in different ones, the slopes of varying very considerably, although the difference in level may be small. Slopes 1½ inch and 6 inches in a mile gave only about 0.3 inch and 1.2 inch in over 1,000 feet, yet the small difference, 0.9 inch, in this distance involved the doubling of the velocity. He remembered a lawsuit in which one engineer, calculating the flow in a mill head-race from the surface-slope, and another from floats, differed as 3 to 1. Not only did the surface-slopes vary in the same river, but so also to the depths, including the hydraulic quantities r and s and the area of the section in motion.

Mr. Neville had considered elsewhere* the various formulas enunciated by their authors from Du Buat to Weisbach and since; most of which were applicable to pipes and rivers, properly understood, r and s being factors in all. In pipes there was no limit to the inclination from flat to vertical; for canals and rivers it never exceeded a few feet per mile. The experiments from which those formulas were deduced were select and varied, ranging from pipe-diameter ½ an inch and less to 18 inches, to open channels, large canals and rivers. There were differences, and would continue to be, between a formula grasping so great a range, in size and inclination and particular experiments; but this was of no practical importance. Very little had been done since to improve this practical branch of hydrodynamics. A formula should be framed meeting any particular group of hydraulic experiments working within practical limits; it must be in part empirical, not too complex, and capable of application to vertical short pipes as well as flat long rivers. Du Buat conceived, and first constructed, a formula to meet these conditions, using one hundred and twenty-five experiments, carefully corrected for the entrance-velocity on both closed and open channels, pipes, and rivers. Nothing better had been done since.

The results to science of his hydraulic experiments was stated by the author to be "that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labor of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the author hopeless to attempt further improvement. "A guide," and "rational theory" were given by Du Buat long ago. The surface-slopes of open channels of any good size were so small in fall, and

* "Hydraulic Coefficients, Tables, and Formulæ." Second Edition, pp. 182 to 226. Also Third Edition, pp. 188 to 251.

varied at the same time so much in ratio, that they would never be used by competent engineers or by experimenters to determine or test the observed mean velocity by the form of equation

$$v = m\sqrt{rs},$$

or any other way; though they might from the equation

$$s = \frac{v^2}{rm^2}$$

determine the slope where experiments would fail. Besides, the surface-slope in most cases was not the hydraulic inclination, nor was the average slope in 2,000 or 4,000 feet, more or less, that also found at intermediate points or at the float runs from 12½ to 100 feet long. It rested with the experimenter to determine accurately the values of s, r and m before giving an opinion on the value of any formulas containing these elements.

Mr. T. W. Stone considered that the results given in the paper did not admit of dispute, as they agreed generally with received opinion on such questions. There were, however, a few points on which he desired to remark, solely with the desire of eliciting further information. The primary objects of the experiments were, he apprehended—

1. The discovery of a good method of discharge-measurement.

2. Testing the applicability of known mean velocity formulas.

3. The discovery of a good approximation to mean velocity.

The two first objects had been attained. In regard to the third, he thought that although a practical result had been achieved in regard to existing canals, the like could not be said in reference to projected canals, or the investigation of floods in rivers, beyond the statement that Kutter's formula for variable co-efficients, applied to the old expression $C\sqrt{RS}$, would give results seldom exceeding 7½ per cent., and that such results fall far short of that given by direct discharge-measurement. In the concluding paragraph of section XXI., "Discharge-Verification," a 5 per cent. difference was called a close approximation, and it was shown in Section XX., "Mean Velocity," that Kutter's formula would give results seldom exceeding 7½ per cent.; it followed, therefore, that the author's experiments tended to establish Kutter's formula as a close approximation for mean velocity in canals and channels 200 feet wide and under. Now, Kutter's formula in effect proposed the use of the old formula $v = c\sqrt{RS}$ with variable coefficients, and to obtain such coefficients the two variable factors S and R were used. The formula agreed nearly as well as the discharge-verifications, yet the author said, Section XIV., "It was found that the direct measurement of any velocity was far more likely to give an approximation to this mean velocity than any expression yet known not involving velocities." And again, "It seems, then, that at present the direct measurement of velocity, such as the central mean or central surface, is more likely to give a near value of the mean velocity than any formula involving surface-slope." It should be constantly borne in mind, that with factors R and S Kutter's formula for variable coefficients agreed to 7½ per cent., while tests for discharge-verification varied, on the author's showing, 5 per cent. and more. A wide range of experiments would be necessary to determine what the central surface or central mean velocity was likely to be in any proposed channel without use of formulas; and such experiments must extend to channels of not only different sizes, but of all various inclinations and descriptions. It was necessary, therefore, that some formula should be used for the design of projected channels, the investigation of floods in rivers, &c., &c., and Mr. Stone maintained that a set of variable coefficients of mean velocity of discharge should be used in accordance with the circumstances of each special case, and the nearest similar recorded observation that could be obtained. This should be so with all hydraulic calculations. There was no necessity to alter the old theoretical formulas; the coefficient expressions might vary as experience became enlarged. Such expressions as

$$v = C\sqrt{RS},\ v = C\sqrt{R(f2)},$$

used with a variable coefficient determined on the lines proposed by Kutter, would give fairly approximate results. The importance of using variable coefficients was pointed out in Mr. Stone's recent work, "Hydraulic Formulæ," and was he thought, always followed by

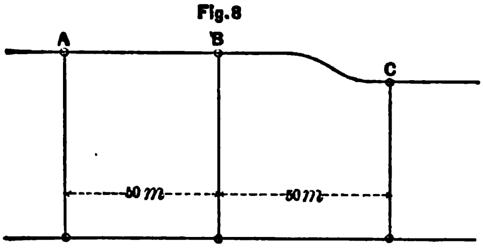
engineers experienced in hydraulics. The author's experiments had been valuable in showing that such a course led to fairly approximate results. The channels constructed by Mr. Stone in Australia were calculated from the formula $C\sqrt{R(2f)}=v$, and C was varied from 35 to 75, to give mean velocity in feet per minute, according to the nature of the proposed channel. The channels varied in section from 2 feet square to 6 feet at bottom 6 feet deep, and slopes of 1 to 1 and $1\frac{1}{2}$ to 1, and some of the channels had a circular lining; the inclinations varied from 2 feet to 10 feet per mile, and the formulas gave very fair results. To establish the statement of the author, above quoted, further confirmation would seem desirable. He thought that the assertion in Section III., "As the canal water is often full of silt. this shows that the water is in pretty rapid motion close to the actual bed, and disproves the idea sometimes advanced that an obstruction across a channel causes a still-water pool above it roughly flush with its crest," should be accepted with caution. Whilst Government Engineer in Australia, Mr. Stone built a series of obstructions from 8 to 11 feet high across a channel for the same purpose as the falls on the Ganges Canal were built up, viz., to reduce the velocity. The channel in question was about 30 feet wide at the top, and irregular in section, on account of the previous velocity having been too great for the bed and banks. The large amount of silt which accumulated in the reaches above the obstructions in this case went to show that the author's deduction was not invariably correct; possibly the frequent exercise of the control spoken of at the falls for working the canal might have tended to produce scour in the bed, and so prevent silting. At Section VI. the author first referred to the fact that both parallel motion and steady motion did not exist even approximately in flowing water, and assumed in effect that all formulas based on such motions must be incorrect. Finally, however, he admitted that there was an average steady motion. Though of opinion that formulæ constructed on the theory of steady flow would be fair approximations in connection with variable coefficients, he agreed with the author that any single velocity-measurement would be clearly an accidental value. At Section VIII, "Surface-convexity," the author stated that the fair conclusion seemed to be that "the water surface across is probably level on the average." As a general rule, Mr. Stone thought the cross-section of a stream showed a horizontal line for the surface of the water only when a uniform depth and velocity obtained right across. or nearly so. If one part of a channel were much deeper than the rest, and the velocity of the water was much greater in that part than in the other parts, the level of the water became raised there, so that the surface line became uneven. When the stream was confined to only a portion of the channel, the rest being still, or backwater, the irregularity of the surface was greatest. This was exemplified by observations taken during an investigation of the floods in the river Barwon, in Australia, in 1880. It had also been observed in some of the large drains in Melbourne, Australia. According to Mr. Chulcheth, the velocity of the stream of the Barwon was 5.4 feet per second, and the river 1,400 feet wide. The mid-stream level was convex with regard to one bank, and concave with regard to the other. The Melbourne drains spoken of were about 9 inches wide at the bottom, with sloping sides of 1 in 5 on one side, and 1 in 10 on the other. They sometimes ran from 1 foot to 1 foot 6 inches deep, and had varying inclinations.

Professor von Wagner did not possess the complete work of Major Cunningham, but only the paper read before the institution, which treated in separate chapters the principal conclusions, and in some cases dealt with them more fully. It would, therefore, be simplest to give his opinion in the order of those chapters, and to add in conclusion some remarks related to the subject.

IV. He agreed with this entirely as regarded surface floats. Large bodies, such as boats, floated, it was said, with greater velocity than the water, but small bodies. such as spherical or disk floats, might well be used for the measurement of normal velocity, because, as was proved by all experiments, no sensible acceleration could be detected. Of many experiments on this point he would only name those on the Rhine at Speyer, in which two carefully adjusted instruments, a screw current-meter and a Darcy tube,

both gave a velocity of $v=2.17$ meters, while nine surface-floats, with a path of 100 meters, gave $v=2.18$ meters. To get accurately the horizontal curve of surface velocities he selected in the transverse water-line not too many positions (for a float-path of 100 meters, about seven or eight), and then for each position he observed at least ten to twenty floats. He considered this better than to select twenty positions, and to observe at each four or five floats. It was very prejudicial that the wind had so much influence on floats, and that good results were only to be obtained when the air was nearly still.

Fig. 8



Further, he thought it important to consider the longitudinal profile of the water surface (Fig. 8). If, for example, the surface velocities at a cross-section B were to be ascertained by a number of floats, the path of the floats being about 100 meters (A B = B C = 50 meters), then it might happen that in one-half of the path B the fall might be greater than in A B. If the surface had such a form, then, in his opinion, the float-path should be shortened and the number of floats observed should be increased, in order to obtain a mean velocity.

As to those floats which reached deeply below the surface (loaded rods or coupled balls), he had hitherto not been convinced that they gave accurate results. His objections to them were stated in his "Hydraulic Experiments on the Weser, Elbe, and Rhine," p. 3. Whenever accurate measurements were required he preferred good current-meters, such as those of Amsler-Laffon, Harlacher, and others. But of floats, he only used surface floats, and these only to check the coefficients of the current meters, or to obtain approximate values as indicated below.

V. Agreeing with this, he would only add in regard to the measurement of the slope, that, if there was a rippling surface, he had always made a small side basin close to the river, in which the surface was kept still by a floating plank, or even by pouring on some oil, and thus the reading was made more definite. He was led to do this by the opinion that the water-level so obtained would be a correct mean between the wave-crest and wave-trough.

He quite agreed with VI., especially with the final words "taking" to "motion." As to theory in the proper sense of the word (hitherto everything was but empirical formula), it could not concern itself with eddies, &c., but must be based on normal conditions. The chief difficulty was the necessary modification by experimental values, coefficients, &c.

His views also coincided with those expressed in VII. Whenever the longitudinal profile of the water-surface had the form of a long curve, then, according to Harlacher's method, the true slope was the tangent to the water-surface at the intersection of the wave form with the cross-section considered.

He had found as a rule that the longitudinal slopes were the same at both banks, if the river at the site had a normal bed and a straight direction. When there were irregularities, then two levelings, one at each bank, were sufficient; but with large rivers one might be desirable in the middle, *i. e.*, in the thread of the stream; its measurement was, however, exceedingly difficult.

VIII. He had also had an opportunity of observing the transverse water-line, and had given the particulars in "Hyd. Untersuch.," p. 42.

X. The statements confirmed what he had found in other rivers:

(*a.*) That generally the vertical curves were more bent, the smaller the depth of water, and *vice versa*.

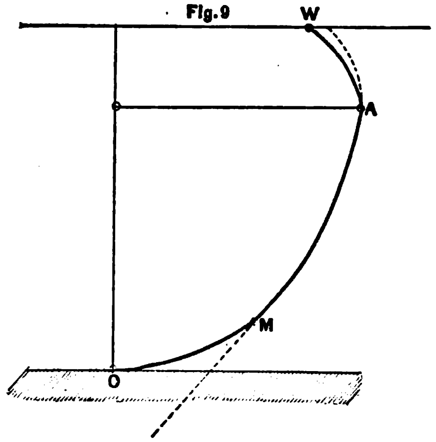
(*b.*) That the mean velocity at a vertical was smaller than that at the half depth.

XI. Differences of opinion still existed as to the position of the parabolic axis. Some placed it vertically, with the vertex of the parabola at the river bed. The majority placed the parabolic axis horizontal, and at the point of greatest velocity. It was interesting to him that

Major Cunningham placed it horizontally, as he did himself. His experiments, giving a considerable number of carefully measured vertical curves, proved that with a vertical parabolic axis were obtained results altogether useless, because the normal parabola deviated so quickly from the measured curve, that there was no dependence on the position of the vertex at the river-bed. On the contrary the results were satisfactory for the other position of the axis. In his investigation of sixty-four curves of small streams and large rivers, he had come to the following conclusions:

(*a.*) Down to a point at depth M, Fig. 9 (on the average at 0.8 of the whole depth), the curve consisted of a parabola

Fig. 9



A M, but below M, of a curve deviating from the parabola, the water being retarded by the bed. A similar deviation, though to a smaller extent, was found from A to W, possibly in consequence of the friction of the air. The vertical curve thus consisted of three parts, of which the middle one, which was parabolic, had the greatest length.

(*b.*) The axes of the parabolas were horizontal (or strictly parallel to the water surface).

(*c.*) The maximum velocity (or parabolic axis) was at from 0.0 to 0.28 of the whole depth below the surface. In the Mississippi, at 0.3. Even those curves, which were measured in a perfect calm, showed the parabolic axis below the water surface. More details of this would be found in "Hyd. Untersuch.," pp. 39, 40, and plate 8, Figs. 86 to 86 c.

XIV. The formulas here given he had compared with the values in a number of different measured curves (Weser, Elbe, Rhine Danube, &c.), and he had arrived at the conclusion that the second formula,

$$U = \tfrac{1}{2}(v_{0.211\,H} + v_{0.789\,H}),$$

gave more accurate results than the other. In most cases it agreed perfectly, the differences being never more than 2½ per cent.

With regard to the other statements in Section XIV., as to the position of the mean velocity on a vertical, he would refer to p. 37 of his "Hyd. Untersuch." It was there shown how little the position of mean velocity differed in the most different curves. Now, if to these results were added those on the Mississippi, as well as that given in Section XIV. of 0.62 or ⅝, the following figures resulted:

River. With Breadth in Metres.		Observer.	Number of Measured Curves.	Depth of mean Velocity Arithmetic Mean of the slightly varying values.
	Meters.			
Weser	80	von Wagener	6	0.587
Elbe	114	"	7	0.594
Rhine	215	"	4	0.589
"	215	"	1	0.600
Oker	14	"	3	0.600
Elbe	120	Harlacher...	9	0.620
"	120	"	10	0.598
"	120	"	9	0.580
Danube	425	"	15	0.599
Mississippi		Humphreys and Abbot	..	0.580
Ganges Canal		Cunningham	46	0.620

The arithmetic mean of these eleven values was 0.597.

If the question was simply to ascertain the discharge for an engineering purpose then he thought it would be accurate enough to take five to seven verticals on the cross-section of a river and to ascertain the velocity on each vertical at 0.6 of the depth, measured from the water-surface.

To this he would add that the maixmum velocity in the sixty-four curves, given in the Table, varied in position from the surface (for example in the Weiser) down to 0.25 of the depth from the surface (as in the Rhine), and yet the proportionate

depth of the mean velocity differed but little.

XVII. Perfectly agreeing with these explanations, he begged leave to add the following remark. He was of the opinion that at the bottom the velocity must be zero. From that point the velocity increased rapidly. The idea of "bottom velocity" was therefore very indefinite. In practice it was frequently stated, and its value was taken as equal to O N (Fig. 10), obtained by prolonging the normal parabola A M to meet the bed, although it was proved that the velocities in the part L O decreased quicker than was given by the parabolic law. In his opinion "bottom velocity" meant only that velocity which existed at the height of the center of gravity of the boulders, on which the local average size of the boulders depended. If at any place the boulders had an average size of 10 centimeters, then there the bottom-velocity was greater than at a place where the bottom was formed of sand of an average diameter of 2 millimeters (Fig. 11). It was therefore fruitless to seek a relation between the bottom and other velocities. As to the measurement of bottom-velocity, the current-meter was not very suitable because of the distance which must be allowed for the rotation of the screw-blades. By Darcy's gauge measurements could be made at 1½ centimeter above the bottom, provided it was level. This was shown by the curve Fig. 86 on Plate 8 in the "Hyd. Untersuch.," where, at each of the given depths, the difference of the water columns had been observed thirty times.

Fig. 10

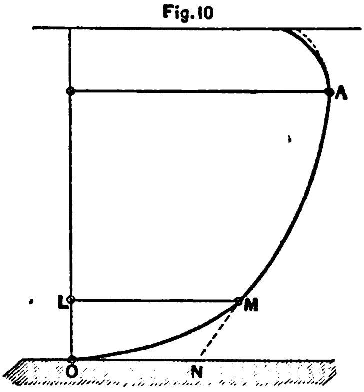
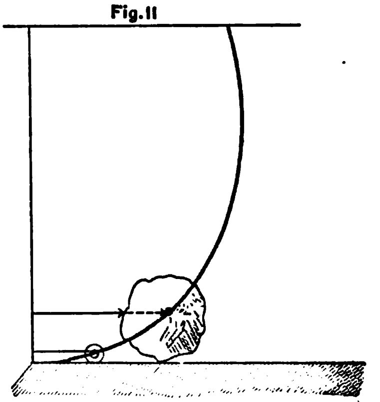


Fig. 11



XVIII. Here the remarks confirmed the opinions in "Hyd. Untersuch.," pp. 40, 41, on the approximate conformity of the horizontal velocity-curve with the form of the wetted border, which was illustrated in Plate 8, Figs. 87 to 93. It would be seen that the conformity (which he had in vain sought to express algebraically) ceased in the same river with increase of discharge (Figs. 92, 93), and besides differed near the shores (Figs. 87, 89, 93), in analogy with the alteration of the vertical curves.

XIX. He considered Harlacher's the simplest and most accurate method of determining the volume of discharge. It was explained in "The Measurements of the Elbe, Danube, &c.," by H. R. Harlacher, 1881. He had used the same method in his "Hyd. Untersuchungen," p. 18.

XX. The valuable work of the author confirmed what Professor von Wagner had said, on p. 33 of the "Hyd. Untersuch.," about the degree of reliability of different formulas, and the statements in the Table, p. 32.

As the measurements in the Ganges Canal had been conducted with great care, it seemed desirable to make use of them to test two laws given in the "Hyd. Untersuch." If a continued confirmation of these laws resulted, then it would be of importance to practical engineering.

Let,

v=mean velocity of the whole cross-section.

c=maximum surface-velocity in the whole transverse width.

v_s = velocity at the center of a figure of a cross-section.

(I.) *Relation between v and c.*—In twenty-four different gaugings of small and large streams, he had found

$$v = a\,c + b\,c^2.$$

The contestants a and b were only discussed in a preliminary way in "Hyd. Untersuch." In his article in the "Deutsche Bauzeitung" (No. 82, 1882), more exact values had been obtained by the method of least squares, and these gave

$$v = 0.705\,c + 0.01\,c^2.$$

From an article in the "Deutsche Bauzeitung," it would be seen that another gauging (of the Elbe, above Hamburg) agreed well with this equation; the calculated value was only 2½ per cent. greater than that obtained by measurement. Should this equation prove sufficiently accurate, then the engineer would only need to measure c by surface floats, in order to get a good approximation to the value of v, and from that the discharge.

(II.) *Relation between v and v_s* (p. 38 of "Hyd. Untersuch.")—For this an equation had been formed from seven rivers, or, adding the gauging of the Elbe above Hamburg, from eight rivers. Using the method of least squares to determine the constants, the equation was—

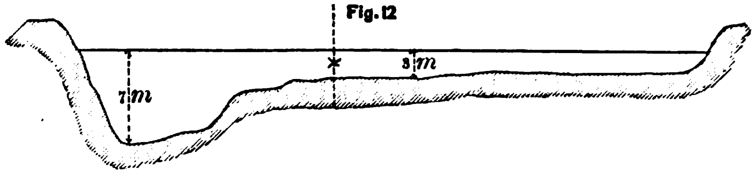
$$v = 0.738\,v_s + 0.05\,v_s^2.$$

The gauging of the Elbe above Hamburg gave by this equation v=1.16 meter, while by direct measurement v=1.17. However, eight examples for determining the constants were far too few, although the differences were only—

1.10	1.10	0.10 per cent. too great.
2.30	1.00	0.70 " " " small.
		0.00

Further the equation did not apply in cross-sections such as that of the Danube, Fig. 12, given by Harlacher. He presumed the equation would only apply to pretty regular and symmetrical cross-sections. Should the equation be confirmed it would only be necessary to determine the center of figure of a cross-section and measure the velocity at that single point. He wished that both equations might be tried by the Ganges Canal results.

Fig. 12



Major Allan Cunningham observed, in reply to the correspondence, that the points noted by Mr. Flamant about comparison of levels of still and running water in free communication, and as to non-existence of surface convexity in permanent régime, and non-existence of transverse surface flow, were of high scientific interest. Further direct experiment was very desirable. As to the mode of weighting the velocity-data of a vertical curve, objected to by Mr. Gordon, it was known that with double-floats the velocity-data increased in accuracy from the bed upwards, so that the weights assigned should also increase from the bed upwards; but at what rate was of course unknown, and therefore a matter of judgment. No stress was laid on the weighting used. Admitting with Mr. Leslie that the action of the air and wind on the projecting portions of the floats used might have exaggerated the observed depression of the maximum velocity-line, still all instruments alike agreed in showing this depression as an existing fact. The irregularity of the bed of the Ganges Canal was not greater than that of most so-called natural streams, as for instance the Thames, in which lumps, bars, hollows, etc., of 1 or 2 feet depth were common: on the vertical scale used in the plates (Vol. II. of the Roorkee Work) these features were enormously exaggerated. Most of the data asked for or suggested as requisite by several of his critics had been actually printed in great detail in Vols. II. and III. of the original

work. Whilst acknowledging Du Buat's great advance in hydraulics as previously known, it could not be admitted, as said by Mr. Neville, that "nothing better had been done since" in way of a mean-velocity formula. The Darcy-Bazin and the Kutter formulas were both important advances. Also Du Buat's "rational theory," quoted by Mr. Neville, was merely a highly general principle; the detail was still wanting to enable mathematical investigation to be properly applied to the flow of water, and in this sense a "rational theory" was still wanting. If Professor von Wagner's conlcusions, p. 90 (*a*), that the vertical velocity-curve deviated greatly from a parabola near both surface and bed were correct, and that the real bed-velocity was zero, it would be right to give up the use of the parabola approximation. Lastly, Major Cunningham desired to thank the several speakers and writers for the great value of their remarks to him in his special research; his only regret now was that these remarks were not available to help him when the experiments were being made.

PROFESSOR DAVIS ON TERRESTRIAL DEFLECTIONS.

By RD. RANDOLPH, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN a review of an article in the February number of VAN NOSTRAND'S MAGAZINE on this subject, Mr. W. M. Davis, of Cambridge, Professor of Geology, who probably relies upon the paper by F~~a~~rrel, published in the *Cambridge Mathematical Monthly*, 1859, which he refers to, aims to supply two omissions in the article he reviews.

e/

The first of these omissions is that of the consideration of the centrifugal force of the earth which tends to urge a body on its surface, whether in relative motion or rest, outward in the line of a plane radius of the circle of latitude.

While the earth was in a sufficiently fluid state its materials, in obedience to this influence, adjusted themselves into the form of an oblate spheroid and departed from that of the sphere exactly to that degree necessary to bring its attractive and centrifugal forces into equilibrium; and to present a slight gradient, towards the equator, of a perfectly horizontal surface, which is exactly that necessary to counteract the tendency of a detached mass to move on such surface by virtue of the centrifugal force, supposing friction to be absent. Therefore the consideration of this feature was entirely unnecessary in the discussion of the influence of the earth's rotation on derailments. For the hypothesis was that of a perfectly level track; and Prof. Davis admits that it had no influence upon the result. If a hydraulician were calculating the discharge of a fluid through a conduit from a distant elevated source he would omit all considerations of the earth's centrifugal force. He would leave that to those philosophers who maintain that this force is the cause of the flowing of springs.

The second omission is that of the consideration of the law of preservation of areas.

If this view of the subject is contained in the paper by Ferrel in the *Cambridge Mathematical Monthly*, its publication had been better postponed for the twenty-four years that have elapsed. The supposed moving body on the earth's surface is not a projectile nor a satellite moving in an orbit which is the result of the composition of an original projectile force with the attractive force of a primary at the focus. Such a body at every part of its orbit will, in equal periods, pass over arcs which, with the two radii vector including them, will form sectors of equal area, whether the orbit is a circle or the most elongated ellipse. But this is nothing more than the expression of the fact that when the projectile force is in excess, and the centrifugal and centripetal forces not in equilibrium, there will be retardations and accelerations, for the same reasons that the projection and fall of a mass on the earth's surface is retarded and accelerated when gravitation is

not acting at right angles with the path; and that these retardations and accelerations will coincide with the lengthening and shortening of the radius vector; so that the bases and vertical heights of the triangles composing these sectors will vary inversely with a constant ratio; thus determining them to be equal in area. But if any external force were to compel the satellite into a closer orbit without altering its velocity, as would be the case if the force acted for a limited period at right angles with the movement, the areas described in equal periods on the new orbit would still be equal to each other, but they would not be equal to corresponding ones on the old orbit. The result would be a shorter period for an entire revolution, and corresponding areas would be smaller; because one of the factors, the velocity, has not been altered, but the other, the length of the radii vector, have been diminished. So when any detached terrestial mass is moved from one circle of latitude to another, gravitation, acting always at right angles to these circles, will force the mass moving on the surface into a smaller circle of rotation. If it did not encounter friction or other resistance, it would maintain its original velocity and describe an arc of the same length on the smaller circle as that described on the larger circle in the same time. But this arc would be included by shorter radii and the area thus formed would be less; while the area would be greater if the movement was to a circle of greater radius. But the areas described by the cone radii (slant heights) of all these circles from a vertex at the center of a true sphere would be equal in equal periods as a consequence of uniform velocity of rotation. That is to say, that the sum of all the small sectors whose radius is the slant height of the cone, and which are the elements of the cone surface, would be equal in equal periods. Not because there is an arbitrary law for describing equal areas, but because these cone radii are the same at all points of the sphere; and because the velocity of rotation is not altered by the action of gravitation at right angles to any movement over its surface. Therefore Prof. Davis is in error when he assumes that the velocity of rotation is accelerated by moving towards the pole and retarded by moving towards the equator, in obedience to an imaginary law for the preservation of the areas of the circular planes of latitude; and the quantity which he adds to the deflective force is a false quantity. If such a law were in force its maximum effect would be in the case of a movement along a meridian, and would be zero in the case of a movement over a circle of latitude. For in the last case equal areas forbid any increase of velocity of rotation. Yet he has assigned a constant value for all directions and made it equal to that obtained by the considerations of what he calls "the whirling table;" by which, it is to be supposed, that the deflection of the cone radius, represented by the cotangent of latitude, is meant. But it is evident that two aspects of the same fact have been the cause of this confusion.

For the purpose of this dissension the surface of the sphere may be considered as made up of narrow zones of a succession of cones whose slant height is the cotangent of the latitude and whose vertices are at different points in the prolonged axis of the sphere. The difference in the length of any two circles of the same cone is the length of the whole circle which is at the same distance from the vertex as that between the two circles. That part of the cotangent included between the two circles does not describe exactly the same cone as the small one at the vertex only because it is required to suffer translation as well as deflection. The movement of deflection is exactly the same; and which, in one revolution of the sphere, would be the sum of all the small sectors which compose the cone. So that the difference in length of two circles of latitude is the exact measure of the amount of deflection which the cotangent has performed, and which is also measured by the circular base of the small cone at the vertex. If this cone were developed into a plane it would be the sector of a circle showing the whole angle of deflection. Now the part of the cotangent between the two circles of latitude which performs this amount of deflection, is a line belonging to a small horizontal plane which is an element of the surface of the sphere. Consequently this plane is rotating to the same extent that the line is deflecting; just as a wheel is rotating to the same extent as any one of its spokes is deflecting in the plane

of the wheel. Therefore if a body moves along any of these lines, or spokes, it will meet with the horizontal resistance due to the constantly increasing or diminishing circle of rotation of the plane or wheel. Another aspect of the same fact is the passing from one circle of latitude to another and observing the difference in the plane radii of these circles. But the circles are the same whether they be viewed as the bases of cones or the circumferences of circular planes. They measure the deflection of the meridian tangent to the earth's surface and of all the lines in the horizontal plane to which that tangent belongs. And upon whichever of these lines the body may move, north, south, east, or west, the different points of the line, moving with different velocities independent of that of translation, will offer a horizontal resistance to the moving body.

STORAGE BATTERIES.

Report of Prof. HENRY MORTON on the Storage Batteries of the Electrical Power Storage Company, Limited, of London.

HAVING made a series of careful tests and measurements of the electrical storage batteries manufactured by the Electrical Power Storage Company, Limited, at their works at Millwall, London, England, I find the following results in reference to their capacity to store and retain energy, afterwards delivered by them as electric current.

In the first place as to the *capacity* of these batteries in thus *storing* electricity, and its relation to the weight and bulk of such battery or reservoir.

The cells with which I have made my experiments are of the pattern called "one horse power" cells, because they will contain, when fully charged, an amount of energy equal to fully 1,980,000 foot pounds, or one-horse power for one hour.

These cells are externally rectangular wooden boxes, 12¼ inches high, 11½ inches wide and 5¾ inches thick.

Two of them side by side, as they would stand when in use, occupy about one cubic foot of space.

Each cell contains 16 plates, whose united weight is 48 pounds, and with the lead-lined box and liquid, the entire weight of the cell when in use, is 79½, or say 80 pounds.

One of these cells fully charged will yield, as I have found by careful experiment, a current of 32.5 amperes at the beginning, and 31.2 amperes at the close of a continuous discharge for nine hours.

This amounts to 286.5 ampere hours of current, and if even short interruptions or periods of repose occur in the use of the current a yet larger total amount can be obtained.

An Edison incandescent lamp of high resistance, giving a light of 16 candles, requires a current of .73 of an ampere to supply it. Such a current, therefore, as these batteries yield for nine hours at a time, will suffice for 44 such lamps.

To secure sufficient electro-motive force or propelling power to overcome the resistance of these lamps would, however, require about 50 of such cells, so that a battery of 50 of these cells connected in series, would operate 44 lamps for nine hours, or for even a longer time in the aggregate, if the use were interrupted, as it would be in practice.

If fewer lamps were used with the same battery, they would be operated for a proportionately longer time.

Thus 11 lamps would be supplied by a 50 cell battery for thirty-six hours of continuous action; or as lights are commonly used in private houses on the average for five hours each night, such a battery once charged, would operate 11 lamps for a week.

To express the relation between weight of battery and power of maintaining a light we might, therefore, say that for each lamp operated for nine hours 1¼ cells of battery would be required, or a weight of about 90 pounds of battery. This would be for each hour of burning each lamp, 10 pounds of battery.

This makes a very simple rule for calculating the weight of battery required for any number of lamps for any time.

Thus, suppose we wish a battery to

operate 25 lamps for five hours each night, the battery being recharged during the day. We have 25x5x10=1,250 pounds as the weight of battery required.

Comparing the efficiency of this storage battery with that of other similar arrangements, such as the Faure battery, measured and reported upon by M. Tresca, of the "Conservatoire des Arts et Metiers," it shows a marked superiority.

Thus in M. Tresca's experiments a cell weighing 95 pounds, yielded a current representing 793,791 foot-pounds of energy where your battery yields, as I have stated above, 1,826,168 foot-pounds and only weighs 80 pounds.

Your battery, therefore, yields more than twice the energy and weighs almost one-fifth less.

Even the experiments made some time afterwards by Professors Ayrton and Perry, on other Faure accumulators, though the conditions were rendered as favorable as possible by distributing the discharge over three periods of six hours each on three successive days, do not show a much better result. In this case the weight of the battery *plates* only is given, and that is 81 pounds. Reducing the results proportionally for batteries whose plates weigh 48 pounds, I find that by the experiments of Professors Ayrton and Perry each cell of this weight should give 853,333 foot-pounds of energy.

This again is less than half the amount of energy which I have repeatedly obtained from your battery.

Passing next to the efficiency of your batteries as regards their delivery of nearly the same current as was used to charge them, I have found that the loss in this relation is less than 10 per cent. In other words, in my experiments I have obtained from these batteries 90 to 91 per cent. of the current used to charge them. This far exceeds the results obtained by M. Tresca with the Faure batteries.

Tresca reports that he recovered only 60 per cent. of the current used to charge the battery, and Ayrton and Perry found the loss in charging and discharging to be "not greater than 18 per cent."

In this, however, is included the loss due to the fall of electro-motive force in the discharge as compared with the charge. This I find to be on the average less than $\frac{2}{10}$ of a volt, which would bring the total loss of available energy obtained from the battery as compared with that expended in charging it to 18 per cent.

Lastly comes the very important question as to the retention of charge during a long time. To test this I charged three cells and locked them in a closet on February 1st, where they remained until February 16th, when I began discharging them at the rate of 32 amperes, continuing this rate of discharge on the next day. I thus obtained 266.7 ampere hours of current.

Comparing this with the 286.5 ampere hours of current obtained from the other cells which I discharged soon after charging them, shows a loss of 7 per cent. caused by standing for 16 days.

The above measurements and comparisons show that this storage battery has attained a degree of efficiency which will render it applicable to a number of uses.

Thus, for example, on steam boats, by the use of such storage batteries, the irregular and occasionally interrupted motion of the main engine might operate a relatively small dynamo-electro machine so as to charge the batteries during the entire twenty-four hours, and the current from these batteries would then supply light with perfect steadiness during the relatively brief time in which it is required. In this way the cost of supplying and running a special and large engine, which would be needed for operating the same lights directly without the storage battery, would be avoided, and also the necessity for extreme steadiness in running the dynamo, and all risks of extinction of the lights from a mometary interruption of motion in any part of the machinery would be removed, as the battery would secure an absolutely steady and continuous supply of current no matter how little regular might be the action of the engine or dynamo-electric machine.

Again, in larger buildings the engine used to operate the elevator or to do any other work, if of sufficient power could charge the storage battery without interrupting its regular work, and thus

supply the light needed at a minimum cost for special machinery and skilled supervision.

In private houses where the running of a large engine with extreme smoothness and absolute certainty during the hours when light is needed, would be out of the question, a small engine operated at convenient intervals, and with no need of regularity or of fixed hours, would accomplish all that was required if a storage battery was employed with it.

I am informed that these storage batteries have been successfully applied to the running of street cars, and in view of the fact that they will contain almost 2,000,000 foot-pounds of energy for each 80 pounds of dead weight, I should consider the prospect in this direction most encouraging.

In short it seems to me that there is manifestly a wide field for the profitable employment of this greatly improved form of the electric storage battery.

DISTRIBUTION OF WATER FROM IRRIGATION CANALS.

By M. BRICKA.

From "Annales des Ponts et Chaussées," for Abstracts of the Institution of Civil Engineers.

This article indicates the importance of a careful system for the distribution of water for irrigation, as gathered from the experience of the Verdon Canal, a matter often overlooked in the construction of irrigation works.

Irrigation in Provence not merely increases the return from the land, but also enables any crop to be cultivated where failing a supply of water, wheat, olives, and almonds are almost the only produce. The gross return, which formerly hardly exceeded from £6 10*s.* to £8 per acre, has been raised by irrigation to from £13 to £24 per acre, in meadow land, and from £16 to £32 for potatoes, affording a very large increase in the net profit, which was formerly only about £1 12*s.* an acre. The profit in small holdings, from the culture of fruit and vegetable, is still larger. The water is generally supplied from April to September, over the whole surface for grass crops, and in channels for fruit and vegetables. It has been found in practice that a supply equivalent to 5⅓ gallons per minute per acre is sufficient; it is distributed periodically, the interval between the commencement of two successive waterings having been fixed for the distributing channels of the Verdon Canal at six days and a quarter, so that each proprietor may have his fair turn of day and night supervision. The rate of discharge which has been found suitable, so that, without undue expenditure of time in delivery, the supply may not exceed what can be properly distributed by the proprietor, is between 400 and 530 gallons per minute, and the quantity supplied to different properties is regulated by a variation, not of the rate of discharge, but of the period of flow.

The Verdon Canal has been previously described; and the author confines himself to a description of the distributing channels which bring the water to the head of the estate of each subscriber, and which were constructed by the same company who had the concession for the Verdon Canal. There are about 15 miles of these channels to every 1,000 acres. As economy is essential in forming the distributing channels, they have to follow the surface of the country, and to avoid severance; and the constructive works have to be restricted to falls, and crossings of unimportant roads and streams. The channels are accordingly very irregular, both in direction and slope; and numerous falls connect abrupt changes of level. Where a hard stratum exists near the surface, the amount of slope given to the channel is immaterial; but in a bed liable to scour, the rate of flow must not exceed from 2 feet 3 inches to 2 feet 8 inches per second. The flow, however, must not be less than 1 foot per second, otherwise the silt, which the water brings down in abundance after rain, or from the melting snows, would deposit and obstruct the channel. In places where it is inconvenient to replace a quick slope by a fall, scour has been prevented by dry rough pitching, resting upon a layer

of broken stone, which impedes the undermining action of the current below the pitching. The section of the channels is generally flat-bottomed, with side slopes of 1 to 1; but sometimes through firm ground, and where scour is needed, the section is made rectangular. The works of construction, which are very numerous, consist of modifications, according to circumstances, of simple types, of which various specimens are given in the plates. The total cost of the 250 miles of distributing channels, in the irrigated area of 18,530 acres in the Aix district, was about £29,600, or 1*s.* 4½*d.* per lineal yard.

The author enters fully into the methods by which the construction and control of the distributing channels can be arranged, and shows the obstacles in the way of entrusting these matters to an association of the proprietors. If the State does not undertake it, the only other suitable authority is a company with a concession. It is impossible to estimate at the outset what the demand for water will amount to. Some soils, though apparently very dry, have a clay subsoil which retains moisture sufficient for good crops of wheat, and irrigation would only render the ground marshy. Small proprietors use a large quantity of water, as they irrigate all their land; whilst large proprietors rarely irrigate more than a third. Also the change of crops takes place slowly, so that at the commencement the demand for water is comparatively small. It is accordingly advisable not to extend the distributing channels till an adequate number of subscribers come forward. It is very important that the supply shall be delivered with great regularity, to prevent injury to the crops depending on it, and that no waste should occur. This is most effectually secured by entrusting the management of the sluice gates to responsible persons, not interested in the irrigation, who distribute the water according to regulations arranged each year, and who also are on the watch to prevent fraudulent misappropriation of the supply. Under these circumstances, the cost of allotment in a year amounts roughly to between 29*s.* and 47*s.* per gallon of water distributed per second; and the cost of maintenance, exclusive of large repairs, may vary from 20*s.* to 34*s.* for distributing channels situated in conditions similar to those of the Verdon canal.

ON THE ADJUSTMENT OF CONDITION-OBSERVATIONS IN THE METHOD OF LEAST SQUARES, WITH ITS APPLICATION TO GEODETIC WORK, AND WITH SPECIAL REFERENCE TO AMERICAN METHODS OF THE ADJUSTMENT OF A SYSTEM OF TRIANGULATION.

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Late Assistant Engineer U. S. Lake Survey.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

40. *The Side-Equations.*

In a single triangle, or in a simple chain of triangles, we have seen that the length of any assigned side can be computed from a given side in but one way. The angle-equation is the only condition-equation. When the triangles are interlaced this is not so.

Thus in the quadrilateral ABCD, all four stations being occupied, the three equations given by the triangles DAB, ABC, BCD, that is:

$$D_1AB + ABD_1 + BD_1A = 180 + \varepsilon_1$$
$$ABC + BCA + CAB = 180 + \varepsilon_2$$
$$BCD_2 + CD_2B + D_2BC = 180 + \varepsilon_3$$

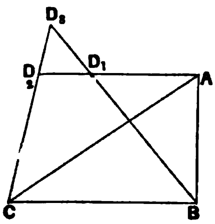
may be satisfied, and yet the figure not be a perfect quadrilateral. The further condition that the lines CD_2D_1, BD_1D_2, AD_1D_2 intersect in the same point, that is, that the computed values of the lines BD_1, BD_2 are equal, must be added.

Proceeding from a base (as AB) we may compute BD_1 directly from the triangle

D_1AB and BD_2 through the triangles ABC, BCD_2. This gives us

$$\frac{\sin.BD_1}{\sin.BA}=\frac{\sin.BAD_1}{\sin.AD_1B},$$

$$\frac{\sin.BD_2}{\sin.BA}=\frac{\sin.DCB}{\sin.ACB}\cdot\frac{\sin.BAC}{\sin.BDC}.$$



But BD_1 must equal BD_2.

Hence the condition

$$\frac{\sin.BAD}{\sin.ADB}\cdot\frac{\sin.ACB}{\sin.BAC}\cdot\frac{\sin.BDC}{\sin.DCB}=1.$$

This condition-equation is called a side-equation or sine-equation, and the vertex B is called the *pole* of the quadrilateral for this equation.

41. The side-equation, deduced for spherical triangles, must hold in most cases that occur in practice for the corresponding plane triangles, the angles of each triangle being adjusted according to Legendre's theorem. We have then

$$\frac{BD_1}{BA}=\frac{\sin.(BAD_1-\varepsilon_1)}{\sin.(BD_1A-\varepsilon_1)},$$

$$\frac{BD_2}{BA}=\frac{\sin.(BCD-\varepsilon_3)}{\sin.(BDC-\varepsilon_3)}\cdot\frac{\sin.(BAC-\varepsilon_2)}{\sin.(BCA-\varepsilon_2)}$$

But $\qquad BD_1=BD_2,$
and therefore

$$\frac{\sin.(BAD-\varepsilon_1)}{\sin.(BDA-\varepsilon_1)}\cdot\frac{\sin.(ACB-\varepsilon_2)}{\sin.(BAC-\varepsilon_2)}\cdot\frac{\sin.(BDC-\varepsilon_3)}{\sin.(ACD-\varepsilon_3)}=1$$

where ε_1, ε_2, ε_3, denote one third of the spherical excesses of the triangles BAD, BAC, and BCD, respectively.

As the two forms lead to the same result it affords a check of the accuracy of the work to compute the side-equation in both ways. It is evidently more simple to use the spherical angles than the plane angles.

42. In a spherical triangle, the sines of the angles being proportional to the sines of the opposite sides, the side-equation

$$\frac{\sin.BAD}{\sin.BDA}\cdot\frac{\sin.BCA}{\sin.BAC}\cdot\frac{\sin.BDC}{\sin.BCD}=1$$

if satisfied, gives the identical relation

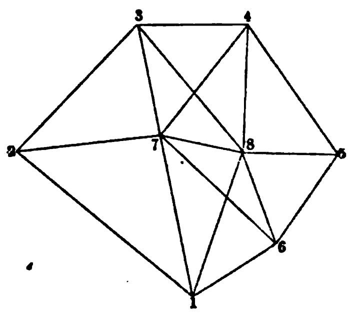
$$\frac{\sin.BD}{\sin.BA}\cdot\frac{\sin.BA}{\sin.BC}\cdot\frac{\sin.BC}{\sin.BD}=1.$$

Hence in writing down a side-equation having chosen the pole (B for example) it is convenient to write down first the scheme

$$\frac{BD}{BA}\cdot\frac{BA}{BC}\cdot\frac{BC}{BD}=1,$$

formed by the lines radiating from the point B and then the sine-equation follows at once.

43. If the triangulation-net instead of involving quadrilaterals only involves central polygons such that we can pass from one side to any other through a chain of triangles, the same process is followed in forming the side-equations as in a



quadrilateral. Thus in the figure which represents part of the triangulation of Lake Erie west of Buffalo Base, there are from side-equations,

the quadrilaterals, 7348, 1786,
the pentagons, 71238, 87456.

The scheme for the pentagonal side-equation 71238 for example would be just as in the case of a central quadrilateral, taking 7 as pole,

$$\frac{71}{72}\cdot\frac{72}{73}\cdot\frac{73}{78}\cdot\frac{78}{71}=1,$$

and hence

$$\frac{\sin.721}{\sin.712}\cdot\frac{\sin.732}{\sin.723}\cdot\frac{\sin.783}{\sin.738}\cdot\frac{\sin.718}{\sin.781}=1.$$

44. Thus far we have considered the side-equations in their rigorous form. But in order to treat them by the method of least squares they must first be reduced to the linear form which may be done as follows:

Let the side-equation be

$$\frac{\sin.A_1.\sin.A_3\ldots}{\sin.A_2.\sin.A_4\ldots}=1,$$

where $A_1, A_2\ldots$ denote the most probable values of the angles. Let $M_1, M_2,\ldots$ denote the measured values and $v_1, v_2, v_3\ldots$ the most probable corrections to these values, which corrections are assumed to be small. Then the equation may be written

$$\frac{\sin.(M_1+v_1).\sin.(M_3+v_3)\ldots}{\sin.(M_2+v_2).\sin.(M_4+v_4)\ldots}=1.$$

Taking the log. of each side of this equation and expanding by Taylor's theorem we have, taking the first powers of the corrections only,

$$\log.\sin.M_1+\frac{d}{dM_1}(\log.\sin.M_1)v_1-$$

$$\left\{\log.\sin.M_2+\frac{d}{dM_2}(\log.\sin.M_2)v_2\right\}$$

$$+\ldots=0 \quad (81).$$

which may be written in two forms for computation.

First if the corrections to the angles are expressed in seconds we may put

$$\frac{d}{dM_1}(\log.\sin.M_1)=\delta',$$

when δ' is the tabular difference for $1''$ for the angle M_1 in a table of log. sines. Then we have

$$\delta'v_1-\delta''v_2+\ldots+\log.\sin.M_1-\log.\sin.M_2+\ldots=0$$

that is $\qquad [\delta v]=l \qquad (82).$

where l is a known quantity.

Secondly and specially for small angles and angles near 180° we may replace

$\frac{d}{dM_1}(\log.\sin.M_1)$ by (mod.) sin. $1''$ cot. M_1

when (mod.) denotes the modulus of the common system of logarithms. Equation (81) may then be arranged

$$\cot.M_1.v_1-\cot.M_2.v_2+\ldots$$

$$\frac{1}{(\text{mod.})\sin.1''}(\log.\sin.M_2-\log.\sin.M_1+\ldots)$$

$$\log.\frac{1}{10^7(\text{mod.}).\sin.1''}=8.67664-10.$$

For convenience of computation there is not much to choose between the two forms. The second is perhaps on the whole to be preferred.

Example.—The quadrilateral N. Base, S. Base, Lester, Oneota.

Take the pole at Lester.

We have the scheme

$$\frac{LS}{LN}\cdot\frac{LN}{LO}\cdot\frac{LO}{LS}=1,$$

and the sine-equation.

$$\frac{\sin.(M_2+v_2)}{\sin.(M_5+v_5)}\cdot\frac{\sin.(M_7+v_7)}{\sin.(M_1+v_1)}\cdot$$

$$\frac{\sin.(M_6+v_6)}{\sin.(M_7+M_8+v_7+v_8}=1.$$

First form,

° ′ ″	log. sin.
118 39 05.07 + v_2	= 9.9618969.7 − 9.22 v_2
43 46 26.40 + v_7	= 9.8399908.7 + 21.98 v_7
70 39 24.60 + v_6	= 9.9747656.9 + 7.39 v_6
	580.0
47 31 20.41 + v_5	= 9.8677859.5 + 19.28 v_5
124 09 40.69 + v_1	= 9.9177470.2 − 14.29 v_1
78 27 06.06 + v_7 + v_8	= 9.9911180.3 + 4.30(v_7 + v_8)
	510.0
	+20.0

Check by deducting $\frac{1}{3}$ of the spherical excess of the triangle to which it belongs.

Angle. ° ′ ″	log. sin.	° ′ ″	
118 39 05.00	9.9618970.3	47 31 20.34	9.8677858.2
43 46 26.36	9.8399902.5	124 09 40.65	9.9177470.7
70 39 24 48	9.9747656.0	78 27 05 93	9.9911179.8
	528.8		508.7
		+20.1	

agreeing closely with the value found from the spherical angles. Hence the side-equation is

$$14.29v_1-9.22v_2-19.28v_5+7.39v_6+17.68v_7-4.30v_8+20.0=0$$

or dividing by 10 which is approximately the mean of the coefficients and which amounts to the same thing as expressing the log. differences in units of the sixth place of decimals,

$$1.43v_1 - 0.92v_2 - 1.93v_5 + 0.74v_6 + 1.77v_7 - 0.43v_8 + 2.00 = 0.$$

Second form.

log. sine.	cot.	log. sine.	cot.
9.9618969.7	$-0.4380v_2$	9.8677859.5	$+0.9156v_5$
9.8399903.4	$+1.0437v_7$	9.9177470.2	$-0.6786v_1$
9.9747656.9	$+0.3510v_6$	9.9911180.3	$+0.2043(v_7+v_8$
530.0		510.0	
510.0			
20.0 log. 1.30103			
8.67664			
9.97767		0.9499	

and the side-equation is

$$+0.6786v_1 - 0.4380v_2 - 0.9156v_5 + 0.3510v_6 + 0.8394v_7 - 0.2043v_8 + 0.9499 = 0$$

Each term of this equation is of course in a constant ratio to the corresponding term of the previous form.

It will be noticed that the log. sines have been carried out to eight places of logs. only. On the Coast Survey and Lake Survey the practice is to carry out the log. sines to ten places of decimals, but on some of the leading European Surveys, such as the Prussian, Danish and Spanish, the log. sines are carried out to only eight places. On the English Ordnance Survey they were carried out to ten places, and on the Great Trigonometrical Survey of India to seven places.

On the Coast Survey the log. differences (δ) for 1″ are computed in units of the fifth place of decimals, on the Lake Survey in units of the seventh decimal place, and then the side-equation was divided by a such a number as would make the average of the coefficients equal unity approximately. By others the sixth place of decimals is taken as the unit for the final values of the coefficients. This I should in general prefer as being most convenient. Or still better, since in a good system of triangulation the angles in the side-equations should range between 30° and 60° and the difference for log. sin. 45° is 21, we may choose 20 as on the whole the best of any single divisor for differences expressed in units of the seventh place of decimals.

The principle involved in all of the preceding is that since the coefficients of the corrections in the angle equations are $+1$ or -1, and these are absolute, the greatest possible equality of coefficients throughout the condition equations is best in order to secure an equable distribution of errors arising from imperfect computation.

A striking difference between condition equations and observation-equations is here brought out. A condition-equation expressing a rigorous relation among the quantities independent of the observations may be multiplied or divided by any number without affecting that relation: with an observation-equation, on the other hand, the effect would be to increase or diminish its weight.

The coefficients of the corrections are usually carried out one place further than the absolute term in a side equation. This for a short computation would be unnecessary, but in a long one, on account of cutting off the last figures in products and quotients all through, errors accumulate so that we must carry with us more decimal places than we finally wish to keep.

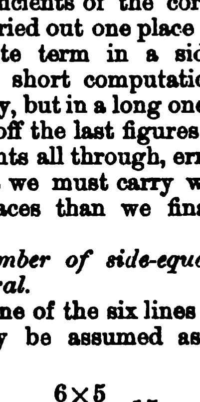
45.—*Number of side-equations in a quadrilateral.*

As any one of the six lines in a quadrilateral may be assumed as base there may be *

$$\frac{6\times5}{1\times2}=15$$

side-equations.

Thus in the quadrilateral ABCD considering AB as base we have for comput-



ing AC, AD, or taking AC as base and computing AD the same equations,

$$\frac{\sin.AC}{\sin.AB}=\frac{\sin.(3+4)}{\sin.5},\ \frac{\sin.AD}{\sin.AC}=\frac{\sin.6}{\sin.(7+8)},\ \frac{\sin.AB}{\sin.AD}=\frac{\sin.8}{\sin.3},$$

and $\therefore$

$$\frac{\sin.(3+4)\sin.6.\sin.8}{\sin.5.\sin.(7+8)\sin.3}=1.$$

* See Zachariæ geodätische Hauptpunkte. Zeitschr. für Vermess. Vol. IX.

This form with A as pole which results from the scheme

$$\frac{AC}{AB} \cdot \frac{AD}{AC} \cdot \frac{AB}{AD} = 1,$$

we shall call (A).

The equations corresponding to the poles B, C, D, are:

$$\frac{\sin.(5+6)}{\sin. 7} \cdot \frac{\sin. 8}{\sin.(1+2)} \cdot \frac{\sin. 2}{\sin. 5} = 1 \qquad (B)$$

$$\frac{\sin.(7+8)}{\sin. 7} \cdot \frac{\sin. 2}{\sin.(3+4)} \cdot \frac{\sin. 4}{\sin. 7} = 1 \qquad (C)$$

$$\frac{\sin.(1+2)}{\sin. 3} \cdot \frac{\sin. 4}{\sin.(5+6)} \cdot \frac{\sin. 6}{\sin. 1} = 1 \qquad (D)$$

Since $(A)\times(C)=(B)\times(D)$ there are only three *independent* forms resulting from the first twelve forms, though there are four that are *different*.

Again computing BC from AD through the diagonals, the side-equation is

$$\frac{\sin. 2}{\sin. 6} \cdot \frac{\sin. 3}{\sin. 7} \cdot \frac{\sin.(5+6)}{\sin.(3+4)} \cdot \frac{\sin.(7+8)}{\sin.(1+2)} = 1$$

which is equivalent to

$$(B)\div(A) \text{ or } (C)\div(D)$$

and it gives no new relation.

Computing CD from AB through the diagonals the equation is

$$\frac{\sin. 1}{\sin. 4} \cdot \frac{\sin. 8}{\sin. 5} \cdot \frac{\sin. 3+4}{\sin. 1+2} \cdot \frac{\sin. 5+6}{\sin. 7+8} = 1$$

which is equivalent to

$$(A)\div(D) \text{ or to } (B)+(C).$$

Computing the side BC from AD or CD from AB though the sides gives the equation

$$\frac{\sin. 1}{\sin. 2} \cdot \frac{\sin. 3}{\sin. 4} \cdot \frac{\sin. 5}{\sin. 6} \cdot \frac{\sin. 7}{\sin. 8} = 1$$

which is equivalent to

$$(A)\times(C) \text{ or } (B)\times(D)$$

The seven forms then expressed in terms of the first three are

$$(A),\ (B),\ (C),\ \frac{(A)\ (C)}{(B)},\ (A).(C),\ \frac{(B)}{(C)},\ \frac{(B)}{(A)}.$$

We have thus seven different forms, though only three that are independent out of the fifteen.

If we take the angle-equations into account, a little consideration will show that these three forms are really one.

For equation (A) means that the points D_1, D_2 coincide. (See Fig. Art. 40.) Equation (B) that D_1, D_3 coincide, and equation (C) that D_2, D_3 coincide. If any one of these conditions is satisfied so are all the others, as all amount to the same condition, that D is not three points, but one point.

The result is that a quadrilateral is determined by three angle-equations and one side-equation. This may be expressed in 56 ways, as each of the 8 sets of angle equations may be combined with each of the 7 side-equations.

46. Taking the seven different forms of the side-equation in a quadrilateral, the conclusion was reached in (45) that any one of these forms was as good as any other in satisfying the conditions imposed. But now that the side equations are no longer rigorous, but only approximate, the question may be asked, Is not some one of the seven to be preferred? This we proceed to examine.

Now we have assumed in reducing the side-equations to the linear form, that the angles in these equations are such that terms of the expansions of the log. sines of these angles involving powers of the corrections above the first, may be neglected. This places the linear equations on the same footing as the rigorous equations.

On the other hand, we must consider that the corrections arising from a side-equation are distributed as the cotangents of the angles entering it. If only the five angles necessary for forming a side-equa- were measured, there would be no angle equations, and the corrections arising from the side-equation would be the more irregularly distributed the more irregular the size of the angles. When all the angles are measured, the modifying influence of the angle-equations is such as to balance this irregularity. Hence on this account the importance of measuring all the angles necessary for forming the complete series of condition-equations.

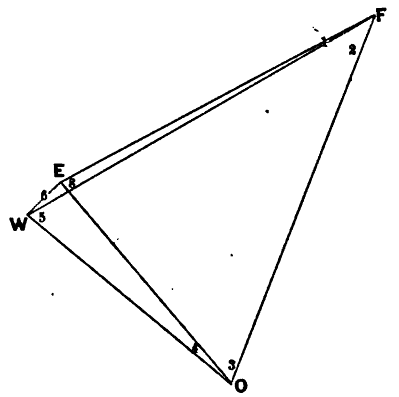
In a central system involving pentagons, hexagons, &c., by taking the pole at the center, the resulting side-equations follow the law of the angle equations nearly. When well hinged together this system is to be preferred, but is much more costly. In the United States the system of quadrilaterals has been followed.

In fact, since a side-equation expresses

but the computation of one side from another by different routes, and we know that the best-shaped triangles should be chosen, it follows that in laying out the work, the angles formed by the diagonals should be taken into consideration so that all small angles be avoided.

There are, however, so many practical difficulties in the way that theoretical considerations of this kind have often to be set aside in part at least.

47. The danger of introducing very small angles in a side-equation may be illustrated by the quadrilateral Farquhar, Outer Island, W. Sawteeth, E. Sawteeth in the triangulation of Lake Superior.



Given, the angles all of the same weight *

	°	′	″	
EFW	0	19	12.80	(1)
WFO	37	41	15.42	(2)
FOE	77	48	12.97	(3)
EOW	2	12	20.89	(4)
OWF	62	18	23.662	(5)
FWE	12	52	20.308	(6)
OEF	64	11	34.564	(8)

The spherical excesses of the triangles FWO, FEO are 15″.845, 15″.577 respectively. The angle-equations are

$$v_2+v_3+v_4+v_5=2.903$$
$$v_1+v_2+v_3+v_8=0.177$$

Taking the pole at O the side-equation is

$$-2.694\,v_1+0.031\,v_2-0.471v_4-1.019v_5+0.086\,v_6+1.018\,v_8=-2.2502.$$

* Report of Chief of Engineers, 1873.

Taking the pole at E the side-equation is

$$+5.343\,v_1-0.038\,v_2+0{,}006\,v_3-0.781v_4\,0.008\,v_5+0.124\,v_6=-1.2533.$$

The corrected angles resulting from the two solutions are

°	′	″	°	′	″
0	19	12.718	0	19	12.557
37	41	15.768	37	41	15.805
77	48	13.325	77	48	13.359
2	12	21.937	2	12	21.920
62	18	24.815	62	18	24.760
12	52	20.291	12	52	20.297
64	11	33.766	64	11	33.855

Taking the side OW as base (=1) let us compute the sides from the different triangles possible, and from the values found by the two adjustments in succession.

Triangle.	Angle, adjusted. ° ′ ″	Angle, plane. ″	log. sin.	log. side.
EOW				
	102 36 53.381	53.240	9.9893877.1	0.
	2 12 21.937	21.796	8.5853862.8	8.5959985.7
	75 10 45.106	44.964	9.9853053.7	9.9959176.6
EOF				
	88 00 28.486	23.294	9.7894047.5	9.9959176.6
	64 11 33.766	28.573	9.9543643.1	0.1608772.2
	77 48 13.325	08.133	9.9900831.0	0.1965960.1
Second Route.				
OWF				
	87 41 15.768	10.486	9.7862807.4	0.
	62 18 24.815	19.534	9.9471579.8	0 1608772.4
	80 00 35.262	29.980	9.9933625.8	0.2070818.4
EFW				
	166 48 27.147	27.095	9.3583593.7	0.2070818.4
	0 19 12.718	12.666	7.7472760.9	8.5959985.6
	12 52 20.291	20.239	9.3478735.5	0.1965960.2

The agreement of the sides is here very satisfactory.

From the values of the angles found by taking the pole at E, we find passing through the same triangles.

Triangle.	Angle, adjusted. ° ′ ″	Angle, plane. ″	log. sin.	log. side.
EOW				
	102 36 53.447	53.306	9.9893876.7	0.
	2 12 21.920	21.779	8.5853853.5	8.5959976.8
	75 10 45.057	44.915	9.9853053.5	9.9959176.8
EOF				
	38 00 28.362	23.170	9.7894044.1	9.9959176.8
	64 11 33.855	28.663	9.9543644.0	0.1608776.7
	77 48 13.359	08.167	9.9900831.2	0.1965963.9
Second Route.				
OWF				
	87 41 15.805	10.524	9 7862808.5	0.
	62 18 24 760	19.478	9.9471579.2	0.1608770.7
	80 00 35.279	29.998	9 9933625.9	0.2070817.4
EFW				
	166 48 27.302	27.250	9 3583579.8	0.2070817.4
	0 19 12.557	12.505	7.7472154.8	8.5959391.9
	12 51 20.297	20.245	9.3478736.1	0.1965973.7

The agreement here especially for the side EW is quite unsatisfactory.

48. We are now in a position to estimate the *number of independent side-equations in any triangulation-net.*

The base line being measured its extremities are known. To fix a third point two directions must be measured; in other words, we must know the other two sides of the triangle of which this point is the vertex. Each superfluous line will therefore give a side-equation. Hence, if we have a net of triangles connecting s stations, two of the stations being the ends of the base, we must have in order to plot the figure $2(s-2)$ lines besides the base. If, then, the total number of lines in the figure is l, the number of superfluous ones must be

$$l-[2(s-2)+1]$$

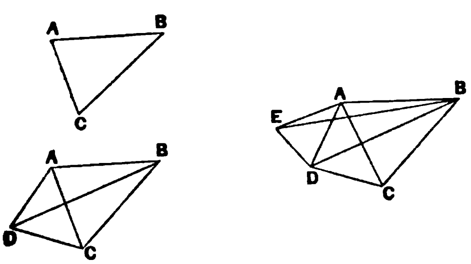
giving in all

$$l-2s+3 \tag{83}$$

side-equations.

50. ***How to pick out the angle and side-equations.***

In the selection of the side and angle-equations in a triangulation-net we have two dangers to guard against: first, that we omit no necessary conditions, and second, that we admit no unnecessary ones. The rule usually followed (due to Bessel) is to start from some line, usually a base-line, and add point to point, writing down the conditions that express the connections of each point to the system as the system grows.



For example, take AB as base. A third point C, all the angles being measured gives an angle-equation from the triangle ABC.

A fourth point D gives in addition

Angle-equations from ABD, ACD,
Side-equation from ABCD.

A fifth point E gives in addition

Angle-equations from ABE, AED.
Side-equation from ABED,

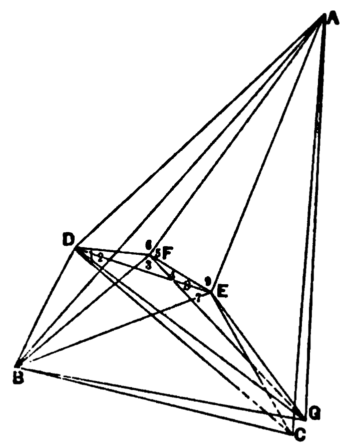
and so on.

We have then in one complete figure five angle-equations and two side-equations.

Check—No. of stations $= 5$.
No. of lines $= 9$.

$\therefore$ No. of angle-equations $=9-5+1=5$
and No. of side-equations $=9-10+3=2$.

51. As an illustration of the difficulties which may arise in solving a triangulation-net, I shall take the triangulation around the Chicago Base. It is represented in the figure.



The peculiarity of this system is that the station F is nearly on the line DE. Thus the triangle equation DEF

$$\begin{array}{lrrl}
<\text{EDF}= & 00 & 00 & 00.976+v_1 \\
<\text{FED}= & 00 & 00 & 1.250+v_2 \\
<\text{DFE}= & 179 & 59 & 56.897+v_3 \\
 & 179 & 59 & 59.123+v_1+v_2+v_3 \\
 & 180 & 00 & 00.000 \\
 & & & 0=-0.877+v_1+v_2+v_3.
\end{array}$$

From the rules given above it follows that there are ten angle equations and eight side-equations in the system.

In the selection of the side-equations it is advisable to avoid those quadrilatera lsentangled with the above triangle. They are ADEF, BDEF, GDEF.

For example, if we take the quadrilateral GDEF we have in units of the seventh place of decimals, pole at G,

$$54.7729v_1 - 0.0008v_2 + 0.0015v_4 - 40.7066(v_5 + v_6) + 26.1803v_7 - 0.0003(v_8 + v_9) - 40.180 = 0.$$

Now, since the co-efficients of v_2, v_4, v_8, v_9, are each less than 2 in the tenth place of decimals, the above equation is nearly the same as

$$5.4773v_1 - 4.0707(v_5 + v_6) + 2.6180v_7 - 4.0130 = 0.$$

or dividing by 4 and replacing $v_5 + v_6$ by $-v_3$, which we can do since the sum of the angles at F is 360.

$$1.3693v_1 + 1.0177v_3 + 0.6545v_7 - 1.0032 = 0$$

which is nearly the same as the above angle-equation, and therefore can scarcely be said to be a side-equation at all.

Similarly the quadrilaterals ADEF, BDFF, give respectively, neglecting coefficients less than 5 in the fifth place of decimals.

$$1.2191v_1 + 0.2085v_7 + 0.7220v_3 - 0.7899 = 0$$
$$0.2560v_1 + 2.4665v_7 + 1.3434v_3 - 0.8355 = 0$$

both of which merely express approximately the same relations among the angles of the small triangle DEF as the angle-equation formed from that triangle. We therefore conclude that in the formation of the angle and side-equations the triangle DEF should be avoided.

52. There is another trouble caused by the introduction of relatively small angles in side-equations, and where consequently the coefficients of some of the unknowns greatly exceed in size some of the others. It is this: in the normal equations the side terms may be as large as, and even larger, than the diagonal term. In our example above, some of the side terms would exceed the diagonal terms if worked with the side-equations mentioned. As the diagonal terms are the divisors in the Gaussian method of solution of the normal equations, they ought to be larger than the terms into which they are divided, to obtain a closely accurate result, especially if any approximate method of elimination is employed. Of course the solution can be carried through, but the number of decimal places to which it must be carried to make the values obtained of any worth, causes a great amount of labor which might have been saved.

Thus the terms of the correlate normal equations found with the side-equations of Art. 51 are as follows, in the neighborhood of the diagonal term:

$$\begin{array}{l} 7.44 + 7.12 - 9.33 + \ldots\ldots.. \\ 7.12 + 28.62 - 12.70 + \ldots\ldots.. \\ -9.33 - 12.70 + 12.34 + \ldots\ldots.. \\ \ldots\ldots\ldots\ldots\ldots\ldots \end{array}$$

From the equations no exact values could be found, without an excessive amount of computation.

53. Again in selecting the side-equations in a net, care must be taken that only independent conditions are chosen. Thus we might have chosen for the eight quadrilaterals the following:

AGBD, AGDE, AGBE, BDFG, BFEG, AGDF, ACBE, BDEG.

But a careful examination will show that these are not independent. For taking the four AGBD, AGDE, AGBE, BDEG, and choosing G as pole, which is common to them all, we have

$$1 = \frac{\sin. ADG}{\sin. DAG} \cdot \frac{\sin. DBG}{\sin. BDG} \cdot \frac{\sin. BAG}{\sin. ABG}$$

$$1 = \frac{\sin. DAG}{\sin. ADG} \cdot \frac{\sin. EDG}{\sin. DEG} \cdot \frac{\sin. AEG}{\sin. EAG}$$

$$1 = \frac{\sin. ABG}{\sin. BAG} \cdot \frac{\sin. BEG}{\sin. EBG} \cdot \frac{\sin. EAG}{\sin. AEG}$$

$$1 = \frac{\sin. BDG}{\sin. DBG} \cdot \frac{\sin. DEG}{\sin. EDG} \cdot \frac{\sin. EBG}{\sin. BEG}$$

which equations multiplied together reduce to the identical form

$$1 = 1,$$

showing that from any three the fourth may be found.

If the rule given in (50) is followed this repetition of conditions will hardly occur.

The entrance of mutually dependent conditions would, however, be detected in the course of the elimination as we should arrive at two identical equations, or in other words one of the correlates would become indeterminate. If this should happen in the course of a solution we should at once conclude that the trouble was in the condition-equations and proceed to re-examine them.

54. We next pass on to the *arrangement* of the condition-equations in the net adjustment. On first thoughts it might seem that it would be well to arrange the angle-equations and side-equations in two separate sets and so carry

them forward for solution. This was done in some of the older work. The only objection to doing it is that the process of finding the corrections is more troublesome. Experience shows that the solution of a series of normal equations is much facilitated if the coefficients are arranged as the steps of a stair rather than irregularly. Thus,

$$\begin{array}{rl} [aa]x+[ab]y & =[al] \\ [bl]y+[bc]z & =[bl] \\ [bc]y+[cc]z & =[cl] \end{array} \qquad 1$$

is a more convenient form for solution than

$$\begin{array}{rl} [aa]x+\qquad [ab]y & =[al] \\ [bc]z+[bc]y & =[bl] \\ [ab]x+[bc]z+[bb]y & =[cl] \end{array} \qquad 2$$

The condition-equations should, therefore, be so arranged that as far as possible (and it cannot always be done in the first trial), the normal-equations will fall in form 1 rather than in form 2. A good rule is to begin with angle-equations, proceeding from triangle to triangle, until the points gone over are covered by a side-equation, and then introduce it. Repeat the process for the remaining sets of triangles.

As to the angle-equations themselves, it matters but little which triangles are taken to form them. It is better, however, to avoid triangles with very small angles, such as DEF in fig. Art. 51, and where angles so very small occur, to avoid triangles involving angles immediately contiguous to these small angles.

55. We may mention another thing which, though it causes no trouble in the solution, is a little perplexing, because unexpected. It is well known that the diagonal terms are always positive. Now in the formation of the normal equations from the correlate equations some of the terms that go to make up the diagonal terms may be negative. These terms are comparatively small and arise generally from angles in the neighborhood of 90°, especially if a little over 90°. It is a good plan, therefore, to avoid angles of 90°* in the formation of quadrilateral side-equations, the change in the log. sine being smallest at that point.

56. In the example chosen the selection of side and angle-equations may be made as follows, AB being taken as base:

1. Triangle AGB
2. " ABE
3. " BEG

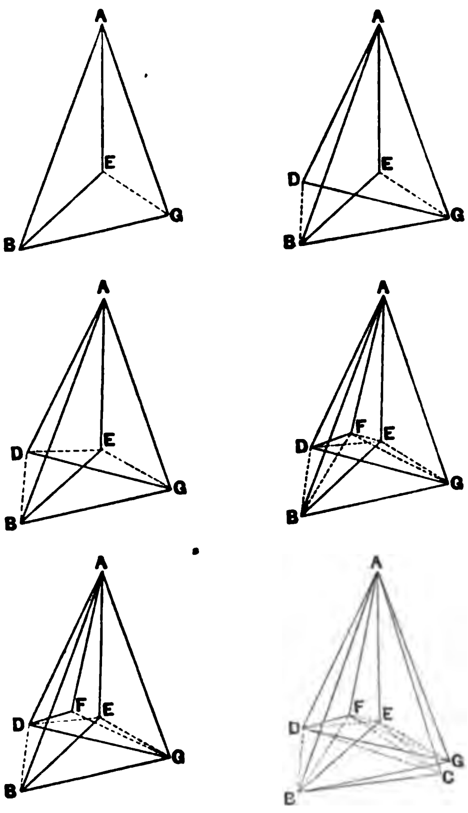
I. Quadrilateral AGBE

4. " DAG
5. " BDG

II. " AGBD

6. " DAE

(AEG=ABG−ABE−BEG)



III. Quadrilateral AGDE

7. Triangle DAF

IV. " AGDF

8. " BDF

V. " BDFG

9. " BFE

VI. " BFGE

10. " BAC

VII. " ACBE

VIII. " CBDE

Thus we have the ten angle-equations and the eight side-equations.

* See C. S. Report, 1864.

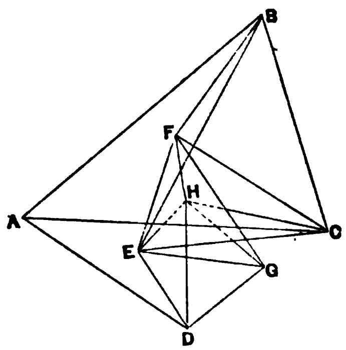
57. In the explanation of the formation of the side-equations we have assumed that the route from one side to the same side again in the triangulation-net furnishing a side-equation proceeds through a chain of triangles. This, however, is not always the case. A good illustration occurs in the triangulation of Lake Superior,* near Keweenaw Base, as shown in figure.

Here there are 18 lines and 8 points, requiring 5 side-equations. Proceeding from the line DG, and adding point to point, we have for the first four side-equations the quadrilaterals

HGDE, HGEF, HEFC, CEFB.

With these there is no difficulty.

The fifth side-equation is furnished by the pentagon ABCED. We cannot com-



pute directly any specified side of this pentagon from another side through triangles only. A little artifice, however, will enable us to do this. Suppose the line CD drawn. Call the angle $GDC=x$ and $ECD=y$. This new line CD gives an additional side-equation. We take the two pentagons ABCED, DEGFC. From the first pentagon ABCED

$$\frac{CE}{CD}\cdot\frac{CD}{CA}\cdot\frac{CA}{CB}\cdot\frac{CB}{CE}\cdot=1$$

or

$$\frac{\sin. EGD+x}{\sin. CDE}\cdot\frac{\sin. CAD}{\sin. ADG}\cdot\frac{\sin. CBA}{\sin. CAB}\cdot\frac{\sin. CEB}{\sin. CBE}\cdot=1$$

and from the second pentagon DEGFC

$$\frac{ED}{EG}\cdot\frac{EG}{EF}\cdot\frac{EF}{EC}\cdot\frac{EC}{ED}\cdot=1$$

or

$$\frac{\sin. EGD}{\sin. EDG}\cdot\frac{\sin. EFG}{\sin. EGF}\cdot\frac{\sin. (ECF+x)}{\sin. EFC}\cdot\frac{\sin. (EDG+x)}{\sin. y}=1 \qquad (84)$$

Now y can be expressed in terms of x from the triangle CDE. Thus

$$y+CED+EDG+x=180+\varepsilon \qquad (85)$$

Eliminating x from equations (84) and (85) the required side-equation results.

Now equation (84) is easily put in the form

$$\text{Cot. }(EDG+x)=\frac{1}{\sin. ADE}\cdot\frac{\sin. CAD}{\sin. CED}\cdot\frac{\sin. ABC}{\sin. BAC}\cdot\frac{\sin. BEC}{\sin. CBE}-\text{Cot. }ADE$$

	°	′	″	log. sin.
CAD=	24	28	40.92	9.6173614
ABC =	49	14	53.54	9.8794082
BEC =	64	40	04.26	9.9596005
				9.4563701
				9.3290804
				0.1272897
log. cot. ADE				0.1807050
				0.0534153

	°	′	″	log. sin.
ADE=	33	24	34.97	9.7408538
CED=	61	18	05.11	9.9430779
BAC =	53	16	36.98	9.9039225
CBE =	33	26	31.73	9.7412262
				9.3290804

Hence from Zech's Tables* it follows at once that

$$\text{log. cot. }(EDG+x)=9.2441624n$$

and $$EDG+x=99°\ 57''\ 04''.47$$

Hence since $\varepsilon=1''.08$ we easily find

$$3y=18°\ 44'\ 51''.49.$$

All the angles of equation (85) being known the solution is carried through in the usual way.

Since a preliminary computation of the lengths of the sides for finding the spherical excess must be made, the angle x could be found roughly from the relation

$$\frac{EC}{\sin. (EDG+x)}=\frac{ED}{\sin. (180-EDG-x-CED-\varepsilon)},$$

* Report of Chief of Engineers, 1872.

* Tafeln der Addition and Subtraction Logarithmen. Von J. Zech.

where ε is the spherical excess of the triangle.

If chains of triangles intersect so as to enclose a polygon the preceding method may be applied in simple cases. It is, however, better treated by other methods. (See Zachariæ *Geodätische Hauptpunkte*, sec. 3; Clarke's *Geodesy*, pp. 255 *seq.*; *G. T. Survey of India*, vol. 2.)

58. *Closeness of computation necessary.* As we know that exact values of the unknowns are never found, we may investigate how far it is allowable to cut short our computation. With a single unknown a method of computation is close enough which will give that unknown within $\frac{1}{10}$ of the mean square error μ expected. Taking this as a basis in a set of equations of the form

$$ax + ly + \ . \ . \ . \ - l = v$$

and that is taking the allowable error of l to be $\frac{1}{10}\mu$, if we let σ be the average value of the greatest allowable error of ax, ly, . . then if m is the number of these terms we have

$$\frac{\mu^2}{100} > m\sigma^2$$

Now it will be a liberal estimate if we let $m = 100$, so that the approximation is sufficient when

$$\sigma < \frac{\mu}{100}$$

The quantities ax, by, . . may be taken to be about 5μ numerically. Hence an approximation is sufficient where the error committed does not exceed $\frac{1}{500}$ of the value of the quantity. This can in general be carried out with a computation to 4 places of decimals, and if a log. table is used, a 5-place table is sufficient. In short systems 4-place tables will give results close enough.

[It does not lie within my province to explain the method of forming and solving normal-equations. This is done in the treatises already referred to. I may, however, remark that in the mechanical part of least squares, the day for the exclusive use of log. tables is past. The best aids are a table of squares, a table of reciprocals, a table of products, and a computing machine. No one who has worked with these tools will ever want to open a log. table for forming and solving normal-equations.]

THE LAKE SURVEY METHOD OF ADJUSTMENT.

59. The method of measuring horizontal angles employed on the Lake Survey is a modification of Bessel's method. It is as follows:

The instrument being in position, the signals around the horizon were sighted at in order of increasing graduation of the horizontal limb closing on the first signal and the microscopes read each time: then these pointings were repeated, but in the order of decreasing graduation, closing again on the first object. This gave one set.

The difference between consecutive pointings gave the included angle. The mean of an angle value in the direction of increasing graduation and an immediately following one in the opposite direction gave a combined result.

The telescope was then reversed, leaving the same pivot in the same wyes, and another set taken from which combined results are found.

The horizontal limb is now moved some aliquot part of 180°, and the preceding process repeated till microscope readings have been obtained at equidistant points entirely round the limb.

The number of sets before and after reversal should be the same. Probable errors are derived from combined results.

The resulting angles are assumed to be independent in the adjustment.

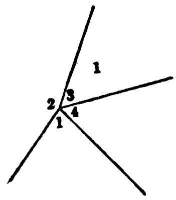
EXAMPLE. AT STATION D.

Stat'n.	Micros. 1. ° ′ ″	Mic. 2. ″	Mic. 3. ″	Mean. ″	Angle. ° ′ ″
	Circle left, positive.				
A...	334 49 21.0	22.2	20.9	21.4	
B...	72 11 54.1	53.3	46.2	51.2	97 22 29.8
C...	250 25 51.8	54.1	55.1	53.7	178 14 02.5
	Circle left, negative.				
C...	51.8	52.9	54.0	52.7	
B...	54.0	53.0	46.0	51.0	01.7
A...	23.5	24.8	23.6	24.0	27.0
	Circle right, positive.				
A...	48.0	40.8	49.0	45.9	
B...	07.0	10.1	13.8	10.3	24.4
C...	14.9	16.0	10.1	13 7	3.4
	Circle right, negative.				
C...	14.3	14 5	9.8	12.9	
B...	7.2	9.7	13.7	10.2	2.7
A...	48.0	41.2	48.7	46.0	24.2

60. *Notation.*

The notation used on the Lake Survey was as follows: The stations were numbered from one end of the triangulation

to be adjusted to the other. The single angles in order of azimuth at each station were denoted by subscript numbers. Thus, at station 1 the angles, if all measured, would be denoted 1_1, 1_2, 1_3, 1_4, starting from the south point and going round by the west.



The local correction to 1_1 would be (1_1) and the general correction $[1_1]$. The measured sum-angle composed of the first two angles 1_1, 1_2 would be denoted by 1_{1+2}.

61. The method of computing the most probable corrections to the angles is almost precisely the same as that already explained in Arts. 19–21, and need not, therefore, be repeated. The example of Art. 34 will serve as an illustration.

Assuming the independence of the measured angles, this method of finding the corrections to them is perfectly rigorous.

EXAMPLE.

The Local Adjustment.

(*a*) At *North Base.*

The observation-equations

p	(1_1)	(1_2)	l
2	+1	+1	0
2		+1	0
14	−1	−1	−1.37

The normal-equations

$$\begin{array}{ll} (1_1) & (1_2) \\ +16 & +14 = -19.18 = [pal]_1 \text{ suppose} \\ +14 & +16 = -19.18 = [pbl]_1 \quad `` \end{array}$$

Solving in general terms

$$(1_1) = +0.267\,[pal]_1 - 0.233[pbl]_1$$
$$(1_2) = -0{,}223\,[pal]_1 + 0.267[pbl]_1$$

Hence

$$(1_1) = -0.64''$$
$$(1_2) = -0.64$$
$$(1_3) = +0.64 + 0.64 - 1.37$$
$$= -0.09$$

and, most probable angles

$$\begin{array}{l} \;\;\circ \quad\; ' \quad\;\; '' \\ 124\ 09\ 40.05 + [1_1] \\ 113\ 39\ 04.43 + [1_2] \\ 122\ 11\ 15.52 - [1_1] - [1_2] \end{array}$$

(*b*) At *South Base.*

The observation-equations

p	2_1	2_2	l
23	+1		
6		+1	
7	+1	+1	−1.07

The normal-equations

$$\begin{array}{lll} (2_1) & (2_2) & \\ 30 & +7 & = -7.49 \\ 7 & +13 & = -7.49 \end{array}$$

Solution in general terms

$$(2_1) = +0.038[pal]_2 - 0.021[pbl]_2$$
$$(2_2) = +0.021[pal]_2 + 0.088[pbl]_2$$

Most probable angles

$$\begin{array}{l} \;\circ \quad\; ' \quad\;\; '' \\ 23\ 08\ 05.13 + [2_1] \\ 47\ 31\ 19.91 + [2_2] \\ 70\ 39\ 25.04 + [2_1] + [2_2] \end{array}$$

The General Adjustment.

The Condition-Equations.

(*a*) Triangle. N. Base. S. Base. Oneota.

$$\begin{array}{rl} & \;\circ \quad\; ' \quad\;\; '' \\ & 122\ 11\ 15.52 - [1_1] - [1_2] \\ & 23\ 08\ 05.13 + [2_1] \\ & 34\ 40\ 39.66 + [3_1] \\ \hline & 180\ 00\ 00.31 \\ 180+\varepsilon & 180\ 00\ 00.06 \\ \hline \end{array}$$

$$0 = 0.25 - [1_1] - [1_2] + [2_1] + [3_1]$$

(*b*) Triangle. Lester, Oneota, S. Base.

$$\begin{array}{rl} & \;\circ \quad\; ' \quad\;\; '' \\ & 70\ 39\ 25.04 + [2_1] + [2_2] \\ & 78\ 27\ 06.06 + [3_1] + [3_2] \\ & 30\ 53\ 30.81 + [4_1] \\ \hline & 180\ 00\ 01.91 \\ 180+\varepsilon = & 180\ 00\ 00.37 \\ \hline \end{array}$$

$$0 = 01.54 + [2_1] + [2_2] + [3_1] + [3_2] + [4_1].$$

(*c*) Quadrilateral. N. Base, S. Base, Lester, Oneota.

$$\frac{\sin.\,LNS}{\sin.\,LNO} \cdot \frac{\sin.\,LOS}{\sin.\,NSL} \cdot \frac{\sin.\,LON}{\sin.\,LOS} = 1$$

° ′ ″	
113 39 04.43	$+[1_2]$
70 39 25.03	$+[2_1]+[2_2]$
43 46 26.40	$+[3_2]$

log. sin.	diff. 1.″
9.9618975.6	− 9.22
9.9747660.1	+ 7.39
9.8399903.4	+21.98

539.1
509.4

29.7

° ′ ″	
124 09 40.05	$+[1_1]$
47 31 19.90	$+[2_2]$
78 27 06.06	$+[3_1]+[3_2]$

log. sin.	diff. 1.″
9.9177479.3	−14.29
9.8677849.8	+19.28
9.9911180.3	+ 4.30

509.4

A glance at the log. differences for 1″ shows that by expressing them in units of the sixth place of decimals their average value is unity. We have then the side-equation

$$1.43[1_1]-0.92[1_2]+0.74[2_1]-1.19[2_2]$$
$$-0.43[3_1]+1.77[3_2]+2.97=0$$

The weight equations.

$$\begin{aligned}
[1_1]&=-0.233[pal]_1+0.267[pbl]_1\\
[1_2]&=+0.267[pal]_1-\ \ 233[pbl]_1\\
[2_1]&=\quad+0.038[pal]_2-0.021[pbl]_2\\
[2_2]&=\quad-0.021[pal]_2+0.088[pbl]_2\\
[3_1]&=\qquad\qquad+1.000[pal]_3\\
[3_2]&=\qquad\qquad+0.032[pal]_4\\
[4_1]&=\qquad\qquad+0.125[pal]_5
\end{aligned}$$

The correlate-equations.

	I.	II.	III.
$[pal]_1=$	−1	0	+1.43
$[pbl]_1=$	−1	0	−0.92
$[pal]_2=$	+1	+1	+0·74
$[pbl]_2=$	0	+1	−1.19
$[pal]_3=$	0	+1	+1.77
$[pal]_4=$	0	+1	−0.43
$[pal]_5=$	0	+1	0.

The corrections in terms of the correlates

	I.	II.	III.
$[1_1]=$	−0.034	0	+0.596
$[1_2]=$	−0.034	0	−0.569
$[2_1]=$	+0.038	+0.017	+0.052
$[2_2]=$	−0.021	+ 067	−0.121
$[3_1]=$	0.	+1.	−0.014
$[3_2]=$	+0.032	+0.032	+1.770
$[4_1]=$	0.	+0.125	0.

The normal-equations

I.	II.	III.		
+0.138	+0.049	+0.021	=	−0.250
	+1.241	+1.687	=	−1.540
		+4.706	=	−2.970

The solution of these equations gives

$$\text{I.}=-1.522$$
$$\text{II.}=-0.647$$
$$\text{III.}=-0.392$$

Substitute for I., II., III., in the correlate-equations we have the general corrections. Adding the station corrections and general corrections together, the total corrections to the measured angles are:

	(v)	$[v]$	v	p	pvv	Final Angles.
	″	″	″			° ′ ″
1_1	=−0.64	−0.18	=−0.82	2	1.34	124 09 39.87
1_2	=−0.64	+0.28	=−0.36	2	.26	113 39 4.71
1_3	=−0.09	−0.10	=−0.19	14	.50	122 11 15.42
2_1	=−0.13	−0. 9	=−0.22	28	1.10	23 08 5.04
2_2	=+0.04	−0.50	=−0.46	6	1.27	47 31 19.95
2_{1+2}	=+0.44	−0.05	=+0.39	7	1.06	70 39 24.99
3_1	=	−0.06	=−0.06	31	0.12	34 40 39.60
3_2	=	−1.33	=−1.33	1	1.77	43 46 25.07
4_1	=	−0.08	=−0.08	8	.05	30 53 30.78
					7.47	

62. *The Precision determination.*

In the precision determination, an approximate method was employed.

For the p. e. of an angle of weight unity, we have the usual formula,

$$r=.6745\sqrt{\frac{[pvv]}{n_c}}$$

when n_c is the number of conditions, local and general.

For the p. e. of an angle in the adjusted figure, an average value is found from formula (25); thus, if

r' = p. e. of an adjusted angle which before adjustment had weight unity,

n_0 = whole number of angles observed.

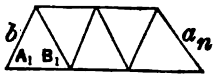
Then

$$r'=r\sqrt{\frac{n_0-n_c}{n_0}} \quad . \quad . \quad . \ (86).$$

For the p. e. of a side, a single chain of the best shaped triangles, and as direct as possible between the base and the side is selected; all tie lines being rejected.

Suppose in a chain, b is the base and a_n the side, whose p. e. is to be found, then

$$a_n = b.\frac{\sin.A_1 \sin.A_2 \ldots\ldots\ldots}{\sin.B_1.\sin.B_2 \ldots\ldots\ldots}$$



or taking logs. and reducing to the linear form, as in

$$d\log.a_n = d\log.b + \frac{d.\log.\sin.A_1}{dA_1}.(A_1) + \ldots$$

$$-\frac{d\log.\sin.B_1}{dB_1}(B_1) - \ldots$$

Now assume that

(1) b is exact.
(2) A_1, $B_1 \ldots\ldots$ are all independently observed,
(3) these observed angles have a common p. e. r'.

Then

$$(\text{p.e.})^2 \log.a_n = [\delta^2_A + \delta^2_B]r'^2.$$

When $\delta_A, \delta_B \ldots$ denote the change of log. sin. A_1, log. sin. $B_1, \ldots$ for the angles $A_1, B_1, \ldots$ in a table of log. sines.

Example.—To find the p. e. of OL as derived from NS. (See Fig. Art. 34).

The chain of triangles is ONS, OLS.
Number of angles=9.
From the adjustment, we find

$$[pvv.] = 7.5.$$

No. of conditions, local and general, = 5

∵ p. e. of an angle of weight 1

$$= 0.6745\sqrt{\frac{7.48}{5}}$$
$$= 0''.82$$

p. e. of an angle of average weight

$$= \sqrt{\frac{9-5}{9}}.r = \frac{2}{3}r$$

Station.	Angle.	Diff. 1″ in units of 7th dec. place.	Diff'ce².
N. Base	122° 11′ 15″	−13.2	174.2
S. Base	23 08 05	49.3	2430.5
Oneota	34 40 40	30.4	924.2
S. Base	70 39 25	7.4	54.8
Oneota	78 27 05	4.3	18.5
Lester	30 53 30	35.2	1239.0
			$[\alpha^2+\beta^2]=4841.2$

∴ p. e. log. $a_n = 38.27$ in units of the seventh place of decimals.

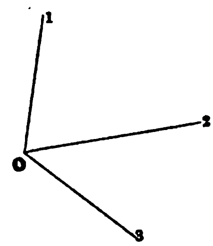
The Method of Directions.

63. I shall take up the usual case in which the directions are observed in "arcs," as explained in Art. 32, and give Bessel's method of reduction as modified and extended by Hansen and Andrae, whose changes have been generally adopted in the more modern European surveys.

The adjustment is divided into two parts, the station adjustment and the general adjustment.

The Station Adjustment.

64. Let O be the station occupied and 1, 2, 3... the stations sighted at in order of azimuth.



Let some one direction be selected as the zero direction, and let A, $B, \ldots$ denote the most probable values of the angles which the directions of the different signals make with this direction.

In the first arc let X' denote the most probable value of the angle between the zero of the limb of the instrument and the direction of the signal taken as the zero direction, then if M_1', $M_1'' \ldots$ denote the readings of the limb for different signals and V_1', $V_1'' \ldots$ the most probable corrections to these readings, we have the observation equations

$$X'-O-M_1'=V_1'$$
$$X'-A-M_1''=V_1''$$
$$X'-B-M_1'''=V_1'''$$
$$\ldots\ldots\ldots\ldots$$

The zero of the limb being changed in the next arc, we have in like manner

$$X''-O-M_2'=V_2'$$
$$X''-A-M_2''-V_2''$$
$$X''-B-M_2'''=V_2'''$$
$$\ldots\ldots\ldots\ldots$$

and so on.

Let $X'=X_0'+x' \quad A=A_0+(A)$
$X''=X_0''+x'' \quad B=B_0+(B)$
$\ldots\ldots\ldots\ldots \quad \ldots\ldots\ldots$

when $X_0', X_0'' \ldots\ldots A_0, B_0 \ldots$ are approximate values of $X', X'' \ldots\ldots A, B, \ldots\ldots$ and $x', x'', \ldots\ldots (A), (B) \ldots\ldots$ denote their most probable corrections.

Also for convenience in writing, put

$$m_1'=M_1'-X_0' \qquad m_2'=M_2'-X_0''$$
$$m_1''=M_1''-X_0'-A_0 \quad m_2''M_2''-X_0''-A_0$$
$$\ldots\ldots\ldots\ldots \qquad \ldots\ldots\ldots\ldots$$

The observation-equations may now be written

$$\begin{aligned} &x'-m_1' = v_1' && x''-m_2' = v_2' \\ &x'+(A)-m_1''=v_1'' && x''+(A)-m_2''=v_2'' \\ &x'+(B)-m_1'''=v_1''' && x''+(B)-m_2'''=v_2''' \\ &\ldots\ldots\ldots\ldots && \ldots\ldots\ldots\ldots \end{aligned} \tag{87}$$

If now $p_1', p_1'' \ldots\ldots : p_2' p_2'' \ldots\ldots$ are the weights of the measured directions of the several series of arcs, the normal-equations follow at once from the above derived observation-equations.

They are

$$\begin{aligned} &[p_1]x' + p_1''(A)+p_1'''(B)+\ldots=[p_1m_1] \\ &[p_2]x'' + p_2''(A)+p_2'''(B)+\ldots=[p_2m_2] \\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned} \tag{88}$$

$$\begin{aligned} &p_1''x'+p_2''x''+\ldots+[p''](A) = [p''m''] \\ &p_1'''x'+p_2'''x''+\ldots+[p'''](B) = [p'''m'''] \\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

The quantities $x', x'' \ldots\ldots$ being merely auxiliary quantities, we eliminate them by substituting their values as found from the first group of normal-equations in the second group.

We have then

$$\left\{[p'']-\frac{p_1''}{[p_1]}p_1''-\frac{p_2''}{[p_2]}p_2''-\ldots\right\}(A)$$
$$+\left\{-\frac{p_1''}{[p_1]}p_1''-\frac{p_2''}{[p_2]}p_2''-\ldots\right\}(B)+\ldots$$
$$=[p''m'']-\frac{p_1''}{[p_1]}-pm-\frac{p_2''}{[p_2]}[p_2m_2]\ldots\ldots$$
$$+\left\{[p''']-\frac{p_1'''}{[p_1]}p_1'''-\frac{p_2'''}{p_2}p_2'''-\ldots\right\}(B)$$
$$+\ldots=[p'''m''']-\frac{p_1'''}{[p_1]}[p_1m_1]$$
$$-\frac{p_2'''}{[p_2]}[p_2m_2]\ldots\ldots$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$

which may for convenience be written

$$\begin{aligned} &[aa](A)+[ab](B)+\ldots\ldots=[al] \\ &[ab](A)+[bb](B)+\ldots\ldots=[bl] \\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned} \tag{89}$$

where $[aa], [ab] \ldots\ldots$ are to be looked on as mere symbols.

65. If the original observations are arranged in groups for adjustment, as is usually done, then

$$p_1'=p_1''=\ldots\ldots\ldots$$
$$p_2'=p_2''=\ldots\ldots\ldots$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$

and if we put

$$[p_1]=\nu_1 p_1, [p_2]=\nu_2 p_2 \ldots\ldots\ldots$$

$\nu_1, \nu_2 \ldots\ldots$ being the numbers of observations in the respective groups, we have for the coefficients of the normal-equations

$$[aa]=p''-\frac{p_1''}{p_1}\cdot\frac{p_1''}{\nu_1}-\frac{p_2''}{p_2}\cdot\frac{p_2''}{\nu_2}-\ldots\ldots$$
$$[ab]= \;\; -\frac{p_1''}{p_1}\cdot\frac{p_1'''}{\nu_1}-\frac{p_2''}{p_2}\cdot\frac{p_2'''}{\nu_2}-\ldots\ldots$$
$$\ldots\ldots\ldots\ldots\ldots\ldots \tag{90}$$

$$[al]=p''m''-\frac{p_1''}{p_1}\cdot\frac{[p_1m_1]}{\nu_1}$$
$$-\frac{p_2''}{p_2}\cdot\frac{[p_2m_2]}{\nu_2}-\ldots\ldots$$
$$[bb]=p'''-\frac{p_1'''}{p_1}\cdot\frac{p_1'''}{\nu_1}-\frac{p_2'''}{p_2}\cdot\frac{p_2'''}{\nu_2}-\ldots\ldots$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$

$$[bl]=[p'''m''']-\frac{p_1'''}{p_1}\cdot\frac{[p_1m_1]}{\nu_1}$$
$$-\frac{p_2'''}{p_2}\cdot\frac{[p_2m_2]}{\nu_2}-\ldots\ldots$$

66. *Checks of the Normal-Equations.* 1.—The sum

$$\begin{aligned} [aa]+[ab]+[ac]&+\ldots\ldots\ldots \\ +[bb]+[bc]&+\ldots\ldots\ldots \\ +[cc]&+\ldots\ldots\ldots \\ &+\ldots\ldots\ldots \end{aligned}$$

is equal to half the number of observations, less half the number of arcs. (91.)

For substitute for $[aa], [ab] \ldots\ldots$ their values from (89) we find the above sum equal to

$$[p''] + p'''] + \ldots - \frac{p_1''^2 + p_1'''^2 + p_1''p_1''' + \ldots}{[p_1]}$$

$$- \frac{p_2''^2 + p_2'''^2 + p_2''p_2''' + \ldots}{[p_2]} - \ldots$$

$$= \frac{1}{2}\Big\{[p'] + [p''] + [p'''] + \ldots\Big\}$$

$$+ \frac{-p_1' + p_1'' + p_1''' + \ldots}{2}$$

$$- \frac{p_1''^2 + p_1'''^2 + p_1''p_1''' + \ldots}{[p_1]}$$

$$+ \frac{-p_2'' + p_2'' + p_2''' + -}{2}$$

$$- \frac{p_2''^2 + p_2'''^2 + p_2''p_2''' + \ldots}{[p_2]}$$

$$+ \ldots\ldots\ldots\ldots\ldots\ldots$$

$$= \frac{1}{2}\Big\{[p'] + [p''] + [p'''] + \ldots\Big\}$$

$$- \frac{[p_1^2]}{2[p_1]} - \frac{[p_2^2]}{2[p_2]} - \ldots\ldots$$

Now $p_1' = p_2' = \ldots\ldots = 1$ and $\therefore$

$[p'] + p''] + [p'''] + \ldots =$ the number of observations.

$\frac{[p_1^2]}{[p_1]} + \frac{[p_2^2]}{[p_2]} + \frac{[p_3^2]}{[p_3]} + \ldots =$ the number of arcs.

2.—The sum

$$[al] + [bl] + [cl] + \ldots\ldots = [wl] \quad (92).$$

where $[wl]$ is formed in the same way as $[al]$, $[bl]$, ... For

$$[al] + [bl] + \ldots = [p''m''] + [p'''m'''] + \ldots$$

$$+ [p_1m_1]\left(\frac{p_1'}{[p_1]} - 1\right) + [p_2m_2]\left(\frac{p_2'}{[p_2]} - 1\right) + \ldots$$

$$= \frac{p_1'}{[p_1]}[p_1m_1] + \frac{p_2'}{[p_2]} + [p_2m_2] + \ldots\ldots$$

Since $[p''m''] + [p'''m'''] + \ldots\ldots$
$- [p_1m_1] - [p_2m_2] - \ldots = [p'm'] = 0$

which proves the proposition.

After having found the quantities m', m''...., by taking the differences between the approximate values of the angles and the several measured values, it is convenient to arrange the formation of the normal-equations according to the following scheme:

No. of group.	$\frac{p}{\nu}$	p	$p''m''$	$p'''m'''$	..	$[pm]$	$\frac{[pm]}{\nu}$
1	$\frac{p_1}{\nu_1}$	p_1	$p_1''m_1''$	$p_1'''m_1'''$	..	$[p_1m_1]$	$\frac{[p_1m_1]}{\nu_1}$
2	$\frac{p_2}{\nu_2}$	p_2	$p_2''m_2''$	$p_2'''m_2'''$	..	$[p_2m_2]$	$\frac{[p_2m_2]}{\nu_2}$
3	$\frac{p_3}{\nu_3}$	p_3	$p_3'''m_3'''$	$p_3'''m_3'''$	..	$[p_3m_3]$	$\frac{[p_3m_3]}{\nu_3}$
..	..	..	....	....	..	..	..
		$[p]$	$[p''m'']$	$[p'''m''']$	..	$[pm]$	

The coefficients of the normal-equations may now be written down at sight.

67. *To find the m. s. e. of a single observed direction.*

We have

$$\mu^2 = \frac{[pvv]}{\text{No. of obs. quan's} - \text{No. of indep't unknowns}}$$

Now, from equations (81), it is evident that the number of observation-equations is equal to the number of directions n_d, and then umber of independent unknowns is the number of arcs n_a + the number of stations or signals n_s less one. Hence

$$\mu^2 = \frac{n_d - (n_a + n_s - 1)}{[pvv]} \qquad (93).$$

To compute the value of $[pvv]$.

From equations (88) and (89) we have the reduced normal-equations,

$$[p_1]x' + \quad + p_1''(\mathrm{A}) + p_1'''(\mathrm{B}) + \ldots = [p_1m_1]$$
$$[p_2]x'' \quad + p_2''(\mathrm{A}) + p_2'''(\mathrm{B}) + \ldots = [p_2m_2]$$
$$\ldots\ldots\ldots\ldots\ldots$$
$$[aa](\mathrm{A}) + [ab](\mathrm{B}) + \ldots\ldots = [al]$$
$$[bb1](\mathrm{B}) + \ldots\ldots = [bl.1]$$
$$\ldots\ldots\ldots\ldots$$

Hence, as in Art. 13,

$$[pvv] = [pmm] - \frac{[p_1m_1]^2}{[p_1]} - \frac{[p_2m_2]^2}{[p_2]} - \ldots$$

$$- \frac{[al]^2}{[aa]} - \frac{[bl.1]^2}{[bb.1]} - \ldots \quad (94).$$

$$= [pmm] - \frac{[p_1m_1]^2}{[p_1]} - \frac{[p_2m_2]^2}{[p_2]} - \ldots$$

$$- [al](\mathrm{A}) - [bl](\mathrm{A}) - \ldots (95).$$

The quantity $[pmm]$ should always, if possible, be found from the original observations. It will generally be quite different if found from the means forming

the different groups of arcs taken as single observations of a certain weight.

When found from the original observations,

$$p_1'=p_1''=\ldots\ldots\ldots=1$$
$$p_2'=p_2''=\ldots\ldots\ldots=1$$
$$\ldots\ldots\ldots\ldots\ldots$$

and then

$$[vv]=[mm]-\frac{[m_1]^2}{\nu_1}-\frac{[m_2]^2}{\nu_2}-\ldots\qquad [al](\mathrm{A})-[bl](\mathrm{B})\ldots.(96).$$

Check formula.—It is evident from the above expression that the computation of the value of $[vv]$ falls into two parts, corresponding to the two sets into which the normal-equations are divided.

A check of the second part is afforded by the equality of the values resulting from

$$\frac{[al]^2}{[aa]}+\frac{[bl1]^2}{[bb1]}+\ldots\ldots\ldots$$
$$[al](\mathrm{A})+[bl](\mathrm{B})+\ldots\ldots$$

The first part which may be denoted by $[vv]_0$ may also be computed in two ways. For

$$[vv]_0=[mm]-\frac{[m_1]^2}{\nu_1}-\frac{[m_2]^2}{\nu_2}-\ldots.(97).$$
$$=m_1'm_1'+m_1''m_1''+\ldots.$$
$$+m_2'm_2'+m_2''m_2''+\ldots.$$
$$-2\frac{[m_1]}{\nu_1}(m_1'+m_1''+\ldots)$$
$$-2\frac{[m_2]}{\nu_2}(m_2'+m_2''+\ldots\ldots)$$
$$+\frac{[m_1]^2}{\nu_1^2}+\frac{[m_1]^2}{\nu_1^2}+\ldots\ldots\ldots \text{ to } \nu_1 \text{ terms.}$$
$$+\frac{[m_2]^2}{\nu_2^2}+\frac{[m_2]^2}{\nu_2^2}+\ldots\ldots\ldots \text{ to } \nu_2 \text{ terms.}$$
$$+\ldots\ldots\ldots\ldots\ldots$$
$$=\left(m_1'-\frac{[m_1]}{\nu_1}\right)^2+\left(m_1''-\frac{[m_1]}{\nu_1}\right)^2+\ldots$$
$$+\left(m_2'-\frac{[m_2]}{\nu_2}\right)^2+\left(m_2''-\frac{[m_2]}{\nu_2}\right)^2+\ldots$$
$$+\ldots\ldots\ldots\ldots\ldots\ldots \qquad (98).$$

The Coast Survey Method of Adjustment.

68. The method of observation of directions in the primary triangulation is essentially that of Bessel. "The number of observations required for the determination of any one angle must depend upon the desired closeness of the result and the character of the instrument." It is left entirely to the judgment of the observer.

"The direction instrument requires that it should be turned on its stand or changed in position in order that the direction of any one line, and consequently of all, should fall upon different parts of the circle as the only security against errors of graduation. The number of positions varies from five to twenty-one of nearly equal arcs: and in each position the circuit of the horizon is made giving the direction of each line by two observations, one in the direct and the other in the reversed position of the telescope. These circuits or series are repeated in each position until two to five values of each direction are obtained. Each angle is therefore determined by from 35 to 63 measurements in the direct, and a like number in the reversed position of the telescope."*

69. The adjustment† is broken into two parts as Bessel's method requires. The total correction to any observed angle M, is therefore made up of two corrections x and y, the one resulting from the adjustment of the directions at the station occupied, the other from the geometrical conditions existing in the triangulation figure. The method of least squares requires

$$[(x+y)^2]=\text{a minimum}$$

which is assumed to be identical with making $[x^2]$ and $[y^2]$ respectively a minimum.

The measured angles at a station are first adjusted, and with these adjusted values $\mathrm{M}+x$, we enter the angle and side-equations which determine the further corrections y, no subsequent notice of the station equations being taken.

This, though not strictly correct, may be considered practically good enough. In good work there will be no marked disarrangement of the station conditions arising from the net adjustment.

The Local Adjustment.

70. The method of deriving the station normal-equations with the checks used is substantially that already given in Arts. 64–65. The quantities $m_1'\ m_1''\ldots.$ are called "diminished measures."

* Methods, Discussions, and Results, Field work of the Triangulation, 1877.
† Report 1854, App. 38: Report 1864, App. 14.

The tabular arrangement for carrying out the numerical work is slightly different, and will now be given for the following

EXAMPLE—AT CLARK MT.

Spear.	Humpback.	Fork.
	° ′ ″	° ′ ″
00 00 00.000	24 09 35.70	78 26 08.55
.00	39.40	09.60
.00	36.23	09.33
.00	33.55	10.45
00 00 00.00		78 26 10.20
.00		11.03
00.00	24 09 36.10	
.00	37.40	
.00	36.53	
.00	38.15	
.00	39.00	
	00 00 00.00	54 16 31.85
		31.94
		32.03
		36.16

Assume the most probable value of the angles.

	° ′ ″
Spear.........	00 00 00.00
Humpback....	24 09 36.90 + (A)
Fork.........	78 26 09.90 + (B)

First form the abstract of diminished measures, by taking the difference between each measured value, and the most probable values of the angles. This gives m_1', m_1''.... To facilitate this work the complements of the seconds are formed for each initial direction, and thus the subtractions are changed to additions. We have then the auxiliary table:

Station.	Assumed angles.	Complements.	Quantities for addition.
Spear.......	00 00 00.00	00.00	
Humpback..	24 09 36.90	23.10	00.00
Fork.......	78 26 09.90	50.10	27.00

In tabulating the abstract of diminished measures, the series of an equal number of directions are placed together beginning with the greatest number. The sums and means are next found as indicated below, and a check is afforded by finding $[pm]$ from the sums in vertical and horizontal rows. (See Table, following page.)

Next form the coefficients of the normal-equations. They are:

$$[aa]=13-\tfrac{4}{3}-\tfrac{9}{2}=\ 7\tfrac{1}{6}$$
$$[bb]=10-\tfrac{4}{3}-\tfrac{6}{2}=\ 5\tfrac{2}{3}$$
$$[ab]=\ -\tfrac{4}{3}-\tfrac{4}{2}=-3\tfrac{1}{3}$$
$$[al]=-0.04\ +1.4633+0.025-1.340$$
$$=+0.1083$$
$$[bl]=-0.29\ +1.4633-0.715-0.025$$
$$=+0.4833$$
$$[wl]=\ -1.4633+0.715\ +1.34$$
$$=\ +0.5917$$

Check (1) $[aa]+[ab]+[bb]=9\tfrac{1}{2}$
$=17-7\tfrac{1}{2}$
$=\tfrac{1}{2}$ (No. of obs.—No. of arcs.)

(2) $[al]+[bl]=0.5916$
$=[\omega l]$ as it should.

Hence the normal-equations are:

$$\begin{array}{ccl} (A) & (B) & \\ +7\tfrac{1}{6} & -3\tfrac{1}{3} & =+0.1083 \\ -3\tfrac{1}{3} & +5\tfrac{2}{3} & =+0.4833 \end{array}$$

Solving we have

(B) = 0.130 (A) = 0.075

and therefore the adjusted values of the angles are

	° ′ ″
Spear...........	00 00 00.000
Humpback.......	24 09 36.975
Fork............	78 26 10.030

So far the method of computation has been rigorous. In what follows to prevent misunderstanding I have frequently quoted directly from the Reports.

71.—We proceed to determine the probable error of each direction.

Now "if s equal the number of observations of any one direction the formula

$$r_1{}^2=\frac{.455\Sigma v^2}{s(s-1)}$$

gives an approximation to the probable error of a direction; to give, however, proper weights to the results since the v's of the more full series are more correct than of series with less directions, it is preferable to substitute for $s-1$ the diagonal coefficient of the weight equations (omitting the remaining combinations) this gives for the

p.e. of the first direction $r_1=\sqrt{\dfrac{.455\Sigma v^2}{s[ww]}}$

" " second " $r_1=\sqrt{\dfrac{.455\Sigma v^2}{s[aa]}}$

" " third " $r_1=\sqrt{\dfrac{.455\Sigma v^2}{s[bb]}}$

etc.; (99).

ABSTRACT OF DIMINISHED MEASURES.

Spear.	Humpback.	Fork.		
00.00 .00 .00 .00	58.80 2.50 59.33 56.65	58.65 59.70 59.42 00.55	$\nu_1=3$	
	$[p_1''m_1'']=-2.72$	$[p_1'''m_1''']=-1.67$	$[p_1m_1]=-4.39$	$\frac{[p_1m_1]}{\nu_1}=-1.4633$
0.00 .00		.30 1.13	$\nu_2=2$	
		$[p_2'''m_2''']=+1.43$	$[p_2m_2]=+1.43$	$\frac{[p_2m_2]}{\nu_2}=+0.7150$
0.00 .00 .00 .00 .00	59.20 .50 59.63 1.25 2.10			
	$p_3''m_3''=+2.68$		$[p_3m_3]=+2.68$	$\frac{[p_3m_3]}{\nu_3}=-1.3400$
	00.00 .00 .00 .00	58.85 58.94 59.08 3.13		
		$p_4'''m_4'''=-0.05$	$[p_4m_4]=-0.05$	$\frac{[p_4m_4]}{\nu_4}=-0.0250$
Sums	$[p''m'']-0.04$	$[p'''m''']=-0.29$	$[pm]=-0.33$	Check sum.
$[p']=11$	$[p'']=13$	$[p''']=10$	$[p]=34$	
ω	a	b		

we also have weight $p=\frac{1}{r_1^2}$."

Here $[aa],[bb]$ . . . are as in (68), and

$$[\omega\omega]=[p']-\frac{p_1'^2}{[p_1]}-\frac{p_2'^2}{[p_2]}\cdot\ \cdot\ \cdot$$

$$=[p']-\frac{p_1'}{p_1}\cdot\frac{p_1'}{p_1}-\frac{p_2'}{p_2}\cdot\frac{p_2'}{v_2}\ .\ .\ (100).$$

Check of the computation of $[\omega\omega]$

$$[\omega\omega]=[p']-\frac{p_1'^2}{[p_1]}-\frac{p_2'^2}{[p_2]}\cdot\ \cdot\ \cdot\ \cdot\ \cdot$$

$$[aa]=[p'']-\frac{p_1''^2}{[p_1]}-\frac{p_2''^2}{[p_2]}\cdot\ \cdot\ \cdot\ \cdot\ \cdot$$

$$[bb]=[p''']-\frac{p_1'''^2}{[p_1]}-\frac{p_2'''^2}{[p_2]}\cdot\ \cdot\ \cdot\ \cdot\ \cdot$$

. . . .

$$\therefore \text{Sum} =[p']+[p'']+\ldots\ \frac{[p_1^2]}{[p_1]}-\frac{[p_2^2]}{[p_2]}.$$

= the number of observations, — the number of arcs. (101).

In our example

$$[\omega\omega]=11-\tfrac{4}{3}-\tfrac{2}{2}-\tfrac{5}{2}=6\tfrac{1}{6}$$

Check

$$[\omega\omega]+[aa]+[bb]=6\tfrac{1}{6}+7\tfrac{1}{6}+5\tfrac{2}{3}$$
$$=19$$
$$=34-15.$$

It is necessary next to find v_1^2, v_2^2, . . . We have

$$v_1' =x'-m_1' \qquad v_2' =x''-m_2''$$
$$v_1'' =x'+A-m_1'' \qquad v_2'' =x''+A-m_2'''$$
$$v_1'''=x'+B-m_1''' \qquad v_2'''=x''+B-m_2''$$
. . . .

which may be written

$$v_1' =x'-d_1' \qquad v_2' =x''-d_2'$$
$$v_1'' =x'-d_1'' \qquad v_2'' =x''-d_2''$$
$$v_1'''=x'+d_1''' \qquad v_2'''=x''-d_2'''$$
. . . .

Now from the first set of normal-equations

$$p_1 x' =p_1'(m_1'-o)+p_1''(m_1''-(A))+p_1'''(m_1'''-(B))+\ \ldots$$
$$=p_1'.d_1'+p_1''.d_1''+\ \ldots$$
$$p_2x''=p_2'd_1'+p_2''d_2''+\ \ldots$$
. . . .

But $\qquad p_1'=p_1''\ .\ .\ .\ .\ =p_1$

and $\qquad [p_1]=p_1.\nu_1$

$$\therefore \quad x' = \frac{[d_1]}{\nu_1} \qquad x'' = \frac{[d_2]}{\nu_2}$$

$$\text{and} \quad v'_1 = \frac{[d_1]}{\nu_1} - d_1' \quad v_2' = \frac{[d_2]}{\nu_2} - d_2''$$

$$v_1'' = \frac{[d_1]}{\nu_1} - d_1'' \quad v_2'' = \frac{[d_2]}{\nu_2} - d_2''$$

$$\dots\dots\dots \qquad \dots\dots\dots$$

The quantities d_1, d_2 . . are called *remaining differences* and v_1', v_2 . . *remaining errors*. The tables used in computing these quantities are called the *abstract of remaining differences* and the *abstract of remaining errors*, respectively.

We have ∴ to pass through two steps in finding the remaining errors:

(1.) The abstract of remaining differences d_1, d_2 . . referred to the initial direction of each series is found by applying the corrections (A), (B) . . . with their signs changed to the respective quantities in the abstract of diminished measures. For another initial direction than the first, the proper differences must be applied. The computation is carried out to the nearest tenth of a second only.

(2.) The abstract of remaining errors v_1, v_2 . . . is formed by comparing each difference d_1, d_2 . . . of the abstract of remaining differences with the mean of its arc. It contains the quantities of the form

$$\frac{[d]}{\nu} - d$$

that is the remaining errors.

ABSTRACT OF REMAINING DIFFERENCES.

Spear.	Humpback.	Fork.	$\frac{d}{v}$
	′	″	
00.0	58.7	58.5	59.1
0	2.4	59.6	.7
0	59.3	59.3	59.5
0	56.6	00.4	59.0
0.0		0.2	0.1
0		1.0	0.5
0.0	59.1		59.6
.0	.4		2
.0	59.6		59.8
.0	1.2		.6
.0	2.0		1.0
	0.0	58.8	59.4
	.0	58 9	59.4
	.0	59.0	59.5
	.0	3.1	1.6

ABSTRACT OF REMAINING ERRORS.

Spear.		Humpback.		Fork.	
v^2	v^2	v^2	v^2	v	v^2
.9	.8	.4	.2	.6	.4
.7	.5	1.7	2.9	1.1	1.2
.5	.2	.2	.0	.2	.0
1.0	1.0	2.4	5.8	1.4	2.0
	2.5		8.9		3.6
.1	.0			.1	0.
.5	.2			.5	.2
	.2				.2
.4	.2	.5	.2		
.2	.0	.2	.0		
.2	.0	.2	.0		
.6	.4	.6	.4		
1.0	1.0	1.0	1.0		
	1.6		1.6		
		.6	.4	.6	.4
		.6	.4	.5	.2
		.5	.2	.5	.2
		1.6	2.6	1.5	2 2
			.86		8.0
	4.3		14.1		6.8

	Spear.	Humpback.	Fork.
s	11.	13.	10.
s[ ]	67.8	93.2	56.7
$.455\, v^2$	1.96	6.42	3.09
r^2	0.029	.069	.054
p	34.5	14.5	18 5
r	0.17	0.26	0.23

72. We have thus at △ Clark Mt. found the corrections x to each direction and also the probable error r_1 of each direction. In the same way at the remaining stations, Spear, Humpback, Fork, of the quadrilateral CSHF which we intend to consider, the corrections have been found from the angles employed. The corrected values for the four stations with the value of r for each direction are as follows:

		°	′	″
At Clark Mt.	(1) Spear....	00	00	00.000±0.17
	Humpback...	24	09	36.975±0.26
	Fork........	78	26	10.030±0.23
At Spear	(2) Humpback...	00	00	00.000±0.23
	Fork........	32	08	11.793±0.21
	Clark Mt.....	54	06	29.197±0.29

		°	′	″	″
At Humpback (3)	Clark Mt.	00	00	00.000	±0.19
	Spear	101	44	03.123	±0.33
	Fork	332	58	11.157	±0.27
At Fork (4)	Clark Mt	00	00	00.000	±0.14
	Spear	79	35	42.479	±0.33
	Humpback ..	98	41	43.926	±0.20

73. It remains now in adjusting the quadrilateral to find the corrections y arising from the side and angle-equations.

The quadrilateral includes four triangles. If S denote the sum of the three angles of a triangle, then $S-180-\varepsilon$ denotes the error of closure of a triangle. The spherical excess and closing error of each triangle are given in the following table. Advantage may be taken of $S-180-\varepsilon$ in forming the angle-equations.

Triangle.	Excess. ″	$S-180-\varepsilon$. ″	$(S-180-\varepsilon)^2$
241	10.773	0.860	0.7396
341	7.386	1.562	2.4398
234	6.402	1 196	1.4304
231	9.789	0.494	0.2440
			Sum 4.8538

Hence

probable error in a triangle=

$$.6745\sqrt{\frac{4.8538}{4}}$$
$$=\pm 0''.743$$

$$\text{probable error in an angle}=\frac{\pm 0.743}{\sqrt{3}}$$
$$=\pm 0''.429$$

$$\text{probable error in a direction}=\frac{\pm 0.429}{\sqrt{2}}$$
$$=\pm 0''.303$$

"These quantities include the errors of observation as well as that arising from the formation of triangles." We therefore write for the probable error of a direction

$$r_{1+\Delta}=\pm 0.''303.$$

"The combination of the angles to triangles introduces a new kind of error which must be taken into account: its general effect is to equalize the weights. This triangle error may be supposed to be principally due to lateral refraction; minor causes may be recognized in the imperfect centering of the theodolite over the same vertical observed upon from the other two stations, and in want of parallelism in the verticals at two stations of unequal elevation, and perhaps in other circumstances. If we separate the observing error r_1 from the error found above, $r_1+\Delta$, we have the triangle error r_Δ. To do this, the probable error of the 24 directions forming the quadrilateral are tabulated and the mean taken. We have

Triangle.	Direction.	Prob. error. ″	Triangle.	Direction.	Prob. error. ″
124	12	±0.17	234	23	±0.28
	14	.23		24	.21
	21	.29		32	.33
	24	.21		34	.27
	41	.14		42	.33
	42	.33		43	.20
123	12	±0.17	134	31	±0.19
	13	.26		34	.27
	21	.29		41	.14
	23	.28		43	.20
	31	.19		13	.26
	32	.34		14	.23

The average of these values gives

$$r_1=\pm 0.238.$$

But

$$r_{1+\Delta}=\pm 0.303.$$

Hence,

$$r_\Delta=\sqrt{0.303^2-0.238^2}$$
$$=\pm 0.129.$$

The observing error, therefore, exceeds the triangle error, and shows that the latter cannot be neglected in the true valuation of the respective weights.

The final value of the probable error (r) of any direction in the quadrilateral, we obtain, therefore, by combining the observing and the triangle error, the latter being constant. We then have

$$r=\sqrt{r_1^2+r_\Delta^2}$$

where $r_\Delta^2=0.0167$

The final weight of each direction is found from the equation

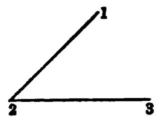
$$p=\frac{1}{r^2}$$

They are as follows:

Direction.	Recip. Wt.	Direction.	Recip. Wt.	Direction.	Recip. Wt.	Direction.	Recip. Wt.
12	.046	12	.046	23	.126	31	.101
14	70	13	.084	24	113	34	.154
21	169	21	.169	32	210	41	.074
24	113	23	.126	34	154	43	.107
41	74	31	.101	42	203	13	.084
42	202	32	.210	43	107	14	.070

The General Adjustment.

74. *Notation.** An angle being considered as a difference of two directions may be expressed in terms of these directions.



Thus the angle 1 2 3 may be denoted

$$-\frac{1}{2}+\frac{3}{2}$$

21, 23 being the arms of the angle and 2 its vertex.

Corrections to a direction are indicated by being included in parentheses; thus $(\frac{1}{2})$ denotes a correction to $\frac{1}{2}$, and the correction to the angle 1 2 3 would be denoted by

$$-(\tfrac{1}{2})+(\tfrac{3}{2}).$$

75. In our example the number of angle-equations is 3, and of side-equations 1. We have

Angle-Equations.

1. The triangle 421

$$\begin{array}{rrrl} & ° & ' & '' \\ 124=21 & 58 & 17.404 & -\frac{4}{2}+\frac{1}{2} \\ 241=79 & 35 & 42.479 & -\frac{1}{4}+\frac{2}{4} \\ 412=78 & 26 & 10.030 & -\frac{2}{1}+\frac{4}{1} \\ \hline 180 & 00 & 9.013 & \\ 180+E=180 & 00 & 10.773 & \end{array}$$

$$0=-0.860-\frac{4}{2}+\frac{1}{2}-\frac{1}{4}+\frac{2}{4}-\frac{2}{1}+\frac{4}{1}$$

Similarly from the triangles 341, 231

$$''$$

$$0=-1.562-\frac{1}{4}+\frac{3}{4}-\frac{3}{1}+\frac{4}{1}-\frac{4}{3}+\frac{1}{3}$$

$$0=-0.494-\frac{1}{3}+\frac{2}{3}-\frac{2}{1}+\frac{3}{1}-\frac{3}{2}+\frac{1}{2}$$

76. *The Side-Equation.*

Take the pole at 1, then

$$\frac{\sin. 123}{\sin. 132}\cdot\frac{\sin. 134}{\sin. 143}\cdot\frac{\sin. 142}{\sin. 124}=1$$

$$\begin{array}{rrrl} & ° & ' & '' \\ \angle 123=54 & 06 & 29.197 & -\frac{3}{2}+\frac{1}{2} \\ \angle 134=27 & 01 & 48.843 & -\frac{4}{3}+\frac{1}{3} \\ \angle 142=79 & 35 & 42.479 & -\frac{1}{4}+\frac{2}{4} \end{array}$$

$$\begin{array}{rrrl} & ° & ' & '' \\ 132=101 & 44 & 3.123 & -\frac{1}{3}+\frac{2}{3} \\ 143=98 & 41 & 43.926 & -\frac{3}{4}+\frac{1}{4} \\ 124=21 & 58 & 17.404 & -\frac{4}{2}+\frac{1}{2} \end{array}$$

$$\begin{array}{l} 9.9085518.108+15.237(-\frac{3}{2}+\frac{1}{2}) \\ 9.6574962.451+41.269(-\frac{4}{3}+\frac{1}{3}) \\ 9.9927991.509+\ 3.866(-\frac{1}{4}+\frac{2}{4}) \\ \quad 472.068 \\ \quad 472.917 \\ \hline \quad -0.849 \end{array}$$

$$\begin{array}{l} 9.9908277.554-\ 4.373(-\frac{1}{3}+\frac{2}{3}) \\ 9.9949791.653-\ 3.220(-\frac{1}{4}+\frac{3}{4}) \\ 9.5730403.710+52.188(-\frac{4}{2}+\frac{1}{2}) \\ \quad 472.917 \end{array}$$

and the conditional equation, with the differences expressed in units of the fifth place of decimals is

$$0.36951(\tfrac{1}{2})+.15237(\tfrac{3}{2})-.36896(\tfrac{1}{3})+.41269(\tfrac{4}{3})+.07086(\tfrac{1}{4})-.03866(\tfrac{2}{4})-.04373(\tfrac{2}{3})-.03220(\tfrac{3}{4})-.52188(\tfrac{4}{3})+0.00849=0$$

77. If now I, II, III, IV, denote the correlates of these four condition-equations, we have the correlate-equations:

$\frac{1}{wt.}$	v	I.	II.	III.	IV.
0.046	$(\frac{2}{1})$	−1	0	−1	0
.084	$(\frac{3}{4})$	0	−1	+1	0
.0					
.070	$(\frac{4}{1})$	+1	+1	0	0
.169	$(\frac{1}{2})$	+1	0	+1	−1.8476
.126	$(\frac{3}{2})$	0	0	−1	0.7618
.113	$(\frac{4}{2})$	−1	0	0	2.6094
.101	$(\frac{1}{3})$	0	+1	−1	1.8448
.210	$(\frac{2}{3})$	0	0	+1	0.2187
.154	$(\frac{4}{3})$	0	−1	0	2.0635
.074	$(\frac{1}{4})$	−1	−1	0	0.3543
.202	$(\frac{2}{4})$	+1	0	0	0.1933
0.107	$(\frac{3}{4})$	0	+1	0	0.1610

The normal equations may now be formed and solved as in Chauvenet, four places of decimals being retained.

The values of the conditions resulting from the solution of the normal equations when applied to the locally adjusted values give the finally adjusted values of the angles.

The triangle sides are completed from the formulæ

$$a=c.\ \sin.\ (A-\frac{\varepsilon}{3}).\quad \text{cosec.}(C-\frac{\varepsilon}{3})$$

$$b=c\ \sin.\ (B-\frac{\varepsilon}{3})\quad \text{cosec.}(C-\frac{\varepsilon}{3})$$

the base c being known.

* Compare Gerling's *Ausgleichungsrechnung.*

* See Reports for 1865 and 1878.

78. *The Precision determination.**

The strict application of the method of least squares to the computation of the probable errors of the adjusted distances is held to be too laborious, and accordingly only an approximate method is employed.

If the triangulation consists of a single chain of triangles, the computation of probable error is carried through the sides of the different triangles between the first and concluding lines. The triangles are independent of one another, and the p. e. r_a of a side a, b being the base is found from

$$r_a = a.\ r \sin. 1'' \sqrt{\tfrac{2}{3}[\cot.^2 A + \cot.^2 B + \cot. A \cot. B]}$$

where r is the average p. e. of an angle as deduced from the corrections to the observed angles, or the difference Δ between observed and adjusted angles. r may also be deduced approximately from the average error of closing of the triangles; also, from

$$r = .6745 \sqrt{\frac{[\Delta^2]}{3n}}$$

where n is the number of triangles, or better, from

$$r = .6745 \sqrt{2 \frac{[v^2]}{n_c}}$$

where n_c is the number of conditional equations and $[v^2]$ the sum of the squares of the direction-corrections.

If the triangulation consists of a chain of quadrilaterals, the p. e. of the last side as found above would need to be divided by $\sqrt{2}$, since we can arrive at that side through two different sets of triangles; and generally "it will be admissible to suppose this combination factor to be proportional to the square root of the fraction (f), number of angles of triangles used in computing r_a divided by number of angles in figure."

In the older work the formula

$$r_a = r\ a \sin. 1'' \sqrt{[\cot.^2 A \cot.^2 B]}$$

which is the same as the one used on the Lake Survey, was employed.

The p. e. found from this formula is too large, because it supposes only two of the angles of a triangle measured instead of three.

The result may be increased by the additional p. e. $\frac{a}{b}.r_b$, which is the effect of the p. e. of the measure of the base b. We have thus finally

$$r_1 = \sqrt{f.\ r_a^2 + \frac{a^2}{b^2}.r_b^2}.$$

The Hansen-Schreiber Method of Adjustment.

79. The general forms that have been given in the articles preceding for the adjustment of a triangulation, though symmetrical and elegant, are quite complex. A method which shall give a marked diminution of work in the reduction without any loss of rigor in the results is a desideratum. Of those proposed I shall notice only that now carried out on the Prussian Land Survey. This method involves processes due to Gauss, Bessel and Hansen.

80. First, a few words as to the measurement of horizontal angles. Gauss, the father of modern geodesy, in his triangulation of Hanover, measured each angle independently of every other, but left no very full account of his modes of procedure. He used a repeating theodolite. The introduction and successful use of non-repeating theodolites by Struve in the measurement of the Russian arc of the meridian, led to the gradual abandonment of repeating theodolites, and consequently drew away attention from Gauss' methods. Through the influence and example of Bessel, the method of "arcs," as developed by him in the *Gradmessung in Ostpreussen*, has been generally followed in primary surveys since his time as being apparently better adapted to the non-repeating theodolite. But for many reasons which every observer must finally discover for himself, as he gains in experience, the conclusion is inevitable that the method of independent angles is the better of the two, even with a non-repeating theodolite.

The points to be considered are mainly accuracy of results; and secondarily, economy of time in making and reducing the observations. When we consider twist of station occupied, the different lengths of lines sighted over, the interruptions that

may occur in the course of reading an arc, the more uniform light that may always be had when the number of directions is small, the greater certainty of eliminating horizontal refraction, and the thoroughness with which periodic error of graduation of the limb of the theodolite can be eliminated, we must conclude that greater precision is to be attained by the method of independent angles. Even Andrae, the author of the most important contributions to the method of directions since Bessel, and who has used this method in the triangulation of Denmark, acknowledges that, "in place of observations of directions in arcs, it is preferable to return to the old method of Gauss of measuring angles." (*Verhandlungen der Europäischen Gradmessung*, 1878.)

As regards the cost it must be acknowledged, that for an equal number of results, leaving quality out of account, the method of arcs has the advantage. Nowadays, however, when facilities exist for measuring angles by night as well as by day, there is less delay in waiting for suitable conditions than when day-work alone had to be depended on. Taking this into account the difference in cost would not be great in any case, more especially as a triangulation party is never a very large one.

81. When the angles are measured independently, the observations may be so arranged as to make the reduction very systematic and easy, as will be shown presently.

This, indeed, is also *possible* with the method of arcs. For if the observations are continuous and every direction is equally well measured in every arc, then

$$p.=p_s=\ldots.=1=p'=p''=\ldots.$$

and $\therefore$

$$[p']=[p'']=\ldots\ldots\ldots=n_a$$
$$[p_1]=[p_2]=\ldots\ldots\ldots=n_s$$

The normal-equations (92) become, remembering that $[m']=o$,

$$\left(n_a-\frac{n_a}{n_s}\right)x-\frac{n_a}{n_s}\cdot y-\ldots=[m'']-\frac{[m]}{n_s}$$
$$+\left(n_a-\frac{n_a}{n_s}\right)y-\ldots.=[m''']-\frac{[m]}{n_s}$$

. . . .

By addition the number of equations being n_s-1

$$\frac{n_a}{n_s}(x=y++\ldots.)=\frac{[m]}{n_s}$$

Hence the normal-equations reduce to

$$n_a\,.\,x=[m'']$$
$$n_a\cdot y=[m'''] \qquad (102).$$

and $\therefore$ the corrections are known.

Also (93) reduces to

$$\mu^2=\frac{[vv]}{(n_a-1)(n_s-1)} \quad . \; . \; (103).$$

This method of reading each arc completely is not *practicable* except when the number of arcs is very small, as the time required would be too great to wait for suitable conditions. Besides, the values of the resulting angles would not be so accurate as if the other method of reading had been employed.

82. In order to diminish the labor of reducing the triangulation the angles must be measured according to some regular form. If measured differently at different stations there is a new problem to be solved for each.

To Gauss and Gerling is attributed the plan of measuring every angle at a station between every two directions. There are many advantages in doing this.

Thus let 0 be the station occupied, and 1, 2, 3, 4, the stations sighted at in order round the horizon; then the angles measured would be

102		
103	203	
104	204	304

Take the first three as independent unknowns.

Then, since

most prob. value=measured value +resid. v

if (A), (B), (C), be the corrections to the assumed approximate values to make the most probable values and (1, 2), and (1, 3) . . the corrections to these approximate values to make the measured values we have the observation-equations

$$\begin{array}{lr} (A) & -(1.2)=v_{1.2} \\ (B) & -(1.3)=v_{1.3} \\ (C) & -(1.4)=v_{1.4} \\ -(A)+(B) & -(2.3)=v_{2.3} \\ -(A)+(C) & -(2.4)=v_{2.4} \\ -(B)+(C) & -(3.4)=v_{3.4} \end{array} \qquad (104).$$

and the normal-equations

$$\left.\begin{aligned} 3(A)-(B)+(C)&=[al] \\ 3(B)-(C)&=[bl] \\ 3(C)&=[cl] \end{aligned}\right\} \quad (105).$$

where

$$\left.\begin{aligned} [al]&=(12)-(23)-(24) \\ [bl]&=(13)+(23)-(34) \\ [cl]&=(14)+(24)+(34) \end{aligned}\right\} \quad (106).$$

Solving these equations (A). (B) (C) are found.

83. So far the solution is that already given in Art. 35, and if the station adjustment only were required we could stop here, as the above equations being symmetrical in form are easily handled.

But in passing on to the net adjustment a very considerable reduction of work can be effected by finding the corrections to the directions instead of to the angles. Thus if X, A, B, C, denote the corrections to the four directions 1, 2, 3, 4, then since A−X, B−X, C−X correspond to the (A), (B), (C), above, the observation-equations may be written

$$\left.\begin{aligned} -X+A\qquad\quad -(12)&=v_{1.2} \\ -X\qquad +B\quad -(13)&=v_{1.3} \\ -X\qquad\qquad +C-(14)&=v_{1.4} \\ -A+B\quad -(23)&=v_{2.3} \\ -A\qquad +C-(24)&=v_{2.4} \\ -B+C-(34)&=v_{3.4} \end{aligned}\right\} \quad (107).$$

and the normal-equations,

$$\begin{aligned} 3X-A-B-C&=-[al]-[bl]-[cl] \\ 3A-B-C&=[al] \\ 2B-C&=[bl] \\ 3C&=[cl] \end{aligned}$$

By addition of these equations there results:

$$0=0$$

and ∴ the unknowns cannot be found without another relation between X, A, B, C. The reason of this is that directions are nothing but the angles which the rays make with some common zero ray whose position is unknown, and which therefore may be taken arbitrarily. To solve the above equations it will be most convenient to make the arbitrary assumption

$$X+A+B+C=0 \quad . \quad . \quad (109).$$

and the normal-equations reduce to

$$\left.\begin{aligned} 4X&=-[al]-[bl]-[cl] \\ 4A&=\ [al] \\ 4B&=\ [bl] \\ 4C&=\ [cl] \end{aligned}\right\} \quad (110).$$

that is

$$\left.\begin{aligned} 4X&=-(12)-(13)-(14) \\ 4A&=\ (12)-(23)-(24) \\ 4B&=\ (13)+(23)-(34) \\ 4C&=\ (14)+(24)+(34) \end{aligned}\right\} \quad (111).$$

which can be very conveniently computed from the scheme due to Col. Schreiber:

1	2	3	4	Sums.
	(12)	(13)	(14)	σ_1
		(23)	(24)	σ_2
			(34)	σ_3
	s_2	s_3	s_4	
$-\sigma_1$	$-\sigma_1$	$-\sigma_2$		
$[al]$	$[bl]$	$[cl]$	$[dl]$	

84. The general adjustment due to the side and angle-equations in the net is equally simple.

The weight-equations, corresponding to equations (42), are:

$$\left.\begin{aligned} 4[\alpha\alpha]\qquad\qquad &=1 \\ 4[\alpha\beta]\qquad &=0 \\ 4[\alpha\gamma]\quad &=0 \\ 4[\alpha\delta]&=0 \end{aligned}\right\} \quad (112).$$

Similarly for the other unknowns. Hence in this case

$$\begin{aligned} [\alpha\alpha]&=[\beta\beta]=[\gamma\gamma]=\tfrac{1}{4} \\ [\alpha\beta]&=[\alpha\gamma]=\ ..\ =0 \end{aligned}$$

and since

$$\begin{aligned} [\overline{a\mathrm{A}}]&=[aa] \\ [\overline{a\mathrm{B}}]&=[ab] \\ &\ldots\ldots \end{aligned}$$

the net correlate normal-equations (47) become

$$\left.\begin{aligned} [aa]\mathrm{I}+[ab]\mathrm{II}+[ac]\mathrm{III}&=l_0' \\ [bb]\mathrm{II}+[bc]\mathrm{III}&=l_0'' \\ [cc]\mathrm{III}&=l_0''' \end{aligned}\right\} \quad (113).$$

just as if formed from

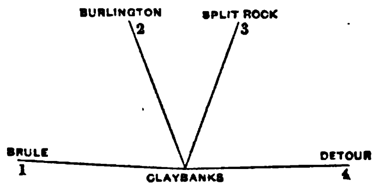
$$\begin{aligned} a'.\,\mathrm{I}+b'.\,\mathrm{II}+c'\ .\,\mathrm{III} \\ +b''.\,\mathrm{II}+c''.\,\mathrm{III} \\ +c'''.\,\mathrm{III} \end{aligned}$$

Also the equations (43) from which the corrections (1) . (2).... are found, reduce to the simple form

$$\left.\begin{aligned} (1)&=a'\ .\ \mathrm{I}+b'\ .\ \mathrm{II}+\ldots. \\ (2)&=a''.\ \mathrm{I}+b''.\ \mathrm{II} \\ &\ldots\ldots\ldots\ldots \end{aligned}\right\} \quad (114).$$

85. *Example of Station Adjustment.* Given at station Claybanks in the triangulation of Lake Superior

	°	′	″
12=	61	33	23.42
13=	100	23	02.38
14=	170	02	03.37
23=	38	49	38.70
24=	108	28	38.98
34=	69	38	60.47



Assumed approximate angles:

	°	′	″
Brule......	0	00	00+A
Burlington..	61	33	23+B
Split Rock..	100	23	02+C
Detour.....	170	02	03+D

1	2	3	4	Sums.
	+0.42	+0.38	+0.37	+1.17
		−0 80	−1.02	−1.82
			−0.53	−0.53
	+0.42	+0.08	−1.18	
−1.17	+1.32	+0.53		
−1.17	+1.74	+0.61	−1.18	

$$\therefore \quad \begin{aligned} A &= -0''.29\tfrac{1}{4} \\ B &= +0.43\tfrac{1}{2} \\ C &= +0.15\tfrac{1}{4} \\ D &= -0.29\tfrac{1}{2} \end{aligned}$$

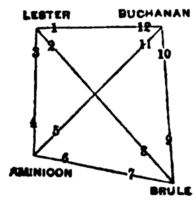
and the adjusted angles,

	°	′	″
12=	61	33	23.73
13=	100	23	02.45
14=	170	02	03.00
23=	38	49	38.72
24=	108	28	39.27
34=	69	38	60.55

86. *Example of Station and Net Adjustment.*

The quadrilateral Aminicon, Brule, Buchanan, Lester.

	Direction.	Measured angle. ° ′ ″	Locally adjusted angle. ″
At Lester....	2−1	51 00 39.77	40.00
	3−1	92 43 49.42	49.20
	3−2	41 43 08.97	9.20
At Aminicon.	5−4	37 47 05.51	5.17
	6−4	97 51 03.11	3.45
	6−5	60 03 58.62	58.28
At Brule.....	8−7	40 25 47.49	47.36
	9−7	71 58 27.31	27.45
	9−8	31 32 40.22	40.19
At Buchanan.	11−10	47 57 36.25	36 21
	12−10	97 26 41.29	41.32
	12−11	49 29 05.15	5.11



The condition-equations formed in the usual way from the triangles Lester, Buchanan, Brule, ($\varepsilon=1''.19$); Lester, Aminicon, Brule, ($\varepsilon=1''.19$); Aminicon, Buchanan, Brule, ($\varepsilon=1''.37$); and from the quadrilateral (pole at Lester) are

$$-(1)+(2)-(8)$$
$$-(2)+(3)-(4)$$
$$-(5)+(6)-(7)$$
$$+3.01(4)-2.72(5)-0.29(6)$$

$$+(9)-(10)+(12)$$
$$+(6)-(7)+(8)$$
$$+(9)-(10)+(11)$$
$$+2.47(7)-5.90(8)+3.43(9)$$

$$+0''.22=0$$
$$+1\ .18=0$$
$$-0\ .57=0$$
$$-0.28(10)-1.80(11)+2.08(12)+1.60=0$$

Auxiliary table for forming the normal-equations:

I.	II.	III.	IV.
−1			
+1	−1		
	+1		
	−1		+3.01
		−1	−2.72
	+1	+1	− .29
	−1	−1	+2.47
−1	+1		−5.90
+1		+1	+3.43
−1		−1	−0.28
		+1	−1.80
+1			+2.08

The normal-equations:

I.	II.	III.	IV.
+6	−2	+2	+11.69 = +0.22
	+6	+2	−11.67 = −1.18
		+6	+ 1.87 = −0.57
			+76.86 = +1.60

$\therefore \quad \mathrm{I} = +0.0095 \qquad \mathrm{III} = +0.0181$

$\mathrm{II} = +0.2123 \qquad \mathrm{IV} = +0.0095$

The corrections:

$''$	$''$
(1) = −0.01	(7) = −0.21
(2) = −0.20	(8) = +0.15
(3) = +0.21	(9) = +0.06
(4) = −0.18	(10) = −0.03
(5) = −0.04	(11) = 0.00
(6) = +0.23	(12) = +0.03

Approximate Method of Adjustment.

87. In an extensive triangulation the form given above is very convenient and gives accurate results. In a chain of central polygons the Schleiermacher solution * may be used to advantage. In this country, however, where chains of triangles and quadrilaterals of the ordinary form have have been more common than central polygons and are likely to be so on account of greater cheapness, a rapid and nearly exact rule for a reduction of this kind will now be given. This will be of special advantage in surveys of rivers where a long string of triangles and quadrilaterals is unavoidable. In more complicated cases, such as triangulating for finding the axis of a tunnel, a complex net is pretty certain to be the form and for the planning and adjustment of this there is nothing better than that given in Arts. 82–86. As an example of tunnel adjustment, the discussion by Koppe of the triangulation for finding the axis of the St. Gothard Tunnel may be cited. (*Zeitschr. für Vermess.* 1875.)

It shall be my purpose in this chapter to derive rules for solving a triangulation chain of a purely mechanical kind requiring no knowledge of least squares in order to apply them.

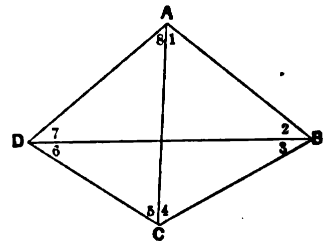
The principle underlying the whole process is that of successive approximation as explained in Art. 27. After an adjustment has been made for one set of conditions, the resulting angles are introduced as measured angles into the adjustment arising from the next set of conditions. The steps are as follows:

(*a*) Adjustment for sum-angles. Rule in Art. 35.

(*b*) Adjustment for closure of the horizon. Rule in Art. 35.

(*c*) Adjustment of a single triangle, all of the angles being measured.

(*d*) Adjustment of a single quadrilateral ABCD, all of the eight angles being measured. A rule for this may be derived as follows: With the usual notation the condition-equations may be written in general terms in two sets.



(1.) The angle-equations from the triangles ABC, BCD, CDA.

$$v_1 + v_2 + v_3 + v_4 = l_1$$
$$v_3 + v_4 + v_5 + v_6 = l_2$$
$$v_5 + v_6 + v_7 + v_8 = l_3$$

(2). The side-equation pole C,

$$a_1v_1 + a_2v_2 + a_3v_3 + a_4v_4 + a_5v_5 + a_6v_6 + a_7v_7 + a_8v_8 = l_4$$

As in Bessel's form we break the solution into two parts.

The correlate-equations are:

$$\begin{aligned} k_1 \qquad\quad &+ v_1 = 0 \\ k_1 \qquad\quad &+ v_2 = 0 \\ k_1 + k_2 \qquad &+ v_3 = 0 \\ k_1 + k_2 \qquad &+ v_4 = 0 \\ k_2 + k_3 &+ v_5 = 0 \\ k_2 + k_3 &+ v_6 = 0 \\ k_3 &+ v_7 = 0 \\ k_3 &+ v_8 = 0 \end{aligned}$$

and the normal-equations:

$$\begin{aligned} 4k_1 + 2k_2 \qquad &+ l_1 = 0 \\ 2k_1 + 4k_2 + 2k_3 &+ l_2 = 0 \\ 2k_2 + 4k_3 &+ l_3 = 0 \end{aligned}$$

Solving these equations there result:

$$k_1 = \tfrac{1}{8}(-3l_1 + 2l_2 - l_3)$$
$$k_2 = \tfrac{1}{8}(+2l_1 - 4l_2 + 2l_3)$$
$$k_3 = \tfrac{1}{8}(-l_1 + 2l_2 - 3l_3)$$

* See Fischer's "Geodäsie," Part III.

Substituting these values in the correlate-equations, and

$$
\begin{aligned}
v_1 &= v_2 = \tfrac{1}{8}(+3l_1 - 2l_2 + \ l_3 \\
v_3 &= v_4 = \tfrac{1}{8}(+\ l_1 + 2l_2 - \ l_3 \\
v_5 &= v_6 = \tfrac{1}{8}(-\ l_1 + 2l_2 + \ l_3 \\
v_7 &= v_8 = \tfrac{1}{8}(+\ l_1 - 2l_2 + 3l_3
\end{aligned}
$$

which may be written:

$$
\begin{aligned}
v_1 &= v_2 = \tfrac{1}{4}l_1 - \tfrac{1}{4}(l_2 - \tfrac{1}{2}l_3 - \tfrac{1}{2}l_1) \\
v_3 &= v_4 = \tfrac{1}{4}l_1 + \tfrac{1}{4}(l_2 - \tfrac{1}{2}l_3 - \tfrac{1}{2}l_1) \\
v_5 &= v_6 = \tfrac{1}{4}l_3 + \tfrac{1}{4}(l_2 - \tfrac{1}{2}l_3 - \tfrac{1}{2}l_1) \\
v_7 &= v_8 = \tfrac{1}{4}l_3 - \tfrac{1}{4}(l_2 - \tfrac{1}{2}l_3 - \tfrac{1}{2}l_1)
\end{aligned}
$$

whence follows at once the convenient rule for adjusting the quadrilateral so far as the angle-equations are concerned.

1. Write the measured angles in order of azimuth in two sets of four each, the first set being the angles of ABC and the second those of CDA.
2. Adjust the angles of each set by $\frac{1}{4}$ of the difference of this sum from 180° + excess of triangle, arranging the adjusted angles in two columns so that the first column will show the angles of ABD and the second those of CBD.
3. Adjust the first column by $\frac{1}{4}$ of the difference of its sum from 180° + excess of triangle and apply the same correction to the second column with the opposite sign.

90. If now $v_1', v_2' \ldots$ denote the further corrections arising from the side-equation, the condition-equations become:

$$
\begin{aligned}
v_1' + v_2' + v_3' + v_4' &= 0 \\
v_3' + v_4' + v_5' + v_6' &= 0 \\
v_5' + v_6' + v_7' + v_8' &= 0 \\
[av'] &= l_4'
\end{aligned}
$$

with

$$[v'v'] = \text{a minimum}.$$

The solution could be carried out by correlates as usual; but by the following artifice the work is much shortened. By writing the corrections in Hansen's form:

$$
\begin{aligned}
v_1' &= +v + v' & \qquad v_5' &= +v + v''' \\
v_2' &= +v - v' & \qquad v_6' &= +v - v''' \\
v_3' &= -v + v'' & \qquad v_7' &= -v + v'''' \\
v_4' &= -v - v'' & \qquad v_8' &= -v - v''''
\end{aligned}
$$

the first three condition-equations become $0=0$ identically, and we have therefore to deal only with the single condi tion-equation:

$$
\begin{aligned}
&a_1(v+v') + a_2(v-v') + a_3(-v+v'') \\
&\quad + a_4(-v-v'') + a_5(v+v''') + a_6(v-v''') \\
&\qquad + a_7(-v+v'''') + a_8(-v-v'''') = l'_4
\end{aligned}
$$

or

$$
\begin{aligned}
&(a_1 + a_2 - a_3 - a_4 + a_5 + a_6 - a_7 - a_8)v \\
&\quad + (a_1 - a_2)v' + (a_3 - a_4)v'' + (a_5 - a_6)v''' \\
&\qquad + (a_7 - a_8)v'''' = l_4'
\end{aligned}
$$

The correlate-equations are:

$$
\begin{aligned}
k(a_1 + a_2 - a_3 - a_4 + a_5 + a_6 - a_7 - a_8) &= 4v \\
k(a_1 - a_2) &= v' \\
k(a_3 - a_4) &= v'' \\
k(a_5 - a_6) &= v''' \\
k(a_7 - a_8) &= v''''
\end{aligned}
$$

Hence, equating the values of k,

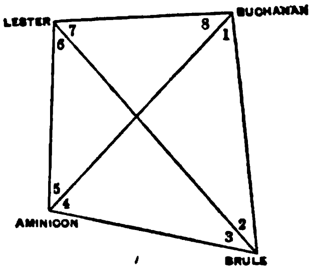
$$
\begin{aligned}
\frac{v}{\frac{1}{4}(a_1 + a_2 - a_3 - a_4 + a_5 + a_6 - a_7 - a_8)} &= \frac{v'}{a_1 - a_2} \\
&= \frac{v''}{a_3 - a_4} \\
&= \frac{v'''}{a_5 - a_6} \\
&= \frac{v''''}{a_7 - a_8}
\end{aligned}
$$

$$
= \frac{l_4'}{\frac{1}{4}(a_1+a_2-a_3-a_4+a_5+a_6-a_7-a_8)^2 + (a_1-a_2)^2 + (a_3-a_4)^2 + (a_5-a_6)^2 + (a_7-a_8)^2}
$$

and, therefore, the corrections are known.

This adjustment is in all respects rigid.

91. *Example.*—Given in the quadrilateral Buchanan, Brule, Aminicon, Lester, the measured angles:



	°	′	″
1=	47	57	36.25
2=	31	32	40.22
3=	40	25	47.49
4=	60	03	58.62
5=	37	47	05.51
6=	41	43	08.97
7=	51	00	39.77
8=	49	29	05.15

ADJUSTMENT.

	Measured Angles.	First Correction.		Angles.	Log Sines.		Diff. 1″		Sums.	Squares
	° ′ ″	″		″						
1	47 57 36.25	35.95		35.85	9.8708000		+1.90			
2	81 32 40.22	39.92	″	39.82		9.7186338		+3.43	5.33	28.41
3	40 25 47.49		47.19	47.29	9.8119206			+2.47		
4	60 03 58.62		58.32	58.42		9.9378201	+1.21		3.68	13.54
	180 00 02.58									
	180 00 01.37	=180+ε								
	4)−1.21									
	−0.30									
5	37 47 05.51		05.91	06.01	9.7872480		+2.72		5.08	25.81
6	41 43 08.97		09.37	09.47		9.8231862		+2.36		
7	51 00 38.77	40.17		40 07	9.8905709			+1.70	3.50	12.25
8	49 29 05.15	05.55		05.45		9.8809474	+1.80			
	179 59 59.40	1.59			395	375	+7.63	+9.96		
	180 00 01.01	1.19			375			+7.63		
	4)+1.61	4)−0.40			20			4)+2.33	0.58	0.34
	+0.40	−0.10						+0.58		80.35

Hence

$$\frac{v}{0.58}-\frac{v^{\mathrm{I}}}{5.33}=\frac{v^{\mathrm{II}}}{3.68}=\frac{v^{\mathrm{III}}}{5.08}=\frac{v^{\mathrm{IV}}}{3.50}=\frac{20}{80.35}$$

and

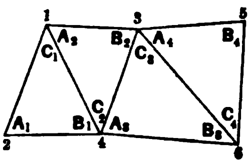
$$\begin{array}{ll}
v\ \ =0.144 & +v+v'\ =+1.47 \\
v^{\mathrm{I}}\ =1.327 & +v-v'\ =-1.18 \\
v^{\mathrm{II}}=0.916 & -v+v^{\mathrm{II}}=+0.77 \\
v^{\mathrm{III}}=1.264 & -v\ -v^{\mathrm{II}}=-1.06 \\
v^{\mathrm{IV}}=0.871 & +v+v^{\mathrm{III}}=+1.41 \\
 & +v-v^{\mathrm{III}}=-1.12 \\
 & -v+v^{\mathrm{IV}}=+0.73 \\
 & -v-v^{\mathrm{IV}}--1.02
\end{array}$$

Corrected Angles.

°	′	″
47	57	37.32
31	32	38.64
40	25	48.06
60	03	57.36
37	47	07.42
41	43	08.35
51	00	40.80
49	29	04.43

ADJUSTMENT OF A TRIANGULATION FOR AZIMUTH AND BASE LINE CONDITIONS.*

So far we have considered the adjustment of a triangulation with reference to only one measured base. But in an extended chain where bases are measured at intervals, it is important, especially in secondary and tertiary work, to adjust so as to allow the bases to exert their proper influence. Not only so, but if the bases are known in azimuth as well as in length, the intervening triangulation should be made to conform to this as well.



Let 12 and 56 be two bases connected by a net of triangles through intermediate stations 3, 4. The lengths and azimuths of 12 and 56 are assumed to be given correctly.

It is supposed that the net has been already adjusted for local conditions and for the geometrical conditions. Now, omitting all superfluous connections, we may reduce the net to a single chain of triangles. Let us then consider 12 and 56 as connected by a single chain of the best shaped triangles that can be selected from the system.

* See Andrae in *Verhandlungen der Europäischen Gradmessung*, 1877. *G. T. Survey of India*, Vol. 2.

When 56 is computed from 12, the computed value and observed value do not in general agree, and when the azimuth of 56 is computed from that of 12 the computed and known values do not in general agree. We therefore still farther adjust the angles of the triangles so that these discrepancies disappear and that the angles of the triangles may still satisfy the conditions of closure. This is strictly in accordance with the general theorem (27).

We shall adjust for the discrepancies one at a time.

(1.) *Correction for discrepancy in azimuth.*

Let A_1, A_2....be the angles opposite to the sides of continuation.

B_1, B_2....be the angles opposite the bases in order of computation.

C_1, C_2....be the angles opposite to the flank sides.

Let (A_1),(B_1)....denote the corrections to A_1, B_1.....

Now reckoning azimuth from the south round by the west, and passing from the base 12 along the sides 13-35,....the excess l'_2 of the observed over the computed value of the azimuth of the last line of continuation 56 is

$$-(C_1)-(A_2)-(B_2)-(C_3)-(A_4)-(B_4)$$

and passing through the sides 24, 46, . . to the same line l_2'' is

$$(A_1)+(B_1)+(C_2)+(A_3)+(B_3)+(C_4)$$

Hence by addition putting $l_2'+l_2''=2l_2$,

$$\begin{aligned}2l_2=&(A_1)+(B_1)-(C_1)\\&-(A_2)-(B_2)+(C_2)\\&+(A_3)+(B_3)-(C_3)\\&-(A_4)-(B_4)+(C_4)\end{aligned}\qquad(115).$$

the angles adjacent to the flank sides having the + sign and the angles opposite to the flank sides having the − sign on one side of the chain and the + on the other side.

We have also the conditions since the triangle close

$$\begin{aligned}(A_1)+(B_1)+(C_1)=0\\(A_2)+(B_2)+(C_2)=0\\(A_3)+(B_3)+(C_3)=0\\(A_4)+(B_4)+(C_4)=0\end{aligned}\qquad(116).$$

all being subject to the relation

$$(A)^2+(B)^2+(C)^2+\ldots.=\text{a minimum}.$$

Calling k_1, k_2....the correlates of equations (116) and k that of equation (115) we have

$$\begin{array}{ll}k_1+k=(A_1) & k_2-k=(A_2)\\ k_1+k=(B_1) & k_2-k=(B_2)\\ k_1-k=(C_1) & k_2+k=(C_2)\end{array}$$

$$\begin{array}{ll}k_3+k=(A_3) & k_4-k=(A_4)\\ k_3+k=(B_3) & k_4-k=(B_4)\\ k_3-k=(C_3) & k_4+k=(C_4)\end{array}$$

and the normal-equations

$$\begin{array}{l}3k_1 \qquad\qquad +k=0\\ \quad 3k_2 \qquad\quad -k=0\\ \qquad 3k_3 \quad\; +k=0\\ \qquad\quad 3k_4 -k=0\\ k_1-k_2+k_3-k_4+12k=2l_2\end{array}$$

Hence solving we should have for n triangles

$$\begin{aligned}2\tfrac{2}{3}\,n\,k=&(A_1)+(B_1)-(C_1)\\&-(A_2)-(B_2)+(C_2)\\&\ldots\ldots\ldots\ldots\\=&2l_2\end{aligned}$$

and

$$k=\frac{3l_2}{4n}$$

also

$$k_1=-k_2=\ldots.=-\frac{1}{4n}.l_2$$

$\therefore$

$$(A_1)=+\frac{1}{2n}.l_2\quad (A_2)=-\frac{1}{2n}.l_2\ldots\ldots$$

$$(B_1)=+\frac{1}{2n}.l_2\quad (B_2)=-\frac{1}{2n}.l_2\ldots\ldots$$

$$(C_1)=-\frac{1}{n}.l_2\quad (C_2)=-\frac{1}{n}.l_2\ldots\ldots$$

Hence the rule. *Divide the excess of the observed over the computed azimuth by the number of triangles, and apply one-half of this quantity to each of the angles adjacent to the flank sides on one side of the chain, and the total quantity with the sign changed to the third angle.* The signs are reversed for the angles on the other flank.

If the azimuth mark is not on a triangulation side, the line of azimuth may be swung on to such a side by adding the angle between the mark and the station.

If, again, the azimuth is observed at intermediate points and not at the ends of the system, a slight error may come into the base line triangles. The process can be repeated if the discrepancy is too great.

(2) *Correction for discrepancy in bases.*

Call the angles adjusted for local and general conditions and for discrepancy in azimuth A_1', B_1', ... Computing 56 from 12 through the intervening triangles we find

$$\text{Computed } 56=12\frac{\sin.\ A_1'.\sin.\ A_2'....}{\sin.\ B_1'.\sin.\ B_2'....}$$

But the given value of 56 being the most probable value we have

Given 56

$$=12\frac{\sin.[A_1'+(A_1')].\sin.[A_2'+(A_2')]....}{\sin.[B_1'+(B_1')].\sin.[B_2'+(B_2')]....}$$

If $\delta_A', \delta_A'', ...\delta_B', \delta_B''$....are the tabular differences corresponding to 1″ for the angles A_1, A_2, ...B_1, B_2.... in a table of log. sines and

log. given base 56 − log computed base 56 $=l_b$

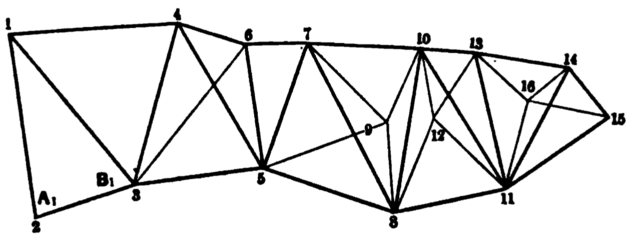
then

$$\delta_A'(A_1')+\delta_A''(A_2')+....$$
$$-\delta_B'(B_1')-\delta_B''(B_2')-.....=l_b$$

Also since the triangles close

$$(A_1')+(B_1')+(C_1')=0$$
$$(A_2')+(B_2')+(C_2')=0$$

....



Call k', k''....the correlates of the angle-equations and k_0 that of the side-equation, then the correlate-equations are:

$$k'+k_0.\delta_A'=(A_1') \quad , \quad k''+k_0\delta_A''=(A_2')..$$
$$k'-k_0.\delta_B'=(B_1') \quad , \quad k''-k_0\delta_B''=(B_2')..$$
$$k' \quad =(C_1') \quad , \quad k'' \quad =(C_2')..$$

Hence eliminating k', k'',....by combining these equations and the angle equations, then result

$$(A_1')=+\tfrac{1}{3}k_0(2\delta_A'+\ \delta_B')$$
$$(B_1')=-\tfrac{1}{3}k_0(\ \delta_A'+2\delta_B')$$
$$(C_1')=-\tfrac{1}{3}k_0(\ \delta_A'-\ \delta_B')$$
$$(A_2')=+\tfrac{1}{3}(2\delta_A''+\ \delta_B'')....$$
$$(B_2')=-\tfrac{1}{3}(\ \delta_A''+2\delta_B'')....$$
$$(C_2')=-\tfrac{1}{3}(\ \delta_A''-\ \delta_B'')....$$

From these equations it is evident that the corrections to the angle C are small compared with those to A and B, and that they vanish when A=B. Hence, as we have assumed the triangles to be well shaped we may take

$$(C_1')=(C_2')=....=0$$

The angle-equations then become

$$(A_1')+(B_1')=0$$
$$(A_2')+(B_2')=0$$

....

and the correlate-equations

$$k'+k_0\delta_A=(A_1') \qquad k''+k_0\delta_A''=(A_2')....$$
$$k'-k_0\delta_B=(B_1') \qquad k''-k_0\delta_B''=(B_2')....$$

whence

$$(A_1')=-(B_1')=\frac{\delta_A'+\delta_B'}{2}k_0$$
$$(A_2')=-(B_2')=\frac{\delta_A'+\delta_B'}{2}k_0$$

....

Substitute these values in the base line equation and

$$\frac{[(\delta_A+\delta_B)^2]}{2}.k_0=l_b$$

Hence k_0 is known and therefore

$$A_1'=-B_1'=\frac{\delta'_A+{}'\delta_B}{[(\delta_A+\delta_B)^2]}.l_b$$
$$A_2'=-B_2'=\frac{\delta''_A+\delta''_B}{[(\delta_A+\delta_B)^2]}.l_b$$

....

that is, the corrections are known.

The corrections to the angles from the base line adjustment will not disturb the azimuth equation, since in each triangle

$$(A')+(B')-(C')=0.$$

Hence it is not necessary to repeat the process.

94. *Example.*—The above sketch represents the secondary triangulation of

Long Island Sound.* It is joined to the primary triangulation at the lines 1-2, 14-15. These lines are assumed to remain unchanged in length and azimuth in the adjustment, and the secondary system is made to conform to them.

The main chain of triangles joining the two primary lines is indicated in the figure by heavy lines. The number of these triangles is 11. The system has been adjusted for local and geometrical conditions, and the resulting angles of these triangles are as follows:

	Angle.			ϵ	Log. Sin. Diff. 1″
	°	′	″	″	
A_1	82	49	19.25	3.66	0.27
B_1	64	55	40.32		0.99
C_1	32	15	04.09		
A_2	52	37	47.98	4.96	1.61
B_2	74	10	50.56		0.60
C_2	53	11	26.42		
A_3	69	20	16.03	3.52	.79
B_3	67	43	43.33		.86
C_3	42	56	04.16		
A_4	39	31	29.66	1.31	2.55
B_4	120	36	36.11		−1.25
C_4	19	51	55.54		
A_5	83	19	31.08	1.41	0.25
B_5	68	57	47.75		0.81
C_5	27	42	42.58		
A_6	87	25	26.66	3.47	0.09
B_6	44	10	45.25		2.17
C_6	48	23	51.56		
A_7	58	25	08.08	3.63	1.29
B_7	82	16	16.16		0.29
C_7	39	18	39.39		
A_8	69	12	06.02	3.03	0.80
B_8	71	58	54.18		0.68
C_8	38	49	03.83		
A_9	59	18	26.33	1.33	1.25
B_9	102	08	26.94		−0.45
C_9	18	33	08.05		
A_{10}	67	38	52.77	2.41	0.87
B_{10}	70	26	23.26		0.75
C_{10}	41	54	46.38		
A_{11}	29	43	55.99	2.13	3.69
B_{11}	55	23	09.36		1.45
C_{11}	94	52	56.78		

M.
Distance, 1- 2=49641.82
" 14-15=22523.20

* Coast Survey Report, 1868.

(1) *The Adjustment for Azimuth.*

After the local and general adjustments were made, a geodetic computation for latitude, longitude, and azimuth was carried through from the line 1-2 to 14-15. It was found that

observed azimuth of 14-15
−computed az. of do$=-2''.93$

Hence the corrections to the angles of the triangles for this discrepancy in azimuth are for the

first triangle $A_1=-0.13$
$B_1=-0.13$
$C_1=+0.27$
second triangle $A_2=+0.13$
$B_2=+0.13$
$C_2=-0.27$

and so on.

(2) *The Adjustment of Base-lines.*

We have

$$\frac{14-15}{1-2}=\frac{\sin.\ [A_1+(A_1)]}{\sin.\ [B_1+(B_1)]}\cdot\frac{\sin.\ [A_2+(A_2)]}{\sin.\ [B_2+(B_2)]}\cdot\cdot\frac{\sin.\ [A_{11}+(A_{11})]}{\sin.\ [B_{11}+(B_{11})]}.$$

or substituting the values of the angles and reducing to the linear form

$$.27(A_1)-0.99(B_1)+1.61(A_2)$$
$$-0.60(B_2)+\ldots.=0.40$$

Hence

$$[(\delta_A+\delta_B)^2]=51.48$$

and

$$(A_1)=-(B^{\circ})=\frac{1.26}{51.48}\times 0.40=0.00$$

$$(A_2)=-(B_2)=\frac{2.21}{51.48}\times 0.40=0.00$$

....

Applying the corrections for the azimuth and side-equations to the already adjusted values we have the final angles.

95. A somewhat different method was used on the Coast Survey in the adjustment of the system of secondary triangulation just given.*

The adjustment consists in an exact conformity to the primary triangulation, to which this secondary series is joined at each end, and to adjust the geometrical conditions in the secondary triangulation. The results deduced cannot differ

* C. S. Report, 1868.

from the most probable by more than the p. e. The successive steps are

1. The measured angles at each station are adjusted if necessary, weights being taken into account. At the primary stations 1, 2, 14, 15, the primary directions remain unchanged, and the secondary directions must be made to conform to them.
2. The condition-equations, 17 angle and 7 side-equations, are formed and solved. The triangle sides are then computed, starting from one of the primary lines, as Ruland-Tashua (1—2), from which the latitude, longitude and azimuth of the whole is computed. This gives

log. McSparran-E.Rock (14–15) 4.3526260
log. do. from pri. triangulation. 4.3526300

	° ′ ″
Az. of line McSparran-E. Rock	282 45 31.382
do. from pri. triangulation.	28.446
Diff. at McSparran....	−2.936
Diff. at E. Rock.......	−2.937
Mean............	−2.931

3. We have now to adjust for the difference in length of the two values of the side McSparran—E. Rock by adding the equation

$$\frac{14.15}{12.1}=\frac{\text{sin. }123.\text{ sin. }413\ldots.}{\text{sin. }231.\text{ sin. }341\ldots.}$$

and to adjust for the difference in azimuth by adding the azimuth-equation.

$$\binom{3}{2}-\binom{2}{3}+\binom{5}{3}-\binom{3}{5}+\binom{8}{5}-\binom{5}{8}+\binom{11}{8}-\binom{8}{11}+\binom{15}{11}-\binom{11}{15}+2''.93=0$$

to the previously-found condition-equations. This makes 26 condition-equations which are again solved and the triangle sides, the latitudes, longitudes and azimuths of all the points recomputed. The secondary line, McSparran—E. Rock, will now of course agree in length and direction with the primary line, but the extremities do not coincide.

4. The small residual differences in latitude ($=-0''.007$) and in longitude $=(-0''.038)$ are next corrected proportoinally to the distance from Tashua, and thus the correct latitude and longitude of each point found.

5. Each station is next reduced to center, the triangle sides recomputed, and a third latitude, longitude and azimuth computation carried through. No contradiction will now appear among the results.

AMERICAN PRACTICE IN WARMING BUILDINGS BY STEAM.

By the late ROBERT BRIGGS, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

II.

Circulation of Steam for Warming.—There are two methods of warming with steam, one with live steam direct from the boiler, and the other with exhaust steam. These two are frequently carried out in combination, and in fact generally so where exhaust steam is used at all for warming. The circulation or distribution of the steam through the warming pipes is effected in an almost unlimited variety of ways, each possessing advantages for special cases. The cause producing the circulation throughout the pipes of the warming apparatus is solely the difference of pressure which results from the more or less rapid condensation of the steam in contact with the radiating surfaces; a partial vacuum of greater or less amount is thereby formed within the radiating portions of the apparatus, and the column of steam or of water equivalent to this diminution of pressure constitutes the effective head producing the flow of steam from the boiler, while the return current of condensed water is determined by the downward inclination of the pipes for the return course.

When using live steam direct from the boiler, the system of what is called closed circulation is carried out either with separate supply and return mains, both of which extend to the furthest distance to which the heat has to be distributed; or else with a single main, which answers at

once for both the supply and the return, either with or without a longitudinal partition inside it for separating the outward current of steam supply from the return current of condensed water. In what is called the system of open circulation, a supply main conveys the steam to the radiating surfaces, whence a return main conducts the condensed water either into an open tank for feeding the boiler, or into a drain to run to waste, the boiler being then fed from some other source; in either case suitable traps have to be provided on the return main, for preserving the steam-pressure within the supply main and radiators. These two systems, in any of their modifications, may also be combined, as is most generally done in any extensive warming apparatus.

In connection with closed circulation, the exceptional character of the single-main distribution calls for some further remarks. The employment of a single main for both supply and return is restricted to buildings where the horizontal distances through which the heat has to be distributed are very short, but where the vertical distances are relatively great. These conditions occur more particularly when the warming is extended to several stories, as in single dwellings or office buildings covering little ground.

There are three principal methods of single-main distribution. In the first, the main is devoid of any internal partition, and is carried up from the boiler at once to the highest point, without taking off from it on the way any distributing branches, all of which are led off from it afterwards in its descending course. Wherever in the ascending course, before the highest point is reached, it has to take an inclined instead of a vertical direction, it must rise at an inclination of not less than 1 in 30 to the horizontal, in order to let the condensed water run back in spite of the current of steam passing in the contrary direction. To ensure the size being large enough, the diameter for a vertical main should be taken at least equal to that furnished by Tables VII. to X. in the Appendix; but for an inclined main (rising 1 in 30) the diameter should be doubled. This first method, however, is much embarrassed by the air which gets entrapped within blind branches that offer no thoroughfare; the radiators are consequently not certain of getting heated, and the apparatus is apt to be noisy for the reasons subsequently explained.

The second method applies to distributions extending through considerable distances horizontally. Here it is more satisfactory to start with a primary circulation through separate inclined supply and return mains, in place of a single main however large or however steep; and to relegate the single-pipe system to a secondary rank, employing it for vertical branches led off from the primary supply main. The course of the primary supply main is a descent with proper slope to the remotest horizontal distance; and, immediately underneath each single-pipe branch rising vertically from it, a receptacle is provided in the primary main, large enough to catch all the condensed water returned from the branch; these receptacles are themselves drained back to the boiler by the return main. From the vertical single-pipe branches, short single offshoots are led off laterally to the radiators in the rooms of the different stories. The air difficulty is avoided with much success by carrying each vertical branch first to the highest point required, free from all lateral offshoots; and supplying the several radiators from its descending or return course, by connections so arranged as to preclude lodgment of air.

In the third method of single-main distribution, which is possibly as satisfactory as either of the two preceding, a single upright pipe is employed, having a hoop-iron partition tightly inserted for separating the ascending from the descending current.

The system of closed circulation requires the boiler to be placed so low as will allow all the return pipes to drain freely back to it above its water-level. This condition has been modified mechanically by the automatic "Albany trap," a device frequently employed for lifting from a lower level part or all of the condensed water, and delivering it into the boiler: it is in fact a displacement pump. The same result has been attained by draining into a closed tank, placed low enough to accommodate all the return pipes, and made strong enough to stand the full boiler pressure with safety; and then employing a steam-pump, either reciprocating or centrifugal, to raise the

water from this tank to the proper level for enabling it to flow back into the boiler, the whole of the circulation being closed from communication with the atmosphere.

Steam mains and branches are apt to be noisy whenever any dipping bend or pocket in the pipes, or any recess in the fittings, allows water to accumulate, and to become cooled below the temperature of the steam supply. In such cases a rapid condensation occurs there, and the steam rushing in carries the water along with it; or sometimes two opposite currents of steam, rushing into the vacuum, meet each other and characteristic noises of rattling and hammering are produced.

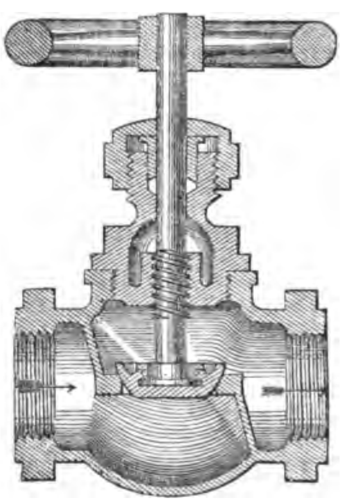
Where separate supply and return pipes are used, whether the system of circulation be open or closed, they should everywhere slope downwards in the direction of the current of steam or of water. A fall of ½ inch in 10 feet (1 in 240) has been found ample to provide against all sag of the pipes or other mechanical defects in the work, and to ensure silent working. When the levels at which the radiators have to be placed do not admit of this slope being continued in the so-called horizontal supply main, vertical breaks are made in the line by the insertion of pipes of larger diameter, which are trapped by check-valves or siphons into the return main. In any extended distribution by separate supply and return mains, the supply main should be connected to the return at the remote ends by the method of open circulation; and between any parallel or not very distant supply and return pipes, occasional drips should be provided, at intervals of say 600 to 1200 times the pipe diameter, siphoned off to prevent any short circuit. Branches upon the horizontal mains, whether supply or return, should be connected upon the top of the main, not at the sides or bottom. Freedom for expansion should be allowed by horse-shoe or S bends, or when practicable by elbows judiciously arranged. Expansion joints are an established fitting for warming apparatus; but their use is not to be approved except in emergency. Repairs are facilitated by substituting at frequent intervals along the mains, in place of some of the screw couplings, cast-iron flange joints, the flanges being screwed upon the tube ends. Main pipes should in all cases be either carried on rollers or suspended, to allow freedom for expansion without straining the joints. The author does not attempt to describe completely all the minutiæ of detail in the construction of the mains for warming by steam; but simply notices some of the appliances commonly employed in America, which have been devised and worked out practically, and are regularly manufactured for general use.

Clothing of Steam Mains.—To prevent loss of heat, steam mains are protected by clothing, as described by Rumford in the last century. Felt is found to perish when applied direct to a surface as hot as 200° Fahrenheit; and Rumford's air-space, formed by enclosing the main within a rather larger casing of thin cast iron, or of sheet iron either plain or tined or zinced, is sufficient for enabling the felt to be employed as the clothing outside the casing. A covering of wire-netting has also been devised. Coats of porous terra cotta or of porous plaster answer, by their low conductivity, to save the felting put outside them. Outside the felt again is applied some suitable sheath or protecting covering that will stand the exposure. When thus clad, the loss incurred in carrying a steam main to any distance—either out of doors, or inside rooms, in passages, in cellars, or in culverts or flues either underground or above—is found to be less than 1 unit of heat for each 100 square feet of external surface of the main itself.

Steam Stop-Valves.—The steam stop-valves, known as "globe valves," are disk or poppet valves, worked by a screw spindle, as shown in the accompanying sketches, Figs. 3 and 4, which represent the "straight-way" make for insertion between two pipes in the same straight line. The annular seating upon which the disk closes is cast in line with the axis of the pipes, and in the middle of the globular or spherical body; a transverse diaphragm above the near end of the seating, and a corresponding wall crossing beneath its far end, close all thoroughfare excepting the aperture in the seating itself. This construction was introduced by the author in 1849, and was immediately followed by all makers. The same globular form for the body has been adopted also, as a matter of symmetrical appearance, for the three makes

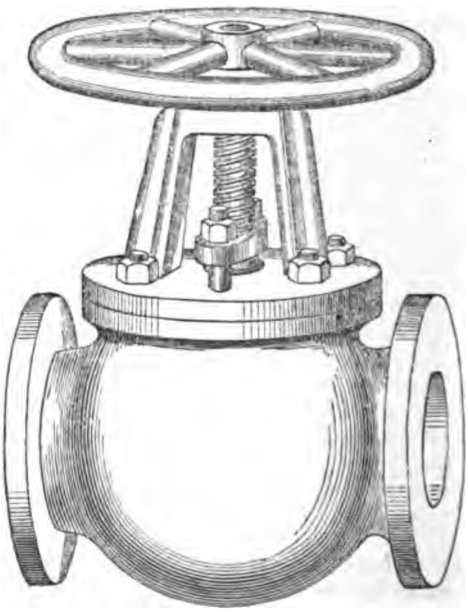
of valve employed to unite pipes at right angles: the first, known as angle valves, unite two pipes, of which one is in line with the valve-spindle, and the other is at right angles to it; the second, called corner valves, unite two pipes at right angles to each other and both of them at right angles to the spindle; and in the third, or cross valves, two pipes in line with each other and at right angles to the valve spindle are united to a third pipe in line with the spindle. The principal dimensions of straight-way globe-valves are given in Table IV. for the various sizes in use. In general the smaller valves, not exceeding $1\frac{1}{4}$ inch in diameter of opening, are wholly of gun-metal; the larger are commonly and preferably made with cast-iron bodies and gun-metal fittings. The smallest valves, from $\frac{1}{4}$ inch up to $\frac{1}{2}$ inch inclusive, have the disk solid with the spindle, and have an ordinary stuffing-box with external gland. Valves of $\frac{3}{4}$ inch and upwards have the disk loose from the spindle, and the spindle is made with a cheese-head let into a recess in the disk, and secured there by an annular nut; they have also a separate internal gland, tightened by a screwed cap, as shown in Fig. 3. The valve-spindles are screwed left-handed, with a double thread of square section; up to 3-inch valves the spindles are screwed to work inside the casing, as in Fig. 3; above that size the screwed portion is outside the casing, and works through a nut in a stool bolted on the casing, as in Fig. 4. In all the valves the disks and seatings have their surfaces of contact shaped spherical; and the disks are without wings to guide them. Above the 3-inch size the nozzles of the cast-iron bodies are generally flanged instead of tapped. This construction is particularly satisfactory for large valves, 10-inch and 12-inch; the last is the largest size usually kept in stock. The resistance presented by a globe-valve to a current flowing through it is assumed at half as much again as the resistance of a sharp right-angled elbow.

FIG. 3.



TAPPED GLOBE VALVE.

FIG. 4.



FLANGED GLOBE VALVE.

Radiators for diffusing heat.—In respect to the radiating surfaces for the diffusion of the heat, there are three distinct classes of warming apparatus in use in America, according to the object to be effected.

First, there is the apparatus for warm-

TABLE IV.—DIMENSIONS OF STRAIGHT-WAY "GLOBE" VALVES.

SIZE.—Diameter of opening in seating.	BODY.—Gun-metal or Cast-iron.	NOZZLES.—Tapped or Flanged.	Length over all.	Diameter of Flanges.	No. of Bolts in each flange.
Inches.			Inches.	Ins.	No.
1/4	Gun-metal	Tapped	1.45		
3/8	do.	do.	1.78		
1/2	do.	do.	2.20		
3/4	do.	do.	2.65		
1	do.	do.	3.30		
1 1/4	do.	do.	3.85		
1 1/2	Gun-metal	do.	4.35		
	Cast-iron	do.	5.10		
2	Gun-metal	do.	5.30		
	Cast-iron	do.	5.90		
	Cast-iron	Flanged	5.75	6	4
2 1/2	Gun-metal	Tapped	6.75		
	Cast-iron	do.	7.30		
	Cast-iron	Flanged	7.25	7	4
3	Gun-metal	Tapped	7.75		
	Cast-iron	do.	9.25		
	Cast-iron	Flanged	9.25	7 1/2	4
3 1/2	Cast-iron	Tapped	10.25		
	do.	Flanged	10.25	8	5
4	do.	Flanged	11.25	9	5
5	do.	do.	13.25	10	6
6	do.	do.	15.25	11	6
8	do.	do.	19.00	13 1/2	8
10	do.	do.	23.00	16	10
12	do.	do.	27.00	19	10

ing rooms by so-called direct radiation; that is, by means of radiating surfaces exposed in the rooms themselves.

Secondly, there is the apparatus for what is sometimes called indirect warming, by means of currents of air: the heated surfaces are placed in a chamber, through which a limited supply of air is allowed to pass on its way into the room. In neither of these two methods is the warming accompanied by any systematic ventilation.

Thirdly, there is the apparatus for both warming and ventilating, arranged so that the warming shall take effect upon the whole supply of air admitted for ventilation.

The first and second methods are, with rare exception, employed for all dwellings, offices, and mills; the third is reserved for hospitals, asylums, public buildings, and the like. On the first and second plans, the warming apparatus is required to be capable of maintaining a comfortable warmth in the very coldest weather, when the outdoor temperature ranges as low as 15° below zero Fahrenheit; whence it is argued in favor of these two methods that in moderately cold weather their warming capacity will be so largely in excess as to admit of ample ventilation by opening windows or doors at pleasure. There are some who condemn altogether the first method or direct radiation, and strongly recommend the second or indirect, on the ground of the supply of fresh air secured by the warming currents in the latter method. But, as a rule, this supply of fresh air is inadequate; and, with the prevalent construction of apparatus, the temperature of the warming currents on issuing from the heating chambers is too high, and cannot be controlled; and the regulation of the warmth in the room has to be effected by opening wider or partially closing the hot-air inlet from the heating chamber into the room, thereby altering the admission of fresh air. On the score of ventilation, the second method is perhaps preferable to the first, because for any temperature of the external air the second method does certainly supply some admission of fresh air, however varying and inadequate. But in the estimation of the community at large, the first method—direct radiation from surfaces exposed in the room itself—offers the pre-eminent advantage that the warming of a room is effected with great certainty and rapidity, and is under the immediate control of the occupant. Direct radiation has also the merit of requiring the smallest consumption of fuel; and although the cost of the apparatus is increased by the superior finish required for the radiators, which are here exposed to view, and by the greater length and complication of the mains, in comparison with the indirect method, yet the increase is about counterbalanced by the saving in extent of radiating surface and in diameter of mains with the direct system; and the special constructions of heating chambers, flues, and dampers, attending the indirect system, make the latter the more costly mode of warming any building.

The conditions of American ventilation and warming, in relation to health and

comfort, differ materially from those prevailing for the same latitudes in Great Britain and other western parts of Europe. The temperature agreeable to Americans in cold weather is about 70° Fahrenheit on the Atlantic coast, rising to 80° or 85° for inland localities and for the severest and driest cold. The American and European requirements differ therefore so completely, that in the tables given in the appendix no attempt has been made to establish any relation between the area of radiating surface in the warming apparatus and the extent of building to be warmed, whether as regards floor area, outside surface of walls or roof, and cubic capacity, or in connection with ventilation for varying numbers of persons or of lights; the tables are presumed to be equally applicable to data derived from experience on either side of the Atlantic.

In the buildings warmed in America by direct radiation there is generally no provision whatever of air-flues, either inlet or outlet, to aid ventilation. This is true for dwelling-rooms, offices, and mill or factory rooms. Recently in many of the better living-rooms, and in offices also, open fire-places have been built, mainly for show, but possessing some utility in producing a little radiating heat for speedy warming. Fire-places, while valuable for ventilation, are chiefly important from the advantages they offer for the attainment of an equable temperature throughout a room. The outlet aperture in any room must be at or near the floor, at any rate in cold weather, so as to remove the air from the bottom of the room where it is coolest. Although the fouler air is of course at the top of every warm room, yet the discomfort consequent upon removing the warmer air from the top, and allowing the cooler air to stagnate below, is found too serious to be endured. A healthy atmosphere would indeed be maintained throughout the room, provided that, in removing air enough from the top, care were taken to supply an equal quantity of fresh air by a diffused admission suitably arranged for comfort; but complete ventilation will always fail to receive due consideration wherever it clashes with comfortable warmth. Gas lights are used, to the exclusion of all others, in American dwellings; and no inconvenience or unhealthiness is considered to result from the unventilated burners. It is fully recognized that the vitiation of air by the combustion of gas is not organic; and although, when the products of combustion are in excess, the air becomes unbreathable, it is by no means the source of disease.

In warming a room by direct radiation, the proper situation of the radiating surface for the attainment of an equable temperature is in some respects a moot question. As long ago as 1846 Dr. Morrell Wyman* called attention to the way in which heat was distributed in any room, when the source of heat was situated as usual at the back of the room, against an inner wall, with windows and exposed outside wall at the front or side of the room; and he showed that a layer of highly heated air collected next the ceiling, whence descended a sheet of air along the cooler wall and window surfaces, becoming itself cooled in its descent, until at about the height of a man a uniform temperature of comfortable warmth was established throughout the room. It is in conformity with this principle that, in the practice of warming in America by means of a hot-air furnace, the hot-air flues from the furnace are generally led up inside an inner wall, remote from windows or outside walls, and a tolerably equable and comfortable warmth is obtained by admitting into the room a limited quantity of very hot air, sometimes nearly as hot as 400° at the inlet. A closed stove, situated likewise against an inner wall, proves similarly effective; but with an open fire the draught of cold air, which from some source or other takes its course along the floor to the fire-place, seriously impairs the desirable uniformity of temperature in a room.

In warming by steam with direct radiating surfaces, the practice for many years was to arrange the steam pipes in lines or groups, called coils, along the bottom of the outside walls or under the windows. But present usage seems to indicate that better results can be obtained by placing the radiating surface where it

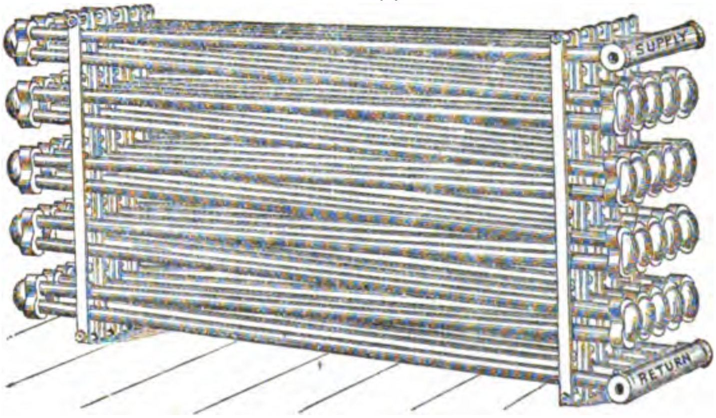
* Dr. Wyman published in Boston in 1846 "A Practical Treatise on Ventilation and Warming," containing much of novelty at the time and of lasting excellence. The work, now out of print, is as suggestive as Walker's "Hints on Ventilation," with which it may be compared.

will apparently be warming what is already the hottest part of the room; in reality it then promotes the natural circulation, whereby the proper diffusion of the warmth is aided, instead of being retarded. For warming rooms in mills, the most recent practice is to place the direct radiating pipes in rows overhead, suspended a foot or two from the ceiling, and two or three feet from the outside walls. Although by this arrangement the heat would apparently be expended in the top of the room, yet very satisfactory results are thereby obtained, in regard both to equability of warming and to efficiency of radiating surface.

In rooms warmed by direct radiation, the attempt has repeatedly been made to effect the ventilation by admitting air direct upon the radiating surfaces; which are then placed under the windows, so as to intercept any descending currents of cooled air. Not much success has attended this plan, inasmuch as through some of the inlet apertures outward currents of warm air are then apt to escape from the room, producing no perceptible warming of the external air; while through others a flood of cold air enters from the outside, and, pssing only some one coil or radiator, gets hardly warmed at all, the effect being far from comfortable.

Construction of Radiators.—For warming by direct radiation, the radiators usually consist of coils, composed of ¾-inch and 1-inch steam-pipes, which are arranged in parallel lines and are coupled to branch tees or heads. In a few exceptional cases, radiators of peculiar shapes are specially constructed. In all cases the coils must have either vertical or horizontal elbows of moderate length, for allowing each pipe to expand separately and freely. Sometimes short lengths of pipe are coupled by return-bends, doubling backwards and forwards in several replications one above another, and forming what are called "return-bend coils;" and when several of these sections are connected by branch tees into a compact mass of tubing, the whole is known as a "box-coil," Fig. 5.

FIG. 5.



RETURN-BEND ON BOX-COIL.

As the amount of heat given off from the radiator cannot be satisfactorily controlled by throttling the steam supply, it is usual to divide all radiators into sections, each of which can be shut off from the supply and return mains, separtely from the rest of the sections. This method of regulation applies to radiators for indirect heating as well as for direct.

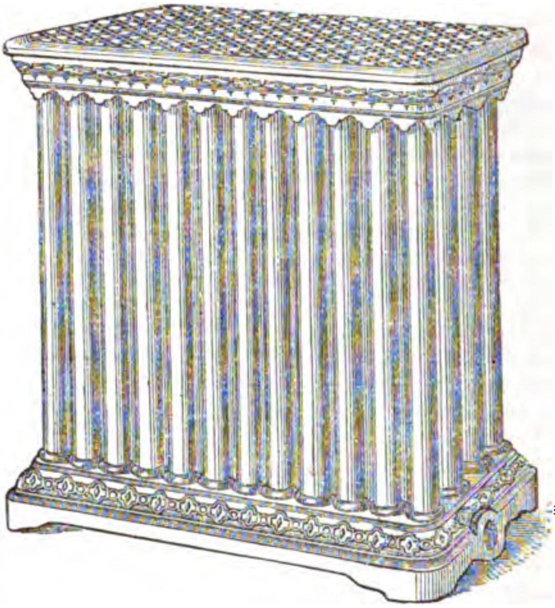
Vertical-pipe coils, Fig. 6, constitutes a distinctive form of radiator now largely used. In these a number of short upright 1-inch tubes, from 2 feet 8 inches to 2 feet 10 inches long, are screwed into a hollow cast-iron base or box; and are either connected together in pairs by return-bends at their upper ends, or else each tube stands singly with its upper end closed, and having a hoop iron partition extending up inside it from the bottom to nearly the top. The supply of steam is admitted into the bottom casting; and the steam

on entering, being lighter than air, ascends through one leg of each siphon pipe and descends through the other, while the condensed water trickles down either leg, and with it the displaced air sinks also into the bottom box. For getting rid of the air, a trap is provided, having an outlet controlled by metallic rods; as soon as all the air has escaped and the rods become heated by the presence of unmixed steam, their expansion closes the outlet.

rosion, although pipes rusted off from the outside are by no means uncommon.

For indirect radiating surfaces, the box coils are the forms most used. The chambers or casings for containing them are made either of brickwork, or often of galvanized sheet-iron of No. 26 gauge, with folded joints. The coils are suspended freely within the chambers, which are themselves attached to the walls containing the air inlet flues. Besides coils of wrought-iron tubes, cast-iron tablets

Fig. 6.



Vertical-Tube Radiator.

One construction of direct radiating surfaces that has had repeated trials consists of tablets of thin sheet-iron. These were proposed and used by James Watt, and were stayed by having their sides indented with recesses and quilted together. The cause of failure lies in the presence of air inside an iron vessel in which steam is condensing. Under no other condition of exposure does iron perish so rapidly. In the ordinary closed circulation, the wrought-iron tubes and cast-iron fittings are practically imperishable internally, owing to the entire exclusion of air. At any rate, during more than thirty years' experience the author has never seen a pipe destroyed by rust by internal cor-

or hollow slabs, having vertical surfaces with projecting studs or ribs, have been extensively used for the radiating surfaces in warming houses with low pressure steam. They are like the tablets common in England for warming with hot water. Their great advantage has been found to lie in their small height; as little as 6 to 8 inches is height enough to warm sufficiently the current of fresh air traveling only that distance in contact with their heating surface.

Ventilation combined with Warming.—Where systematic ventilation is carried out in conjunction with warming, the indirect radiators and chambers just described are employed. The regulation of

the warmth to be supplied by the apparatus can be effected by dividing the coil into independent sections, one or more of which can be shut off at pleasure, as previously mentioned. But in combination with systematic ventilation the warming can also be more effectually controlled by so arranging the casing or chamber containing the coil, that the whole or any part of the fresh air entering can be made to pass through the coil and be warmed, or to "by-pass" the coil and escape warming; the warmed and unwarmed currents are then mingled in a mixing chamber or flue, whence a supply of fresh air suitably tempered flows into the room. This method renders it feasible for the occupant of the room to control the tempering of the air, and thereby to regulate the warming of the room; it also ensures the constant supply of a definite quantity of air.

Where a blowing fan is employed as a means of impelling a current of air through a building, a further improvement for large and extensive warming apparatus is proposed, which consists in placing a large auxiliary warming coil at the entrance of the main supply flue that leads from the fan into the building.* Provision is made for using either the whole of the auxiliary coil or only a portion of it, and it is also provided with by-passages and regulation shutters or dampers, so that the air entering the main supply-flue to the building can at all times be warmed to a uniform temperature of say 50° Fahrenheit. The air thus warmed can be allowed to pass along flues wheresoever situated--underground, through cellars, or under passages of a building—without much loss of warmth, and without danger of injuring foundations by freezing. From these flues the air is admitted into the coil chambers previously described, where it undergoes the further warming requisite to give the desired temperature in the rooms. By this means, while the volume of fresh air entering a room remains constant, its warmth may be regulated to any temperature from 50° as a minimum up to 120° as a maximum. The extent of radiating surface distributed inside the rooms will by this arrangement be only about one-half of the total that has usually to be provided where no auxiliary warming coil is employed; while the large auxiliary coil itself has only about 40 per cent. of the total surface usually provided in its absence. Hence this arrangement actually saves about 10 per cent. of warming surface, irrespective of the saving in cost of steam mains when the boilers are placed near the fan or entrance to the main supply-flue.

The blowing fan generally employed in America for ventilating large buildings is that described by the author in a former paper to this Institution. The exhaust steam from the engine driving the fan is utilized for warming some large coil; and any deficiency in quantity is made up by a supply of live steam taken direct from the boiler through a "differential pressure" valve, which is of common use where exhaust steam is employed for warming.

Examples of extensive Warming by Steam.—As an example of warming on an extensive scale may be taken a large office in New York, of which the following are the particulars:—

Total number of rooms, including halls and vaults....	286
Total area of floor surface....sq. ft.	137,370
Total volume of rooms......cub. ft.	1,923,590
Number of constant occupants during office hours..................	650
Maximum average of occupants at any time.......................	1,300
Volume per occupant, excluding vaults....................cub. ft.	1,443

In addition to the steam for warming, the boilers all furnish steam for the engine power expended in working lifts or elevators in the building, in pumping the water supply, and in electric lighting; and the same boilers also furnish steam for motive power and for warming to other buildings at several hundred feet distance, this extra service absorbing about one-third of the total boiler capacity. The boilers are eight in number, and have altogether 173 square feet of grate area, with about 8,000 square feet of heating surface. The lifts or elevators convey about two million persons per year.

A second example is furnished by the State Lunatic Asylum at Indianapolis:—

* This method is believed to have been first proposed by Major-General M. C. Meigs, U.S.A., when in charge of erecting the U.S. Capitol at Washington, in 1856.

Length of frontage of building, more than		2,000 lin. ft.
Total volume of rooms		2,574,084 cub. ft.
Warming apparatus	Indirect radiating surface . . 23,296	
	Direct 10,804	
	Total	34,100 sq. ft.
Boilers	Grate area	180 sq. ft.
	Heating surface	5,863 sq. ft.

This warming apparatus was constructed by the Walworth Manufacturing Co. of Boston, after the plans of their engineer, Mr. L. R. Greene, contrary to whose advice, however, the ventilation is effected by chimneys instead of by a blowing fan.

As examples of the extent of heat transmission from a central source may be mentioned the hospital at Columbus, Ohio, and that at Buffalo, New York, each warmed by steam, the former having a linear frontage of 2,280 feet, and the latter of about 3,000 feet.

While these dimensions give a notion of the magnitude of the warming apparatus for numerous large buildings, they fail to convey any idea of the very general prevalence of warming by steam in any of the commercial cities of America. The boilers in use for the purpose at any one warehouse are made to supply steam, for warming and for motive power, to any distance and to any extent within the limit of their capacity. The system is adopted in Boston and New York for the larger residences in flats or stories, which are now rapidly coming into favor. There appears indeed no limit to the future extension of systematic steam supply for warming and for motive power; and every facility is afforded for its growth, from the fact of the necessary mechanical details having already been fully worked out.

RECENT ARMOR-PLATE EXPERIMENTS.

From "Nature."

At the conclusion of their labors the "Iron Plate Committee" reported, in 1865, that the best material for the armor of war-ships was wrought iron of the softest and toughest nature. Steel, or steely iron, or combinations of iron and steel, were all pronounced unsuitable for the purpose, after a long course of careful experiments. Accepting this verdict, the designers of armored ships continued to specify for soft iron armor, the makers of guns and projectiles aimed at the perforation of this kind of armor, and the manufacturers sought to secure the desired qualities of softness and toughness in the thicker and heavier plates which they were constantly being called upon to produce. All the armored ships built from 1860 to 1876 were "ironclads," and in that time the thicknesses of armor plates carried on the sides or batteries of completed ships had advanced from 4½ inches to 14 inches, while the weights had risen from 4 or 5 tons to 20 or 25 tons. Greater aggregate thicknesses of iron had been arranged for prior to 1876. For example, the Inflexible had been designed to carry 24 inches of iron on her sides, but this was in two layers of 12-inch plates. The adoption of the so-called "sandwiched fashion" of armor plating was based upon experiments made at Shoeburyness, and it had certain advantages of a constructive character; it also enabled broader and longer plates to be produced within the fixed limits of weights with which the manufacturers could deal, and enabled them to insure excellence of quality which might not have been so certain of attainment in plates of 20 inches or upwards in thickness.

While the two great Sheffield firms and their rivals in France were thus developing the manufacture of iron armor plating, the Creusot Company, of which M. Schneider was the head, were attempting to reverse the verdict against steel armor, and to produce specimens which could hold their own against the best iron armor of equal thickness. The Italian Admiralty brought the claims of the rival materials to the test of experiment at Spezia in October, 1876. In order to decide on the kind of armor to be used on the Duilio and Dandolo, specimen targets were erected and a series of firing trials made against them on a scale of unprecedented magnitude. A gun weighing 100

tons, manufactured at Elswick, was brought to bear upon targets protected by iron or steel plates 22 inches thick, backed by great masses of timber and strong supports. Other guns of considerable weight and power were also used, but their performances were overshadowed by those of the monster weapon. The results of these trials may be briefly summarized. Against the 10-inch and 11-inch guns the 22-inch iron plates had a decided advantage over the steel plate of equal thickness. The penetration was somewhat greater in the iron plates, but the steel plate cracked badly. On the other hand, when the 100-ton gun was brought against the targets, the iron plates and their backings were completely perforated as well as broken up; whereas the steel plate, although smashed to pieces, prevented the shot from passing through the backing. Various opinions were formed as to the deductions which should be made from these trials. On the one side it was urged that as steel plates of great thickness could be gradually cracked and destroyed by guns incapable of perforating them, steel ought not to be used instead of iron, which could be battered by a great number of projectiles from such guns, and be neither perforated nor cracked. On the other side, it was maintained that there was small probability of any single armor plate on a ship's side being struck repeatedly in action, and consequently that the material should be preferred which could best resist perforation by a single projectile from the most powerful gun, even if the resistance to perforation involved the partial destruction of the plate struck. The Italian authorities adopted the latter view, and the Duilio and Dandolo have steel armor, being the first ships protested in that manner.

Although these steel armor plates were made in France, the French authorities did not follow the Italian lead and abandon iron armor. Nor was a similar course followed in England. Change was seen to be inevitable, but it was endeavored to make the change in a direction which should combine the high resistance to perforation of steel with the power to resist cracking and disintegration possessed by tough rolled iron. To Messrs. Cammell & Co., of Sheffield, belongs the honor of taking the lead in this direction; Messrs. Brown speedily followed, and the Admiralty gave substantial assistance in the conduct of the necessary experiments. In the earlier stages many failures and disappointments were experienced, but eventually better results were obtained, and "steel-faced armor" became recognized as the substitute for iron on English war-ships. Steel-faced armor, as the name implies, consists of a rolled iron back-plate, on the face of which is welded a layer of steel. The hard steel face resists perforation, and breaks up or deforms the projectiles, while the intimate union of the tough iron back with the hard steel face prevents the serious cracking which occurs in steel alone. Curiously enough, the idea was not merely an old one, but a small plate made on this principle, 4½ inches thick, had been fired at in 1863. This early steel-faced plate was broken into two pieces at the first shot of a light gun, and was condemned by the Iron Plate Committee. Fourteen years later plates of a similar character, so far as the combination of steel and iron is concerned, but of improved manufacture, were successfully resisting three shots, either of which would have perforated an iron plate of equal thickness.

The first steel-faced plates were used on the Inflexible's turrets; they were 9 inches thick, worked "sandwich-fashion" outside 7-inch iron armor. It was part of the contract that a test-piece from each steel-faced plate should be fired at with a 12-ton gun, and should receive three shots without being broken up or perforated. This was considered to be a very severe test at the time, and undoubtedly was so when the novel conditions of the manufacture are considered. It was successfully met, however, and from that time onwards the manufacture has steadily improved. As an indication of what has been done, it may be stated that steel faced plates, 11 inches thick, have received no less than eight shots from the 12-ton and 18-ton muzzle-loading guns, with battering charges and at 10 yards' range, without perforation or very serious cracking, this enormous "punishment" having been sustained by an area of 48 square feet only. Most of the trials made against steel-faced armor have been against plates from 10 to 12 inches in thickness. For thicknesses up to 12 inches it is

probably within the truth to say that for *normal impact* the steel-faced plates of recent manufacture have been equal in their resistance to perforation to iron plates 25 to 30 per cent. thicker and heavier. For oblique impact the hard armor is probably still more superior to iron, glancing the projectiles at angles of obliquity when they would have "bitten" into the iron. A few experiments have been made in this country and abroad on much thicker steel-faced plates, ranging up to 18 or 19 inches in thickness, and of these the most recent and important are the trials made at Spezia in November, 1882. Three targets were constructed for these trials, the armor plate on each being nearly 11 feet long, 8½ feet wide, and 19 inches thick. One of the targets was covered by a steel-faced plate made by Messrs. Cammell, another by a steel-faced plate made by Messrs. Brown and, the third by a steel plate made at Creusot. All three plates were similarly backed and supported by 4 feet of oak; the Creusot plate was fastened by no less than 20 bolts, and the Sheffield plates had only 6 bolts each. Against these targets the 100-ton muzzle-loading gun was brought into action. At first the powder charge used (329 lbs.) was that which gave such a velocity to the chilled cast-iron projectiles—2,000 lbs. in weight—as would have perforated a 19-inch iron armor plate. The actual penetrations were from 3½ to 5 inches in the steel-faced plates, and 8½ inches in the steel plate, showing that the actual superiority of all the plates over iron considerably exceeded the estimate. The steel plate did not crack at the first shot: the steel-faced plates did, but not to any serious extent. Next followed a more severe attack, the powder charge being increased to 480 lbs., giving the projectiles a velocity estimated to be capable of perforating about 24 inches of iron armor. The total energy of the projectile moving at this velocity exceeded 33,000 foot-tons. All the plates were broken into pieces by this terrific blow. The steel plate was split into six pieces, but the numerous bolts held these pieces in position, and still preserved the defensive power of the target. Each of the steel-faced plates was broken into five pieces, and on account of the fewness of the bolts these pieces fell to the ground, leaving the targets uncovered. The whole of the chilled cast-iron shots were broken up on impact, and the penetration into the steel-faced plates was less than that in the steel plate. At this stage the comparative tests ended. A third round was fired, with the heavier charge and a steel projectile against the steel plate. The shot was stopped, the penetration was only 7 inches, but the plate was broken up, and the backing seriously splintered. A fourth round was fired at this target, and completely wrecked it.

On a review of all the circumstances of the experiments, it must be admitted that the greatest success was attained by the steel plate, although this must be attributed rather to the number and excellence of its fastenings than to superiority in quality of the plate over the steel-faced plates. The latter proved themselves less penetrable than the steel plate, and had rather the advantage as regards fracture at the end of the first two series of rounds; but they were insufficiently secured. One definite lesson to be learned from these experiments is, therefore, that a larger number of bolts is needed for a given area of steel or steel-faced armor than has been commonly used. Another lesson taught by these trials is that the steel armor plates of Creusot manufacture in 1882 are far superior to those made six years earlier. It is not at all probable that light guns such as broke the 22-inch steel plate to pieces in 1876 would have been equally effective against the 19-inch plate recently tested. In both cases the plates were made specially for the firing tests, and they may not have been "merchantable articles" in the sense of representing large quantities of steel armor. But nevertheless this 19-inch plate shows what can be done with steel, if cost is of secondary importance. Authoritative statements are wanting of the actual processes of manufacture, or of the cost of production. It is reported that the 19-inch plate was hammered down from an ingot three or four times as thick as the finished plate, and that the face was oil tempered. If this is correct, the cost must be high, and probably as great as, if not greater than, that of steel-faced plates. Moreover, if such an amount of "work" has to be put into steel plates in their conversion from ingots into the finished forms, then no

great economy or advantage can result from the power which the maker has to cast steel ingots in special shapes or sectional forms. The Creusot Company use a soft steel containing perhaps three-tenths to four-tenths per cent. of carbon, give it toughness by means of a large amount of hammering, and harden the face by oil tempering. On the contrary, the Sheffield firms, as the result of numerous experiments, use a hard steel for the face, the percentage of carbon amounting to about twice that in the Creusot plate. and support this by a tough iron back-With this hard steel oil tempering does not appear to be beneficial, although with softer steel it undoubtedly is an advantage. These steel-faced plates which were tested at Spezia were really samples of large quantities made at Sheffield in the same manner. Probably equally good results would have been obtained if any one of the batch of plates represented had been selected for test. In this respect, therefore, there is a marked difference between the test to which the two manufactures were subjected.

As between the steel and steel-faced plates tried at Spezia, we may assume that there is no notable difference in resistance to perforation or to fracture. Possibly, with equally good and equally numerous fastenings, the steel-faced plates would have had some slight advantage, and in other trials mentioned later on steel-faced plates have had a decided advantage. Supposing no important difference to exist, then the choice between the two kinds of armor will be governed by their relative prices; and how these compare we have no means of judging, but it seems probable that the steel-faced plates would be at least as cheap as steel plates made in the manner described above for the steel test-plate.

It may be convenient in this connection to briefly describe the mode of manufacture of steel-faced plates. Messrs. Cammell prefer to pour the molten steel on to the face of a wrought-iron plate which has been brought to a good welding heat. The layer of molten steel is surrounded by a frame of wrought-iron which has previously been attached to the iron plate; and it is pressed against the surface of the iron plate by a cover carried by an hydraulic ram, until the welding is complete and the steel has solidified. Messrs. Brown prefer first to roll a steel face-plate, as well as an iron back-plate, and then to raise both to a welding heat; the molten steel is afterwards poured into a space left between the two, and hydraulic pressure is applied until the solidification has taken place. The remaining processes are similar in the practice of both firms. After welding has been completed the whole mass is reheated and rolled down to the finished thickness of the armor plate. The steel face is usually about one-half the thickness of the iron back, and it is a curious fact that the iron and steel maintain their relative thicknesses as the rolling proceeds, even when the reduction in thickness during rolling is very considerable. This reduction varies from one-half for thin armor-plates, up to 10 or 11 inches in finished thickness, to one-third with 18 to 20 inches of finished thickness. Some competent authorities consider that too little work is done in the rolls on the thicker plates, but there is a need for further experiment to show whether this view is correct. Whatever may be the cause, it would seem that the best results so far have been obtained with steel faced plates below 12 inches in thickness.

Simultaneously with the Spezia experiments another competition was proceeding, near St. Petersburg, between steel faced and steel armor. The plates tested were 12 inches thick, 8 feet long, and 7 feet wide. They were first fired at with the 11-inch breech-loading gun, throwing a 550-lb. chilled cast iron prejectile, with a powder charge of 132 lbs. The velocity of the shot was 1,500 feet per second. Messrs. Schneider supplied the steel plate, which was fastened with twelve bolts. Messrs. Cammell made the steel-faced plate, which had only four bolts in it. The first blow on the steel plate broke it into five pieces; the projectile was destroyed, but it penetrated 13 inches into the target. A blow of equal energy on the steel-faced plate produced only a few unimportant cracks in the steel, and the penetration was about 5 inches only. Three out of the four bolts were, however, broken. A second shot was then fired at each plate with 81 lbs. charge. The steel plate was broken into nine pieces, and the penetration was 16 inches; whereas, on the steel-faced plate the principal effect produced was to break

the only remaining bolt and to let the plate fall to the ground, face downward. The back of this plate was perfect, and the target behind the plate was uninjured. In this trial the steel-faced plate proved greatly superior to the steel, but had insufficient fastenings. It is proposed to increase the bolts in number, re-erect the plate, and continue the trial, of which the further results cannot fail to be interesting.

This contest between steel and steel-faced armor must not be allowed to withdraw attention from the great superiority of both, in certain respects, to iron armor. Even as matters stand, either of these modern defences is greatly to be preferred to their predecessor. Against this hard armor chilled cast-iron projectiles break up in a manner never seen with soft iron. Projectiles of this kind are virtually impotent, and must be replaced by more expensive, harder projectiles, if steel or steel-faced armor is to be attacked. Even with steel projectiles results cannot be obtained such as were possible with iron armor. Perforation of armor by shells carrying relatively large bursting charges is no longer a possibility; and the heaviest gun yet made cannot drive its projectiles through a thickness of hard armor only three-fourths as great as the thickness of iron which it could perforate.

The use of steel and steel-faced armor will involve many experiments to determine not merely what descriptions of projectiles are best adapted to damage or penetrate it, but what are laws of the resistence of such armor to the penetration and disintegration. All the formulæ based on experiments with soft iron armor and chilled cast-iron projectiles are inapplicable under the new conditions. Perforation is no longer to be feared as the most serious damage likely to happen to armor plates; more moderate thicknesses of hard armor suffice to stop the projectiles from the heaviest guns than would have been considered possible a short time ago. Instead of perforating 19 inches of steel or steel-faced armor, the projectile of the 100-ton gun with a given velocity only penetrates 8 inches into the plates. But, on the other hand, the possible disintegration and fracture of the armor plates are becoming important matters. Makers of armor plates have to endeavor to produce materials which shall resist fracture as well as penetration, and the only proof of their success or failure is to be found in the results of actual trials. Experiments are equally essential to progress in the manufacture of guns and projectiles. The example set by Italy must be followed; the necessary experiments must be on a large and costly scale, and they may lead to many departures from former practice. But if real progress is to be made in the armor and armament of ships, it must be prefaced by experiments beside which those of the former Iron Plate Committee will appear insignificant.

In conclusion, it may be stated that although iron armor has been practically superseded for the sides and batteries of war ships, it is still preferred for decks. Experiments have shown that for angles of incidence below twenty degrees, and for such thicknesses—not exceeding 3 or 4 inches—as are used on decks, good wrought-iron is superior to both steel and steel-faced plating. The explanation of this departure from the laws which hold good for thicker plates and greater angles of incidence cannot be given here, but the fact has been established by elaborate trials made in this country and abroad.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—Regular meeting April 18th, 1883.

The Secretary stated that arrangements were well advanced for the Convention of the Society to be held at St. Paul and Minneapolis, beginning June 20th. The ordinary meetings to be held at St. Paul, one meeting at which the President's address will be delivered to be held at Minneapolis.

A paper by the late Wm. R. Morley, M. Am. Soc. C. E., was read by the Secretary upon the subject of the "Proper Compensation for Railroad Curves upon Grades." Mr. Morley expresses he opinion that the resistance due to curvature is measured not by the length of radius but by the length of train or what is the same, by the ruling grade and that while there may be some increased resistance due to radius, that may be largely overcome by the elevation of the outer rail, and that in the location of a railroad the length of train or ruling grade should be made the basis of compensation, and not the radius of curvature.

He gives examples in his experience where the practice of compensation with reference to the radius resulted upon steep grades in a decided excess of compensation, and in a noticeable increase of speed of the train upon curves.

He also gives the rules adopted by him in his practice which were as follows:

Rate of max. grade.	Per degree compensation.
.00 to .70 per 100 feet.	.06 per 100 feet.
.70 to 1.60 "	.05 "
1.60 to 3.00 "	.04 "

The paper was discussed by Messrs. Bogart, Chanute, T. C. Clarke, Emery, Forney, MacDonald, North, Wm. H. Paine, D. Ward, and L. B. Ward.

In the discussion Mr. Chanute referred particularly to a paper by S. Whinery, M. Am. Soc. C. E., published in the Transactions of the Society in 1878, on the "Resistance of Curves," and the discussion upon that paper, stating that the theoretical resistances determined by Mr. Whinery agreed very closely with the practical results obtained by experiments upon ordinary wheels at low speed, and that the result was an addition of about one-half pound per ton, per degree, to the resistance on straight lines, and that the equation for curvature resulting from this was about half of what Mr. Morley has adopted for his lighter grades.

The paper will be published in an early number of the Transactions, and will be discussed with others at the approaching convention.

Meeting of May 2d, 1883.

The death of Milton Courtright, Fellow of the Society, was announced. Photographs of a dredge and snag boat and of a drilling scow of peculiar construction used by J. H. Striedinger, M. Am. Soc. C. E., in the improvement of the Magdalena River, South America, were presented. The following candidates were elected: As members, George H. Benyeuberg, Milwaukee, Wis.; J. W. Bishop, St. Paul, Minn.; T. L. Griswold, Lagos, Mexico; Randell Hunt, Fargo, Dakota; F. W. Kimball, Milwaukee, Wis.; C. J. Poetsch, Milwaukee, Wis.; A. H. Scott, Milwaukee, Wis.; D. C. Shepard, St. Paul, Minn.; J. A. Smith, Indianapolis, Ind.; G. H. White, Minneapolis, Minn.; as Junior, G. A. Lederle, Bismark, Dakota.

A paper by Professor J. A. Waddell, M. Am. Soc. C. E., "Suggestions as to the Conditions Proper to be Required in Highway Bridge Construction," was read by the Secretary and discussed by Messrs. Cooper, Haight, and MacDonald.

ENGINEERS' CLUB OF PHILADELPHIA. Meeting, April 7th, 1883.

Mr. Arthur Winslow, presented, and described the derivation of, Tables for Stadia Reductions, which furnish expressions for horizontal distances and differences of elevation corresponding to 100 foot stadia readings for 2° up to 30°, on the supposition that the rod be held vertically, and the stadia wires be equidistant from the center wire. They are not mere reductions of inclined distances to their horizontal and vertical exponents, but embody certain corrections necessary from the facts: 1st, that, with horizontal sights, the length cut off by the stadia wires on the rod is not directly proportional to its distance from the center of the instrument but from a point, at a distance in front of the object glass, equal to its principal focal length; and, 2d, that, with inclined sights, a correction has to be made for the oblique view of the rod. Both the distances and elevations in these tables are given in feet; they are adapted to use with a telescope whose object glass has any focal length, and with a rod which is so graduated that the spaces cut off on it by the stadia wires are directly proportional to its distance from a point at a distance in front of the object glass equal to its principal focal length, differing in these respects from the tables issued by the Engineer Department, U. S. A.

Mr. C. O. Hering presented a table giving the number of electrical units of work and power corresponding to all mechanical units, such as horse powers, foot pounds, kilogrammeters, heat units, etc., and the number of the latter corresponding to each of the former, illustrating the same with some examples.

The Secretary presented, for Mr. P. F. Brendlinger, a description of the Bridge Specifications, of the P. McK. & Y. R. R., with specimen copies of the same.

Mr. C. O. Hering exhibited specimen of cable for electric wires.

ENGINEERING NOTES.

BOSTON WATER-WORKS.—An elaborate description of the additional supply of water for the city of Boston from Sudbury River, compiled by Mr. A. Fteley, the resident-engineer upon the work during its construction, has just been issued by the city government in a large, finely printed, and copiously illustrated, volume. The works for supplying Boston with water from Sudbury River consist of three storage-reservoirs in Framingham, and a conduit from that town to Chestnut-hill reservoir in Brookline. In 1881 Sudbury River furnished to Boston more than twice the quantity of water supplied from Lake Cochituate; and steps have already been taken to increase still further the storage-capacity of the system. The volume begins with a discussion of the sources of supply, the rainfall, and the storage-capacity of the reservoirs. Next follows a general description of the dams and reservoirs, and of the several sections of the work, in all its engineering features. The quality of the water, the gauging of the river, and a discussion of the capacity of the conduit, and the flow of water over weirs, conclude the body of the work. The appendix contains valuable tables on water supply hydraulics, and a large amount of information for the practising engineer. The work is illustrated with 69 large plates, commencing with a map of the Sudbury River watershed, and giving very fully the constructive details of the dams and conduits. To give the city 40,000,000 gallons of water daily, it is estimated that the storage-reservoirs on Sudbury River should have a capacity of 4,900,000,000 gallons. So far, three reservoirs only have been built; having a capacity, with that of Farm Pond, of 2,000,000,000 gallons, intended to give a supply of 20,000,000 gallons daily to the city.

THE PROGRESS OF THE ARLBERG TUNNEL.—Some interesting particulars are given concerning the Arlberg tunnel, the boring of which is being pushed with great energy and success. The present road over the Arlberg, which forms the frontier between Austria and Switzerland, is 5,400 feet above the level of the sea; but the tunnel is much lower down, the opening on the Tyrol side being 4,030 feet, and that on the Swiss side 3,770 feet above sea level Its total length will be 10,270 meters (11,161 yards, or six miles and 601 yards), and it runs for the most part through a formation of mica schist. The method of excavation differs from that practised in the making of the St. Gothard tunnel. Instead of piercing the upper part of the passage first, and working down, the Austrian Engineers have preferred to begin at the base and work upwards. The face of the rock is drilled by perforators actuated by compressed air, which is pumped into the tunnel by turbines stationed at its two extremities. When a sufficient number of holes have been drilled they are charged with dynamite and exploded After the blast the débris is removed by trucks, which follow closely on the track of the perforators, and a few minutes later the drilling is going on as rapidly as before. The drift thus made is 2.75 meters wide and 2.50 meters high. While one drift is being driven below, another, to which access is gained by vertical shafts, is being driven above. This work has necessarily to be done by hand, and the rubbish is shunted through openings, made for the purpose, into an inferior gallery. Until very lately the ventilation has given rise to no difficulty, and the heat has rarely exceeded 14 deg. Cent. (58 deg. Fahr.). The contractors have undertaken to make an average advance of 6.60 meters a day. For every day they exceed the given time they will be mulct in a penalty of £68; for every day gained they will receive a premium of £68. So far the contractors have kept well up to time. On not a few occasions the agreed rate of advance has been more than doubled. From January, 1881, when the work began, to September 30th, 1882, the length pierced on the east side was 2,976 meters, on the west 2,643, together 5,619, equal to 8.80 meters daily, figures which are highly significant of the progress made of late years in the method of boring great tunnels. In the month of February last the rate of advance per day was 4.68 meters on the east side, 4.74 on the west side, and but for the scarcity of water, owing to the freezing of the sources of supply, a still better average would have been made. On the west side there is now a stretch of 3,070 meters practicable for locomotion, while on the east side the completed stretch is only 1,430 meters. Up to the end of February the quantity of earth and rock removed amounted to 429.082 cubic meters, and the walling to that date executed measured 121.511 cubic meters. The tunnel is expected to be completed and the line ready for opening by the autumn of 1884.—*Iron.*

THE boring of the Alberg tunnel, which recently suffered some interruption by reason of the nature of the material encountered, is now progressing at the rate of ten meters a day. At the end of last year there remained no more than 3468 meters to pierce.

RAILWAY NOTES.

RAILWAY ACCIDENTS IN 1882.—The blue-book just issued gives returns of accidents and casualties as reported to the Board of Trade by the several railway companies in the United Kingdom during 1882. From this it appears that the total number killed in 1882 was 1,121, as against 1,096 in 1881, and of injured 4,601, as against 4,571 in 1881. The passengers killed were 127, and injured, 1,739, as against 108 killed and 1,860 injured in the previous year.

A SWISS ELECTRIC RAILWAY.—It is proposed to build an electric railway between Saint-Moritz-les-Bains and Portresina, in Switzerland. The length will be 7,200 meters (4½ miles). Previous to the opening of the St. Gothard Tunnel the traffic between Switzerland and Italy passed by Coire and Chiavenna, across the Col du Jubier or the Col du Splugen. The railway on the Swiss side terminates at Coire, and during the next year the Italian system will be completed as far as Chiavenna. The projected electric railway will serve to connect Coire to Chiavenna by two routes. The distance from Coire to St. Moritz is 76.5 kilometers, and from St. Moritz to Chiavenna 48.8 kilometers, or 125 kilometers in all. It is only proposed to erect a short portion at first, and if successful to extend it, as there is ample water-power in the district.

NEW ALGERIAN RAILWAY.—A bill has been laid before the French Chamber of Deputies, the object of which is (1) to declare "of public utility" the proposed railway from Ménerville to Tizi-Ouzon, and (2) to approve a convention agreed upon between the Minister of Public Works and the Eastern-Algerian Company. The concession for the Ménerville and Tizi-Ouzon line has been granted to this company. It is of a total length of about 31 miles, and connects the richest and most populous part of Kabylia with Algeria. It also establishes strategic communication between the upper Sebaon and Fort National on the one hand, and the capital of Algeria on the other. The scheme has been approved by the Minister of War, and the General Council of Bridges and Highways. By the convention, the company is bound to execute the work in four years, and the State guarantees interest at five per cent. on a capital of 676,694*l.* The company is permitted to issue stock to the value of 1,000,000*l.* It deposits 2,000*l.* caution money.

RHINELAND STEAM TRAMWAYS.—This line has been working since June, 1881, with five of Messrs. Merryweather's steam tramway locomotives. It is 5½ miles in length, and consists of a single line with three crossing stations. The roadway is over sand, and it is satisfactory to learn that the wearing parts of the engines give very little indication of want of repair. The traffic is very heavy in the summer time, owing to the many seaside excursions to Kat-

wyk. The metals weigh 15¼ kilogrammes per meter, and are of steel with cross sleepers; and the cost of making the line was under £2,500 per mile, inclusive of the construction of three bridges. In summer the load behind each engine is not less than 20 tons. There is a driver only to each engine; the fire is filled up with coke at starting on each journey, and requires no attention *en route*; in fact, it is found in practice that it is unnecessary to open the furnace door at all *en route*. This is owing to the large capacity of the boiler and its regularity in steaming, it being of the horizontal type. The average consumption of coke is 10 lbs. to 12 lbs. per mile run. The coke is make from German coal, and is of a quality equal only to ordinary gas coke; otherwise the consumption would be even less. The cost of working, including all shop charges, is 2½*d*. per mile, or the same as the Stockton line, which runs the same type of engine. The depot is well and economically managed; it is a plain shed with duplicate line of rails, with a pit to take three engines. The machinery consists of a half-horse power hot-air engine, which drives a pump for filling water tank, also a lathe with thirteen centres and a drilling machine; in addition, there is a smith's forge and two pairs of vises. This is the whole plant, with the exception of small hand tools The shop staff consists of one foreman at 30*s*. per week, and three cleaners at 20*s*. per week. The director states that he finds it much cheaper to get his duplicate parts from the constructors of the engines than to attempt to do the work at home, and this he appears to have proved through two years of successful running. In order to show the sharpness of the sand and the wear of tires, it is stated that a tire runs three months, then is turned up twice, and the average length of a tire is ten months.

IRON AND STEEL NOTES.

As in other departments, so in that of metallurgical science, the past year yields up no event of surpassing importance to place on record. One useful, practical improvement, however, there certainly is for notice, and that is the soaking-pit process of Mr. John Gjers, of Middlesbrough, which was brought under the notice of the members of the Iron and Steel Institute at Vienna last autumn. This process renders it now easy to roll a bloom into a rail or other finished article with its own initial heat, instead of having to submit the ingot to the heating furnace. This is effected by placing the ingots in pits, where, little or no heat being able to escape to the surface, and the ingots being surrounded by walls as hot as themselves, the surface heat of each ingot is greatly increased, and in about half an hour it is fit for the rolling mill. The process was introduced early last year at the Darlington Steel and Iron Company's works in Darlington and at the West Cumberland Steel Works, and at the close of the year at each place over 30,000 tons of ingots had been successfully treated. At Darlington they are now turning out a greatly-increased output by using the pits, and so much heat do the pits retain that, although work is stopped at noon on the Saturday, they are found to be sufficiently hot on the Monday morning, or even later on, to treat the first blow. Of the 30,000 tons treated at Darlington so far, about 10,000 tons have been soft steel for wire billets, and it is considered that the steel is actually improved for all purposes by the soaking pit process. That the invention is a thorough practical success has been established beyond all question, and there are several other works in England at which preparations are being made for its adoption. There is a prevailing opinion amongst railmakers that rails made from soaked ingots are better than if made from ingots put in the heating furnaces, for the danger of burning is entirely obviated. Another departure in metallurgical practice came under our notice in the early part of last year, at which time we gave particulars of it. This was the process of manufacturing iron direct from the ore by the aid of crude petroleum as fuel in a special revolving furnace, the invention of Dr. G. Durgee, of New York. There are two furnaces, an upper and a lower one, placed at slightly different levels, their total length being 120 feet, and their working capacity 100 tons of ore in 24 hours. The ores are pulverized, and then submitted to the action of an oxy-hydrogen flame produced by a blast of air with petroleum and coal-dust, thus to some extent following in wake of the Crampton coal-dust furnace. At the date mentioned some very successful and encouraging experiments were reported, but we have not heard a word nor seen a line about it since. From the Continent we recently received particulars of a new metal—"steel-iron." This metal is produced by pouring molten steel and molten iron into a cast-iron mould divided centrally by a thin sheet of iron, the fluid iron and steel being poured one on either side of the plate, which serves as a medium, welding both parts, steel and iron, completely together. By this process—which is stated to be the invention of Professor Keil—steel-iron of varying character is produced in various ways, and is said to meet the requirements of every class of manufacture, the new metal possessing the characteristics of both steel and iron. In May last year some stir was caused by the announcement of a new system producing of iron and steel, known as Bull's process. The chief features of the process were stated to be the calcination and heating of the charge, the introduction of highly-heated gas into the blast furnace at a certain stage, and the gradual removal of the fuel, the charge being maintained at certain stated levels according as it was desired to vary the percentage of carbon in the metal. Upon instituting inquiries at the time, we could not find that it had passed the experimental stage, but we have just heard that it is now intended to lay down plant and machinery in Sweden for testing it on a working scale.

In spite, however, of the various attempts to introduce new processes for producing steel, that of Sir Henry Bessemer still holds its own, whilst the adoptions of the basic or Thomas-Gilchrist process continue to increase. Turning to the broad question of the produce of steel throughout the world, from some recently

published statistics, it appears that there are now in England 23 steel works, with about 115 converters of a productive capacity of 1,461,000 tons per annum. Austria has 14 steel works, with 36 converters, and a capacity of 350,000 tons; Belgium, 4 steel works, with 18 converters, and a production of 380,000 tons; France, 7 works, with 34 converters, giving a production of 632,000 tons. Germany has 23 Bessemer and Thomas steel works, with 80 converters, and a productive capacity of about 1,3(0,000 tons; Russia has 5 works, with 10 converters, and a production of 100,000 tons; and Sweden, 35 converters of 80,000 tons capacity. In the United States there are in operation altogether 34 converters, with an annual production of 1,500,000 tons. The total number of converters in the world is therefore about 360, with an aggregate annual productive capacity in round numbers of 5,800,000 tons of steel. The substantial position occupied by the Thomas-Gilchrist process is well shown by the large amount of basic steel turned out during the month of October last. In this respect Germany holds the first position with an output of 25,170 tons of steel by eight firms. England stands next with an output of 9,500 tons by one firm. Austria shows an output of 7,700 tons by three firms; Belgium, 1,687 tons by one firm; Russia, 1,270 tons by one firm; and France, 1,240 tons by one firm. We thus have total output of basic steel for the month of October of 46,437 tons, by fifteen firms. When we consider that the process is still but little more than in its infancy, this output may be taken as a satisfactory proof that it is paying, and it is only reasonable to expect in so young a process that with increased practice the anticipations that the present cost will be reduced will be realized.—*Iron.*

BOOK NOTICES.

PUBLICATIONS RECEIVED.

ANNUAL REPORT OF THE ACUSHNET WATER BOARD (NEW BEDFORD.)

MONTHLY WEATHER REVIEW FOR MARCH. Washington: Gov't Printing Office.

BULLETIN OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON. Vols. 4 and 5.

REGULATIONS FOR LIGHTING BRIDGES OVER NAVIGABLE RIVERS. Washington: Light-House Establishment.

CHEMISTRY: INORGANIC AND ORGANIC. By Charles Loudon Bloxam. Fifth Edition. Philadelphia: P. Blakiston & Son.

No work on general chemistry enjoys a higher reputation than Bloxam's. Treating of both organic and inorganic compounds in a single volume it is one of the most convenient books of reference.

The determination to keep up with the progress of the science is sufficiently indicated by the numerous revisions to which the work is subjected.

The date of the completion of this edition is March, 1883. Price, $4.00.

DYNAMO-ELECTRIC MACHINERY. By Silvanus Thompson, M. S. T. E. Science Series No. 66. New York: D. Van Nostrand.

While the dynamo-electric Machine has, within a comparatively short period, achieved a most wonderful development in connection with the rapidly increasing application of electricity to the useful arts, yet the essential principles of its construction and operation appear to be but imperfectly understood, even by otherwise well-informed electricians. The forms in which it has appeared have already become so numerous and varied, and the conditions of its construction and use are so complex in their mutual relations, as not only to defy all general methods of mathematical investigation, but to a considerable extent even satisfactory classification.

The great value of Professor Thompson's work, especially to the student and investigator, is mainly due to his masterly power of analysis, as shown in his method of logically grouping the various dynamos in connection with certain typical forms, each possessing characteristic individual elements which are readily comprehended, and when once fixed in the mind, are instantly recognized wherever found. Hence, any existing machine, or any new machine which may hereafter appear, may be at once referred to its proper type, and the essential features of its organization compared with those of its rivals or predecessors.

The present series of lectures, like all the productions of its eminent author, are characterized not only by a thorough mastery of the subject, both in its theoretical and practical aspects, but by a felicity of classification, and a conciseness and clearness of statement and illustration, which literally leaves nothing to be desired. It forms a work which will be found alike indispensable to the educated electrician, the student, the inventor, and the intelligent mechanic or apprentice.

THE PORTER-ALLEN STEAM ENGINE. By Chas. T. Porter. Philadelphia: Southwark Foundry and Machine Co.

This book is simply descriptive of those parts of the Porter-Allen engine which are characteristic, but these are presented with a clearness of type and diagram that are rarely exhibited in treating of mechanical details.

THE STUDENT'S MECHANICS. By Walter R. Browne, M. A. London: Charles Griffin & Co.

This work differs from the ordinary elementary treatise on mechanics in the exceptional fulness with which the subjects of force and motion are presented.

Definitions and laws are expressed with great care and abundantly illustrated by examples.

The book will prove valuable to students who have completed a rudimentary course in mechanics and have begun the calculus. Price, $1.80.

FRENCH FOREST ORDINANCE OF 1669. Translated by John Croumbie Brown, LL.D. London: Simpkin, Marshall & Co.

Dr. Brown is an indefatigable champion of the conservancy of forests and contributes

more than any other English writer to the cause of forest culture and preservation.

Advocates of the cause in this country will find valuable counsel in the works of this author and will read with interest this sketch of laws of two centuries ago.

REPORT OF THE CHIEF OF ORDNANCE FOR THE YEAR 1882. Washington: Gov't Printing Office.

This report, aside from the uninteresting details which only government officials can regard with any show of interest, contains much that will be read with interest by those who care for the science of gunnery and the theory of projectiles. The plates are numerous and better than usual.

A MANUAL OF PRACTICAL HYGIENE. Sixth Edition. By Edmund A. Parkes, M. D., F. R. S. Philadelphia: P. Blakiston & Son.

More than the usual number of topics is embraced in this voluminous treatise on Hygiene. Water, air, food and habitation are all fully considered.

Aside from its value as a general treatise it affords the best possible guide to sanitary inspectors by supplying suggestions in regard to the seat of troublesome causes of disease.

Altogether it is the most complete and authoritative compendium of sanitary science that we know of. Price, $6.00.

MODERN PERSPECTIVE. By Wm. R. Ware. Boston: James R. Osgood.

Readers of the *American Architect and Building News* know something of the value of Prof. Ware's treatise on Perspective. The present book contains the papers published in the above mentioned journal, together with a large amount of additional matter including many plates.

Nothing seems wanting to make a complete text-book for the student. The plates are large and well executed. The text is in a separate volume. Price, $5.00.

A MANUAL OF MARINE ENGINEERING. By A. E. Seaton. London: Charles Griffin & Co.

This work has been prepared to supply a need of a guide to the application of theoretical principles to the practical construction of marine machinery. It is designed to aid both the engineer and draughtsman in their respective labors.

The treatise is exhaustive: text covering 425 royal octavo pages and is well illustrated. Price, $6 00.

USEFUL RULES AND TABLES. By William John Macquorn Rankine. Sixth Edition. London: Charles Griffin & Co.

This new edition of a well known table-book is enriched by a collection of tables and formulæ relating to electrical engineering. This latter has been compiled by Andrew Jamieson, C. E., F. R. S. E.

Aside from this important addition, which covers about seventy pages, the book is like the previous edition. Price $4 00.

MISCELLANEOUS.

A CHEAP brilliant black can, it is said, be produced on iron and steel by applying with a fine hair brush a mixture of turpentine and sulphur boiled together. We have not tried it.

IN water containing saltpetre and air free from carbonic acid, lead and zinc, in experiments made by Professor A. Wagner, were attacked most violently; tin and Britannia metal a little; copper, brass and German silver not at all. With air and carbonic acid present, zinc and lead were attacked most; copper, German silver and brass were not more acted upon than by distilled water; tin and Britannia metal were acted upon somewhat. None of the metals were dissolved when carbonic acid was absent; when it was present, perceptible quantities were dissolved. In carbonate of soda, and air free from carbonic acid, lead, copper, brass and German silver lost nothing in weight, but zinc, tin and Britannia metal were sensibly affected. Perceptible quantities of tin and Britannia metal were dissolved; none of the other metals entered into solution. It was not possible to pass carbonic acid into the solution, as this would convert the carbonate of soda into bicarbonate of soda.

A TRIAL of an electro-magnetic motor, aërial screw, and bichromate elements constructed by MM. Tissandier for their directing balloon, took place in their aëronautical workshop at Point du Tour, on January 26th, before a large number of electricians and aëronaunts. It was shown that the twenty-four elements, each of which weighs about six kilogrammes, give during almost three hours a current which rotates a screw of 2.85m. diameter, and about five meters of path, with a velocity of 150 turns in a minute. *Nature* says the motive power really developed may be estimated at that of four horses per hour. But in a paper read on the 22d ult. before the Academié des Sciéncés, M. Tissandier said: "The system, with a total weight equal to three men, gives during three hours the work of twelve to fifteen men. The two-vaned propeller—of steel wire and varnished silk—is driven by a small Siemens dynamo—120 turns of the former to 1200 of the latter; the battery being of thirty-four elements mounted in tension, and divided into four series. An element consists of a vulcanite box—four liters capacity—holding ten zinc and eleven carbon plates. Strong bichromate solution is let in or drawn off by raising or lowering a separate vessel connected by a tube with the battery." The weight of all the machinery and elements is a little less than 250 kilogrammes. The real effect on the air can only be found by experiments in the air, but according to measurements taken with a dynamometer of the horizontal tendency to motion, it is about the same as in the experiment tried by Dupuy de Lome. The motive power of Dupuy de Lome having been obtained with eight men working his large screw, whose diameter was nine meters, it may be inferred that the results in the present case will be more advantageous in the

ratio of two and one-half to one. These results are not very powerful when compared with the immense power of aërial currents. But MM. Tissandier have no intention of directing their balloon against st ong winds. Their object is to organize an apparatus with which rational experiments may be made in the air, and they have taken advantage of the most recent improvements of science. If their elongated balloon answer their wishes, a real advance will be registered in the history of aëronautics.

In an article on "Freeboard," from a seaman's point of view, the *Nautical Magazine* says: "Superstructures, no matter how or in what part of the ship erected, cannot add to the safety of the platform as represented by the lowest portion of main-deck, and consequently any reduction on that score is manifestly a reduction of safety. The matter resolves itself into a simple argument. In estimating the freeboard for a flush-decked vessel, the chief desideratum is to have such an amount of clear side as will afford a safe platform on which the crew can go to and fro to work the ship, &c. Now, as vessels are constructed at present, is it not a fact that in loading and putting a ship down in the water, the deck ceases to be a safe platform for human life long before there is any danger of the deck, bulkheads or hatches being stove in? Clearly, then, safety of platform is the point at which we must stop, and the erection of no superstructure either before or abaft the lowest part of that platform can make it any safer."

Mr. J. P. Maginnis, of 10 Victoria-chambers, Westminster, has made two new drawing instruments, which have wide ranges of application, and will no doubt be extensively used. One is what he calls a dead-beat adjustable sectioner, an instrument by the aid of which parallel lines may be ruled at any distance apart, from about 6 or 8 to 200 to the inch for sectioning, shading and cross hatching The other instrument is a universal sector, which is an ingenious though simple instrument. It was specially designed for constructing scales to suit shrunk drawings and lithographs, but it is equally well adapted: (1) To divide a line of any length, up to 5in., into any number of equal parts; (2) to give the proportion, in decimals, of the distance between any two given points, to the distance between any other two given points; (3) to set off or determine angles; (4) to bisect, trisect, or otherwise sub-divide angles; (5) to find the center of a given circle; (6) to reduce or enlarge ordinates of railway or other sections, curves, &c., in any desired proportion.

Some time ago Mr. Herbert McLeod pointed out that the deterioration of ebonite surfaces was due to the combined action of light and air. He now writes to *Nature*: "Some time ago it was remarked to me that our laboratory —an old greenhouse—was too light, and as a result, all our india rubber tubes would rapidly deteriorate. This led me to submit some pieces of ordinary black india-rubber to the same treatment as the ebonite in the former experiments. On October 11, 1879, four pieces of caoutchouc connector of 5 mm. internal diameter were taken, two were placed in test tubes plugged with cotton wool, and the remaining two inclosed in hermetically sealed tubes. One of the sealed tubes, and one of those plugged with cotton wool, were placed in a dark drawer, and the other pair in the laboratory window, with a north aspect, and in such a position that they were not under the influence of direct sunlight in the summer. To-day the specimens were examined. Both the sealed tubes were found to be slightly moist inside, and on opening them an organic odor, like that of an india-rubber shop, was perceived. The caoutchouc which had been exposed to air and light, was covered with a thin brown coating, and on being bent this coating cracked; the end which had been most exposed to the light was rather brittle, and could not be stretched without splitting. The other three specimens were unaltered. All four specimens were slightly acid to test paper, but the quantity of acid was too small to be determined. Mareck (*Chem. News*, xlvii. 25, from *Zeitschr für Anal. Chem.* xxi.) has lately recommended the preservation of caoutchouc tubes, by keeping them in water when not in use. This is, no doubt, efficacious, in consequence of the exclusion of air."

The results of a fourth year's observations of periodic movements of the ground as indicated by spirit levels at Secheron, are given by M. Ph. Plantamour in the *Archives des Sciences*, of December 15th. The curves obtained from the east-west spirit level for the four years are strikingly similar in the manner in which they follow the thermal oscillations of the air. Different years show a notable difference in the epoch of maximum descent of the east side relatively to the minimum of mean temperature, and maximum rise of the same side relatively to the minimum of temperature. One is led, says *Nature*, to consider the maximum and minimum of temperature rather as accidents as regards the epoch at which they occur, and to attribute a preponderant influence to the distribution of mean temperatures during the four months November-February, and the four June-September. Probably, too, the degree of moisture influences largely the rapidity with which the deeper ground layers are affected by exterior temperature. The curve for the north-south level is also very similar to the previous ones: but has this peculiarity, that while the south side follow, in general, from October 1st to the end of September, the oscillations of external temperature—descending in winter and rising in summer—the intermediate variations of temperature have no inverse effect. The cause is at present unknown. Colonel van Orff's observations at Bogenhausen reveal oscillations of the ground similar to those at Secheron, only with greater amplitude south-north, and less east-west. M. Plantamour regrets that, excepting Colonel van Orff and M. d'Abbedie, no one, so far as he knows, has undertaken observations of the kind at any other station. They are easily made, and should yield important results.